The Constructivist Critique of Economic Methodology*

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This paper outlines the main elements of the constructivist, or anti-representationalist, critique of economic methodology. It is argued that, whatever insights may follow from this approach, the arguments that have been put forward do not undermine the case for methodology. Methodology can be defended in ways that do not require the existence of certain foundations for knowledge, and which are not vulnerable to the constructivist critique. It is concluded that methodological inquiry is needed and should not be allowed to wither away.

1. Introduction

The last decade has seen an explosion of interest in the rhetoric of economics with economists beginning to draw upon insights from rhetorical analysis, literary criticism, linguistics and the sociology of science. A recurring theme in much of this literature concerns the inappropriateness of more traditional methodological concerns. Amongst specialists on methodology, methodologies which seek to provide criteria whereby economic ideas can be evaluated (such as Popperian falsificationism, of Lakatos’s methodology of scientific research programs) have become unfashionable. It has been widely argued that knowledge should be seen as socially constructed, and that once we accept this insight it becomes inappropriate to look for general criteria according to which we can judge whether economic theories are true or false. It is argued that an analysis of the way in which meanings are negotiated within the economics community should replace such inquiries.

The economist coming to this literature finds exciting new arguments, and may well have his or her perspective on economics permanently altered. At the same time, however, it is very difficult to know just how far to go with the new arguments. We may, for example, agree that economists’ scientific pretensions may need debunking, and that we have a lot to learn from studying how economists actually persuade each other, but we may nonetheless hesitate to draw the conclusion that such arguments imply the need to reject all methodology. Solow sums up this reaction very well.

‘I don’t see how anything but good can come from studying how trained economists actually go about persuading one another. We will learn something about the strategy and tactics of their arguments. Self-knowledge might help to make the arguments better, or at least honest if they are not so... Nevertheless, I have to report a certain discomfort, a vague itch. It feels like my eclecticism warning me that Klamer and McCloskey are in grave danger of Going Too Far. To be specific, I worry that their version of the occupational disease is to drift into a belief that one mode of argument is as good as another. In this instance I side with Orwell’s pigs: All arguments are equal, but some are more equal than others. For that reason I mistrust McCloskey’s favorite image – borrowed from Rorty – of economics, or science generally, as an ongoing conversation. It seems too permissive’ (Solow, 1988, pp. 32-3).

One might have hoped that by now the issues would have been clarified at least to the extent that there was agreement on precisely where people disagreed – that the half-articulated issues that caused Solow discomfort would have been openly explored. But this is only partially true. Caldwell and Coats (1984) made it clear that discussions of economic methodology had moved beyond positivism long before McCloskey’s article, but they did not tackle the nature of McCloskey’s objections in detail. The exchange between Hausman and McPherson (1988) and
McCloskey (1989) made it clear that the problem concerns where 'standards' are to come from. The exchange between Mäki (1988a,b) and McCloskey (1988) focused attention on 'realism' but with both Mäki and McCloskey both claiming to be realists, yet drawing different conclusions, nothing seemed to be settled. Similarly, the exchanges between McCloskey (ibid.) and Rappaport (1988) and Rosenberg (1988), though they tackled the key issue of epistemology and rhetoric, gave the impression that the underlying reasons for McCloskey's rejection of methodology were not being either isolated or appraised: it was almost as though he was using one set of arguments to attack methodology, whilst his critics were using other arguments to defend it.

One of the merits of more recent contributions to the 'anti-methodology' case is that they focus on a narrower range of issues. The most important such contribution, is that of Weintraub (1989, 1991) because he makes it abundantly clear that the key issue is not rhetoric, realism, scientism, positivism, the impossibility of making predictions, or any of the other arguments (all of them important in their own right) but the implications of the thesis that all knowledge is socially constructed. This is something of which other contributors to the debate are clearly aware – they could hardly fail to be aware of it – but it has not been explored adequately. Why is this? Solow, as an economist, clearly considers it obvious that the constructivist argument is taken too far, and soon turns to other matters (1988, pp. 31-2). Mäki (1988, p. 90) takes the insights to be obtained from rhetoric as the main aspect of McCloskey's and Klamer's arguments, and because he believes he can show that 'it is unnecessary to marry them [the insights obtained from rhetoric] with enthusiasm about the fashionable anti-realism of Richard Rorty and others' he does not explore this 'anti-realist' position in detail. Rosenberg (1988, p. 130) states explicitly that he does not wish to get involved in discussing a fashion that will pass away.

The aim of this paper, therefore, is to address the constructivist critique of methodology directly. In order to keep the task to manageable proportions, it focuses on two recent examples: McCloskey's argument that the study of rhetoric should replace the study of methodology; and Weintraub's claim that the entire project of 'Methodology' is misconceived. It is important to consider McCloskey's arguments because his plea that economists should pay attention to their rhetoric has been so influential. Weintraub's thesis is discussed because of its radical nature and the exceptional clarity with which the argument is expressed. In order to present their arguments, the paper outlines some of the philosophical and literary-theoretic work on which these two economists' arguments draw: Rorty's critique of epistemology, and Fish's argument against any theory of literary criticism. The paper then explores, in section IV, the notion that this critique should be seen as post-modernist critique of modernism in economics. The response to these ideas is presented in two stages. The first stage, section V, is negative: to raise a number of problems concerning the constructivist arguments. The second stage, section VI, is to suggest a number of ways in which traditional methodological concerns can be defended in a way that is not vulnerable to the constructivist critique. The role of the methodologist is considered in section VII, and conclusions are drawn in the final section.

2. From Methodology to Rhetoric

Rorty's Critique of Philosophy

Though Rorty's work has implications for the whole of philosophy, his starting point is a critique of epistemology – a critique which argues not simply that specific theories about the nature of knowledge were wrong, but that the whole project of seeking to provide a theory of knowledge in general, which can be used to pass judgement on specific claims to knowledge, is misconceived. His starting point is the argument that it is philosophy's claim to have access to the nature of knowledge in general – which rests on a particular notion of the mind and of mental processes – that underlies its claim to be able to pass judgement on the claims to knowledge made in other disciplines – whether in science, morality, art or religion.

'Philosophy can be foundational in respect to the rest of culture because culture is the assemblage of claims to knowledge, and philosophy adjudicates such claims. It can do so because it understands the foundations of knowledge, and it finds these foundations in a study of man-as-knower, of the "mental processes" or the "activity of representation" which make knowledge possible' (Rorty, 1980, p. 3).
His aim is to show that such claims are unjustified.

'The aim of this book is to undermine the reader’s confidence in "the mind" as something about which one should have a "philosophical" view, in "knowledge" as something about which there ought to be a "theory" and which has "foundations", and in "philosophy" as it has been conceived since Kant' (Rorty, 1980, p. 7).

Rorty argues that this position rests, crucially, on the metaphor of the mind as a mirror, which reflects nature, sometimes accurately, sometimes less accurately.

'The picture which holds traditional philosophy captive is that of the mind as a great mirror, containing various representations – some accurate, some not – and capable of being studied by pure, nonempirical methods' (Rorty, 1980, p. 12).

Without such a metaphor, he continues, ‘the notion of knowledge as accuracy of representation would not have suggested itself’ (ibid.).

The flaw in this line of argument is, Rorty claims, that it rests on the notion of ‘privileged access’ – the notion that somehow the philosopher has some source of knowledge which is not subject to the problems that beset those whose quest for knowledge he is investigating – for no individual has any privileged, absolute source of knowledge outside itself. It is, he argues, meaningless to speak of knowledge as involving the accurate representation of nature because that presumes we know what nature is ‘really’ like – that we have access to a privileged source of knowledge. Even the concept of the mind as a distinct entity is undermined by the absence of any privileged access to knowledge for, he argues, there is no other way in which it can be conceived: ‘the purportedly metaphysical “problem of consciousness” is no more and no less than the epistemological “problem of privileged access”’ (Rorty, 1980, p. 69). We must, therefore, discard the notion of ‘the mind as the Mirror of Nature’ (Rorty, 1980, p. 170) and along with it the notion that it is useful to search for criteria for appraising the truth of claims to knowledge.

Rorty proposes that we replace the conception of knowledge as accurate representation – involving a confrontation between the mind and nature – with a view of knowledge as ‘a matter of conversation and of social practice’ (Rorty, 1980, p. 171). This involves abandoning the ‘desire to find "foundations" to which one might cling, frameworks beyond which one must not stray, objects which impose themselves, representations which cannot be gainsaid’ (Rorty, 1980, p. 315). To search for such a ‘permanent neutral framework whose “structure” philosophy can display’ is, Rorty argues, to accept the indefensible notion that the objects to be confronted by the mind, or the rules which constrain inquiry, are common to all discourse, or at least to every discourse on a given topic’ (Rorty, 1980, pp. 315-6). In contrast, to view knowledge as the product arising out of a conversation is to accept its dependence on the rules and assumptions that govern that particular conversation. The justification of beliefs (and hence knowledge, if we view knowledge as justified beliefs) becomes a social phenomenon (Rorty, 1980, p. 9), the question of ‘truth’ becoming uninteresting.

‘A pragmatist theory says that truth is not the sort of thing one should expect to have a philosophically interesting theory about’ (Rorty, 1982/1987, p. 26).

The task of philosophy becomes that of keeping the conversation going, not that of finding objective truth: conversation is ‘the ultimate context within which knowledge is to be understood’ (Rorty, 1980, pp. 377, 389). The role of the philosopher is thus that of the therapist, not the judge.

Rorty’s pragmatist position has an important implication for the nature of philosophy, namely that one has to abandon the idea that there are certain perennial problems that constitute the ‘central questions’ in philosophy. To assume that there were such central questions, to which all ‘great philosophers’ must be seen as having offered answers, is to accept the notion that ‘human activity ... takes place within a framework which can be isolated prior to the conclusion of inquiry’ (Rorty, 1980, p. 8) – in other words, to assume privileged access to knowledge.'
case for analysing economists’ rhetoric. This wider argument raises too many issues for it to be possible to deal with them adequately here.8 A major component of his argument, however, is the argument that the whole project of methodology is misconceived, for much the same reasons as Rorty argues that epistemology is impossible. His claim is that any ‘Methodology’ must be based on the assumption that the ‘Methodologist’ has privileged access to knowledge; for in presuming to tell economists how to do their job, the ‘Methodologist’ is talking as though he or she had some privileged access to knowledge. McCloskey portrays the ‘Methodologist’s’ relation to economics in the same terms as Rorty portrays the traditional philosopher’s presumed relation to culture – as that of an external arbiter and judge.

Those who study economic arguments (we can hardly call them methodologists!) should therefore, McCloskey argues, abandon ‘Methodology’ in favor of rhetoric – in favor of analysing the means economists actually use to persuade one another. The purpose of analysing rhetoric is not to appraise, but to help economists understand one another better – to improve the ‘conversation’ amongst economists. Though his stress on rhetoric distinguishes his work from Rorty’s, this role is very similar to the role Rorty sees for philosophers vis a vis the world in general.9

3. Against Methodology

Fish’s Critique of Literary Theory

‘Theory’, in literary criticism, is concerned with the nature and source of meaning in the interpretation of literary texts: is the text or the reader the source of meaning? Fish’s answer is that it is neither of these, meanings being produced by interpretive communities:

‘[I]t is interpretive communities, rather than either the text or the reader, that produce meanings .... Interpretive communities are made up of those who share interpretive strategies not for reading but for writing texts, for constituting their properties’ (Fish, 1980, p. 14).

The claim that the text cannot be the source of meanings rests on the argument that what the reader sees in a text (i.e. the ‘units of sense’ to which the reader responds) are not ‘in the text’ but are ‘always a function of the interpretive model one brings to bear’ on it (Fish, 1980, p. 13). It follows that the text as an entity independent of interpretation and (ideally) responsible for its career drops out and is replaced by texts that are consequences of our interpretive activities. There are still formal patterns, but they do not lie innocently in the world; rather they are themselves constituted by an interpretive act. The facts one points to are still there (in a sense that would not be consoling to an objectivist) but only as the consequence of the interpretive (man-made) model that has called them into being’ (Fish, 1980, p. 13).

We do not read a text and then interpret it according to some strategy: interpretive strategies determine the shape of the text. On the other hand, if texts are not the source of meanings, neither are individuals: interpretive strategies are ‘community property’ which enable and limit the consciousness of individual members of the relevant community. It is wrong to think of individuals as standing apart from the communities to which they belong:

‘since the thoughts an individual can think and the mental operations he can perform have their source in some or other interpretive community, he is as much a product of that community ... as the meanings it enables him to produce’ (Fish, 1980, p. 14).

Thus for Fish, neither the text nor the reader exists apart from the interpretive community. Meaning is thus neither subjective (totally dependent on the individual reader) nor objective (originating in the given text).

‘An interpretive community is not objective because as a bundle of interests, of particular purposes and goals, its perspective is interested rather than neutral; but by the very same reasoning, the meanings and texts produced by an interpretive community are not subjective because they do not proceed from an isolated individual but from a public and conventional point of view’ (Fish, 1980, p. 14).

Stability in interpretations is easily explained without positing stability in the texts: it is a consequence of stability in interpretive communities.

Weintraub’s Critique of Economic ‘Methodology’

Weintraub critique of economic methodology
draws on Fish’s critique of theory in literary criticism. Like McCloskey, Weintraub distinguishes between ‘Methodology’ – a normative enterprise, and ‘methodology’, sometimes referred to as ‘methodological discourse’ – discussions amongst economists about the way they pursue their discipline. He draws an explicit parallel between ‘Methodology’ in economics and ‘theory’ in literary criticism, arguing that some simple substitutions can transform a definition of the latter into a definition of the former.

‘By “theory” we mean a special project in literary criticism: the attempt to govern the interpretation of particular texts by appealing to an account of interpretation in general’ (Knapp and Michaels, 1982, p. 723, quoted in Weintraub, 1989, p. 264).

‘By “Methodology” we mean a special project in economics: the attempt to govern appraisal of particular economic theories by appealing to an account of theorizing in general’ (Weintraub, 1989, p. 264).\(^\text{10}\)

However, where McCloskey argues that replacing concern with ‘Methodology’ with the study of rhetoric can have beneficial effects, Weintraub’s critique is more radical, for he argues that neither ‘Methodology’ nor ‘Rhetoric’ can have any implications for economic practice. His argument, in short, is that ‘Methodology’ is impossible and therefore cannot succeed; and that because it cannot succeed it cannot have consequences. For the same reasons, ‘anti-Methodology’ (as he prefers to term what McCloskey would call ‘Rhetoric’) cannot succeed either (Weintraub, 1989, p. 263). The essential point in his argument is that economic arguments should always be appraised from within economics, using economic arguments, never using arguments ‘in Methodology’ which come from outside economics. Weintraub echoes Rorty’s arguments when he states that ‘there is no position totally apart from the doing of economics which can inform the consideration of the doing of economics’ (Weintraub, 1989, p. 272). In support of this, he quotes Fish’s view of theory in literary criticism:

‘Theory cannot guide practice because its rules and procedures are no more than generalizations from practice’s history (and from only a small piece of that history), and theory cannot reform practice because, rather than neutralizing interest, it begins and ends in interest and raises the imperatives of interest – of some local, particular, partisan project – to the status of universals’ (Fish, 1985, pp. 438-9; quoted in Weintraub, 1989, p. 272).

Before considering these arguments in more detail, it is worth examining the way Weintraub works out his argument and some of the examples he uses. After defining ‘Methodology’\(^\text{11}\) he provides, in a single paragraph, an account of ‘Science’ since the time of the Greeks in which the culmination of the history of philosophical inquiry is claimed to be Logical Positivism, according to which science is clearly demarcated from non-science, and in which ‘the philosophy of science itself defines the epistemological enterprise, for only science provides us with True Knowledge, or warranted belief’ (Weintraub, 1989, p. 265). This program, he continues, implies two things: if economists think of themselves as ‘Truth-seeking’, they should think of themselves as scientists; and they should imitate ‘in both fact and perception’ the activities of science (Weintraub, 1989, p. 265).

By presenting it as the subdiscipline of economics comprising those economists who share the perspective of ‘the Philosophy of Science’, he defines ‘Methodology’ in such a way that it is the outgrowth of a foundationalist epistemology. Such a perspective is, he claims, shared by most economists. This has the effect of virtually equating ‘Methodology’ with Logical Positivism, a position that most philosophers have long since abandoned.

‘Most economists, believing that claims about economic knowledge have no less epistemic, or truth, status than claims about physical, and chemical, and biological phenomena, are positivists in the sense that they believe there is a particular framework in which science’s knowledge claims may be justified’ (Weintraub, 1989, p. 266).\(^\text{12}\)

Such foundationalist thinking, which is the basis of ‘Methodology’, leads to economists’ believing that ‘the Methodologist’s arguments can have an effect on practice, can lead to a better way of doing economics’ (ibid.). In Fish’s words,

‘[T]hose who believe in the consequences of foundationalist theory are possessed by a hope – let us call it “theory hope” – the hope that our claims
to knowledge can be "justified on the basis of some objective method of assessing such claims" rather than on the basis of the individual beliefs that have been derived from the accidents of education and experience" (Fish, 1982, p. 439; quoted in Weintraub, 1989, p. 266).

As examples of Methodologists he cites Terence Hutchison, Lawrence Boland and Mark Blaug.

After making this general point about the Positivist nature of most economists’ thinking, Weintraub proceeds to examine the way in which 'Methodological' arguments are used in practice. He cites several examples. (1) Post Keynesians (such as Nicholas Kaldor, Paul Davidson and D. Vickers) oppose general equilibrium theory on the grounds that certain of the assumptions on which it rests are demonstrably irrelevant. Weintraub's interpretation is that, 'These writers acted as Methodologists because their critical position is developed from a perspective "outside" economics, in this case from the epistemological theory of probabilistic knowledge' (Weintraub 1989, p. 267).

(2) Marxians who criticise neoclassical economics for its neglect of class, power, exploitation and social relations do so on the basis of categories derived from 'Hegelian historiographic tradition', something 'separate from either Marxian economic analysis of neoclassical analysis' (ibid.). (3) The Austrian argument about methodological individualism is Methodological. (4) Paul Samuelson appealed to 'Operationism', taken from the physicist Percy Bridgman. (5) Milton Friedman appealed to his 'methodology of positive economics'. (6) Keynes attacked Tinbergen on grounds that 'since the structures of the world change, time series analysis cannot possibly explain macroeconomic phenomena' (Weintraub, 1989, p. 268). (7) The 'Methodological' argument that 'the world is in disequilibrium, not equilibrium' is used to attack Lucas's view of unemployment.

In an italicized sentence, Weintraub comments that 'All these examples are associated with attempts to criticize existing economics as it is done by real economists, and are thus methodological in nature; but additionally all the particular criticisms are Methodological as they depend on a perspective outside economic analysis to criticize particular work in economics' (ibid.).

His argument rests on the assumption that there is a particular discourse community (or collection of related discourse communities) called economics, for his arguments depend on being able to separate out those criticisms of particular economic theories that originate within economics from those that emanate from without.

4. Constructivism as Post-Modernity

McCloskey's Critique of Modernism in Economics

In attacking 'Methodology' McCloskey chooses as his target a point of view he terms 'modernism', arguing that it describes the 'official rhetoric' of economics. The leading idea of modernism is, he argues, 'that all sure knowledge is modelled on the early 20th century's understanding of certain pieces of 19th century physics' (McCloskey, 1983, p. 484). He characterises (caricatures?) modernism in terms of eleven precepts.

'(1) Prediction (and control) is the goal of science. (2) Only the observable implications (or predictions) matter to its truth. (3) Observability entails objective, reproducible experiments. (4) If (and only if) an experimental implication of a theory proves false is the theory proved false. (5) Objectivity is to be treasured; subjective "observation" (introspection) is not scientific knowledge. (6) Kelvin's Dictum: "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind." (7) Introspection, metaphysical belief, aesthetics, and the like may well figure in the discovery of an hypothesis but cannot figure in its justification. (8) It is the business of methodology to demarcate scientific reasoning from non-scientific, positive from normative. (9) A scientific explanation brings an event under a covering law. (10) Scientists, for instance economic scientists, have nothing to say as scientists about values, whether of morality or art. (11) Hume's Fork: "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume – of divinity or school metaphysics, for instance – let us ask, Does it contain any abstract reasoning
Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames, for it can contain nothing but sophistry and illusion" (McCloskey, 1983, pp. 484-5).

His argument is, somewhat controversially(?), that a large majority of economists believe all eleven propositions, whereas few philosophers believe even half of them, with a substantial, respectable and growing minority of philosophers believing none of them.

The main argument arrayed against modernism is simply the case, discussed in sections 2 and 3 above, against the very project of epistemology (or, in literary criticism, theory; or, in economics, Methodology) of which this is a special case. McCloskey thus argues that modernism is impossible, there being no knowledge that is free from doubt; and that any method is arrogant and presumptuous' (McCloskey, 1983, p. 490). To these, however, are added some arguments that are specific to modernism: falsificationism is not cогent, for it is always possible to find reasons for disbelieving any alleged falsification; and prediction is impossible in economics.

'It [a serious prediction] must answer again the American Question: If you're so smart why aren't you rich? At the margin (because that is where economics works) and on average (because some people are lucky) the industry of making economic predictions, which includes universities, earns only normal returns' (McCloskey, 1983, p. 488).

Finally, he argues that other sciences do not follow modernist methodology.

# Modernism and Post-modernism

McCloskey's definition was selected as the starting point for this discussion of modernism because it is the earliest and by far the most prominent of all the papers considered here. His definition is, however, limited in that it comes very close to identifying modernism with positivism. In the wider discussion of modernism in other disciplines, this identification is clearly not made, important though positivism is as an example of modernism. A more broadly-based definition of modernism, derived from the characteristics attributed to it by Klammer (1987) and Dow (forthcoming) is the following:

(1) A break with the past involving: (a) the search for universal theories, independent of history and context; (b) a commitment to the new and to the idea of progress, with each generation using all that is useful from the past.

(2) Formalism, with a preference for axiomatic, reductionist, dualist reasoning and the use of mathematics.

(3) Compartmentalization: (a) between public and private spheres; (b) between positive and normative; and (c) between disciplines. Disciplines turn inwards, this involving (i) a split between amateur and professional; (ii) jargon; and (iii) a tendency towards self-referential discourse.

A more concise and more systematic definition of modernism and post-modernism is provided by Wendt (1990) who argues that modernism is centered on the metaphor of the machine, which gives it three essential characteristics: 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, he claims, inessential (Wendt, 1990, p. 49).

Such definitions of modernism, used by both Klammer and Dow to interpret 20th century trends in economics, have the attraction of explicitly explaining the alleged character of economics with reference to a wider cultural movement. Klammer has argued that modernism spread to economics at the end of the 1930s, with the rapid growth then and during the late 1940s, of mathematical formalism (c.f. Mirowski, 1991) associated particularly with Paul Samuelson. Dow dates the rise of modernism to the same period, associating it with 'the successful translation of Keynes's theory into orthodox terms' (Dow, forthcoming, p. 150). The assumption of rational, maximizing behavior permitted the increasing use of formal, mathematical argument to derive theories that could be claimed to be of universal relevance.

If this were all there were to it, this discussion of modernism could have been subsumed into the earlier discussion of McCloskey's critique of prescriptive methodology. Arguments about modernism have, however, as they have in other disciplines, been taken further. The term post-modernism is used as a label for developments which go beyond modernism, recognising the impossibility of achieving certain knowledge, and which have moved away from the search for unifying
theories, moving instead towards a more pluralist approach, with different theories and methods being seen as appropriate for different problems. Wendt, for example, characterizes post-modernism as an anti-modernist program calling for discourse studies, which is characterized by, amongst other features: 'multiple knowledges or discourses, with incommensurability and persistent or irreducible differences among them'; 'the discursive production of subjects, which decenters the individual'; irrationality, contradiction, the arbitrary, play and surprise; knowledge as unstable and ever changing; the unmeasurable and guesswork, rejecting content and system; and deconstructing modernism and modernist discourse (involving the destruction of economic theory and reasoning) (Wendt. 1990, pp. 53-4).

Dow, on the other hand, adopts a more positive view of post-modernism, defining it as involving 'openness in a range of methods, disciplines and influences, accepting that no theory can be regarded as true in any absolute sense' (Dow, forthcoming, p. 157). The problem with openness, however, is that choices have to be made, which means that more definite criteria are required. The solution to this dilemma, Dow argues, is to be found in schools of thought, a school of thought being 'a Kuhnian paradigm, adopted by an academic community with shared views of the world as a whole, and as to the preferred techniques to analyse it' (Dow, forthcoming, p. 158). Her contention is that criteria as well as theories must be up for discussion, but that disagreements can go only so far: 'ultimately disagreements may have to stop at an underlying disagreement on vision' (Dow, forthcoming, p. 159). Thus within schools of thought Kuhnian 'normal science' can carry on, even though a pluralist stance is being taken towards other schools, none claiming to offer absolute truth. Such an approach, through encouraging on the one hand openness and tolerance, and on the other hand structured theorizing would, she claims, be beneficial to economics.19

5. Criticisms of the Constructivist/Post-Modernist Case

The Case to be Answered

Sections 2 to 4 have presented a range of critiques of 'traditional' methodology. Central to all of them, however, is the claim that there are no uninterpreted givens. In Rorty's critique of epistemology this is very clearly the main point. The argument is given an important twist in Fish's arguments about interpretive communities, but in essence it is the same. Introducing the terminology of modernism relates these ideas to wider cultural developments and to discussions in other disciplines, but seems to add nothing substantial: shorn of the claim that there are no secure foundations for knowledge, modernism has no normative content.

To defend methodology against this charge it is necessary to do at least one of three things: to show that the constructivist, or post-modernist position is unsustainable; to show that, contrary to Rorty's claim, there are things that we can legitimately take as given; or to show that methodology can be defended even if Rorty's arguments against the possibility of providing a foundation for knowledge are accepted. In this section we point out some weaknesses in the constructivist case; in section 4 we explore ways of defending methodology. In the course of these explorations we shall return to ideas raised by Fish, Dow and others.21,22

Discourse Communities, Paradigms and Schools of Thought as the Basis for Knowledge

Many of the arguments discussed above make use of the notion that knowledge is specific to particular communities: Rorty refers to epistemic communities; Fish and Weintraub to interpretive communities; Dow to paradigms and schools of thought. Insofar as these entities are intended descriptively they are something to which no-one can take exception. Clearly, there do exist different communities, sharing particular views and beliefs. Problems arise, however, when the attempt is made to justify knowledge claims with reference to such communities.23

The first problem is that of how membership of the relevant discourse community is to be determined.24 As Munz puts it, 'If knowledge is to be regarded as the prevailing state of mind in a convivial community, as Rorty has it, the real crux of the matter then is the question as to who is included and who is excluded in these communities' (1984, p. 211).

This question is vital because the definition of the community determines knowledge. If we accept Rorty's claim (1980, p. 38) that this is a matter of decision, then we simply regress one stage, for we have to ask what the criteria are. This, it can be argued, is a moral issue as much as a sociological one, for decisions as to what
counts and does not count as knowledge cannot be taken in a vacuum. Without such a decision criterion, Rorty’s claim to have avoided ‘relativism’ seems vacuous – given sufficient diversity of beliefs it is, surely, possible to define a discourse community such that knowledge is whatever one wishes it to be.

The answer Fish and Weintraub would, presumably, give is that we are not free to define discourse communities as we like.\textsuperscript{25} There are, in any academic community, recognisable discourse communities, known to (or knowable by) workers in the relevant areas, and which can, in retrospect, be recognised by intellectual historians. This does not, however, solve the problem. It is not possible to take such communities as ‘given’, obviously recognisable, for this goes against the premise that there are no uninterpreted givens. If we simply say that ‘qualified’ people can agree on who these communities comprise, then we are left with the question of who is qualified to make the decision, and so on in an infinite regress. One coherent way out of the problem is to specify that the community must be such that its membership is agreed upon by all its members. This, however, means simply that a self-selected group is deciding what is, for it, knowledge. In some societies\textsuperscript{26} this may present no problems, but in the democratic, pluralistic societies in the 1990s this hardly seems an acceptable solution to the problem.

Suppose that defining the relevant communities were unproblematic. Would the case for grounding knowledge in such communities then be defensible? There are several reasons why it would not be. One is that knowledge would depend on power, an issue that was raised long ago in the context of Kuhn’s paradigms. This may be a fact of life, but there seems no place for such a concept in an evaluative methodology.

A second reason concerns relationships between communities, and the right of outsiders to raise questions concerning the claims to knowledge made by a specific community. Consider, for example, Dow’s arguments (outlined in section 4 above) concerning ‘schools of thought’. Insofar as she is arguing that disagreements between members of different schools of thought will sometimes stem from methodological differences, with the result that debate will get nowhere until these methodological differences are addressed, her position is unexceptionable.\textsuperscript{27}

If, however, we try to read a normative methodological position into Dow’s arguments about schools of thought, we find that the outcome depends critically on what we mean by ‘openness and tolerance’. If we interpret this as meaning what McCloskey refers to as Habermas’s ‘Sprachethik’, then there are no problems in accepting it. We might even agree, though this is going further, that institutional power should not be used to make life difficult for minority schools of thought. But if this is all that is meant by ‘openness and tolerance’, there is nothing special about schools of thought, for there is no suggestion that such standards of behavior should not hold within schools of thought. The notion of schools of thought would thus appear to have methodological implications only if it is argued that schools of thought should be exempt from certain types of criticism from members of other schools: that economists should accept that the visions on which they base their theories differ, and should not judge other schools’ theories against the standards according to which they judge their own. This argument is vulnerable to the question of how schools of thought are to be defined: What is to prevent a particular group (even a group of one?) simply asserting independence and demanding to be appraised as a new school rather than as members of an existing one?

As an example, consider the exchange between Weintraub and Dow in the Journal of Post Keynesian Economics in 1982, in which Weintraub demands that Dow demonstrate that Post Keynesian economics is progressive in the sense of Lakatos. Dow’s ‘openness and tolerance’, if taken as having normative implications, would presumably rule such challenges – with the effect of forcing debate to take place on the terms favored by Post Keynesians. As another example, consider the Kaldor-Hahn debate cited below. Do Dow’s criteria mean that there is good reason why Kaldor should not challenge Hahn on the basis that the assumptions of general equilibrium theory were unrealistic? It may well be that Kaldor failed to make his point in an effective manner, but that is another matter.\textsuperscript{28}

None of this is to say that the notions of interpretive communities or schools of thought may not be an invaluable tool for analysing the history of economic thought. Weintraub (1991) has used the notion with brilliant effect in analysing the changing understanding of economic dynamics. Dow (1985) has shown how
schools of thought can be used to analyse different attitudes towards macroeconomic methodology. This, however, takes us into historiographical issues.

Is Economics like Literature?  

The notion that economics should be viewed from the perspective not of natural sciences, but of literature is a major contribution of the work being discussed here. It is, however, important not to adopt this new perspective uncritically. Perhaps the best way to clarify the issues involved without falling into any epistemological trap is to focus on the term 'text' as it is used in Fish's work. It could be argued that in much of Fish's writing the term 'text' is being used metaphorically. It then becomes appropriate to ask about the appropriateness of the metaphor when applied to economics.

If we apply the metaphor to economics, what are the 'texts' in economics discourse? In Fish's writings there is no suggestion that different texts need to be treated as being different in kind from each other; indeed, given his subject-matter, there is no reason why there should be. Yet in economics presumably the 'texts' include not only written documents (not all produced in the same way) but also statistics (produced in ways that may be radically different from the ways in which literary documents are produced) and other forms of information. One does not have to succumb to McCloskey's fifth and sixth modernist precepts to ask whether all these 'texts' should be considered on a par with each other. Without wishing to suggest that statistics are anything other than 'constructed', the relationship between different types of 'text' would seem to be an issue that needs confronting in a way that is not necessary in literary theory.

6. The Case for Methodology

A 'Common Sense' Response

The claim underlying the arguments against methodology discussed in this paper is the claim that we cannot know the truth about anything. Though the proponents of these arguments would deny this, claiming that the discourse communities in which people work impose constraints, it can be argued that the outcome of accepting such arguments is a form of conventionalism which involves a position very close to pure subjectivism, in which it is not possible to provide any empirical foundation for knowledge claims. Although it may not be as satisfactory a response as we would like, it is quite coherent to argue that although we cannot refute these claims, they can be ignored. Consider the following argument about scepticism in philosophy.

How important is it to defeat scepticism? How central is it to the tasks of epistemology? There is an attitude towards epistemological issues that can be expressed thus: Although we cannot answer the sceptical arguments, we are unconvinced by them. No one genuinely doubts his beliefs about his surroundings on the grounds that he cannot show that he is not a brain suspended in a vat of nutrients, sustained and manipulated by a brilliant scientist. Indeed, the more convinced we are that the arguments cannot be met head on, the more they look like pointless philosophical games. Consequently, let us leave them behind, and, making the best use we can of our knowledge of the history of science and the psychology of cognition, construct a plausible, scientifically informed, account of how we know as much as (we all agree) we do know.

This view exploits the fact that our puzzlement by sceptical arguments is apparently "insulated" from our first order practice of conducting inquiries and forming beliefs. Since they have no impact on this practice, we can ignore these arguments when we search for a philosophical understanding of our success in obtaining knowledge of reality (Hookway, 1990, p. 130).

The challenge posed by scepticism to epistemology is not quite the same as the challenged posed by constructivist arguments to economic methodology, but the parallels are sufficiently close that we can respond in a similar way. Whilst it is certainly the case that knowledge is constructed, and that much of our knowledge of economic events is conditioned by what we take for granted as members of particular discourse communities, it is going too far to argue that there is no such thing as empirical evidence. The constructivist arguments end up treating all knowledge as similar in kind, whereas in practice different knowledge claims are made with very different degrees of certainty. Economic knowledge comprises statements which differ markedly as regards the certainty with which they are held. Though there may not
be many such statements, and though they may need to be stated with great care and with appropriate qualifications, we could ignore the constructivist arguments and take the existence of empirical evidence for granted. We could then use our knowledge of contemporary economics and the history of economic thought, together with such ideas from philosophy or any other relevant discipline, to explore the nature of economic knowledge and to make such generalizations as we can concerning the way in which economic knowledge progresses. Though the results of such inquiries will always remain, to a greater or lesser extent, conjectural, there is no reason in principle why they should not be used as the basis for methodological prescriptions. Such prescriptions will, inevitably, be only as strong as the arguments on which they are based, but that is no reason why they should not be made and debated.

A Realist Response

A similar response to the challenge posed by constructivism is implied by the realist methodology advocated by Lawson (1987) and Mäki (1989, 1990). Realism, according to Lawson, asserts that

'there is a material and social world that exists independently of any individual consciousness and which is knowable by consciousness — true theories of real entities can be obtained' (Lawson, 1989, p. 61; c.f. 1987, p. 951, n. 1).

In addition, there is the very important methodological prescription 'that such knowledge, or true theories, should be pursued' (ibid).

What this has in common with the 'common sense' position is that it bypasses the main epistemological questions discussed above, for our ability to know something about the real world is assumed. Lest this sound an unduly naive position, it is worth making two remarks. The first is that it does not imply any fixed set of appraisal criteria. To quote Mäki,

'the standards of appraising economic theories do not have to be accepted by realism as fixed: they can be regarded as conjectural and evolving through competition and learning. This means that nobody may pretend to hold the final, infallible set of criteria of acceptance. ... Only little by little can we hope to learn which criteria serve best our purposes, including the major goal of attaining true, explanatory theories' (Mäki, 1989, p. 196).

The second remark is that it is compatible with the view that at present we understand too little about the nature and structure of economic theories for it to be possible to answer the types of methodological questions that have traditionally been posed (some of which are the subject of this paper). In other words, the argument is that in order to explain how it is that economists can know anything about their subject, it is necessary first to understand more about it. A realist approach, Mäki would argue, provides a coherent perspective from which this task can be tackled.

Taking this argument further, we might argue that, paradoxical as it may sound, the use of constructivist ideas as an anti-methodological argument is a symptom of what Mäki has called 'the Popperian dominance' (Mäki, 1990, pp. 79-80). By this he does not mean that the Popperian view of good science has been dominant, or that most methodologists have held Popperian ideas. His point is that,

'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. In other words, a limited slice of the whole of Popperian philosophy has dominated the major themes of interest in economic methodology' (Mäki, 1990, p. 79).

If it were not for this concern with a limited range of methodological issues, to the exclusion of others, the significance of the constructivist contribution might well have been perceived rather differently.

Economic Knowledge as Based on the Practices of Economists

A response which is based on the acceptance of many of the constructivist claims discussed above, but which differs from them in arguing that we can legitimately speak of an empirical foundation for knowledge, is that provided by Bazerman (1988, particularly chapter 11). He argues that a scientific community is defined not simply by a set of common concepts and a common language, but also by a common set of practices. Becoming a member of a scientific community involves learning not only the community's language, but also the ways in which, for example, experiments are undertaken and data are processed. It follows that although scientific theories are the result of negotiation
within a scientific (discourse) community, they are subject to 'empirical' constraints. ‘Thus within the negotiation of meanings that turn individual proposals into intersubjective realities, we find ambient nature passively constraining possible meanings through the active experience that is inseparable from the language use. Claims that may appear crisp and certain to their proposers will only be fuzzy intersubjective speculations until they settle into a regularized use within repeated activities, and these activities will only be repeatable if they are consonant with some regularity in the appearances or operations of the natural world’ (Bazerman, 1988, pp. 311-12).

He thus distinguishes two types of constraint affecting the production of knowledge: active constraints comprise ‘the elements of the thought style of the thought collective’; whilst passive constraints are those imposed by natural phenomena.

‘Natural phenomena passively constrain the kinds of formulations you can make in the sense that once you begin formulating statements in whatever style of your thought collective, certain behaviors or features of nature will limit what you can properly say. Once you have established, for example, a procedure for identifying the hardness of rocks and have developed a taxonomy of rock types, which rocks are labelled as harder than others is no longer a matter of cultural discretion. Thus, formulating practices are constrained by the activities that bring the language into active contact with nature’ (Bazerman, 1988, pp. 312-13).

Though expressed in language that relates to natural sciences, a similar case could be made for economics. The data with which economists deal are very different from those dealt with by, say, chemists, but like chemical data they are the outcome of a certain set of practices. Given these practices, the world constrains the data that can be produced. In this sense, therefore, it is possible to talk of an empirical base for knowledge. The nature of this empirical base can be explored – which is methodology.33

A Popperian Response

Rorty’s argument is that knowledge needs to be justified; and that since such justification cannot be provided by the metaphor of a mirror, it has to be sought in the consensus of a particular community. Rorty is, in other words, committed to the notion that knowledge must be justified, and he merely seeks to replace one form of justification with another. In view of the importance of this point it is worth quoting Munz.

‘He [Rorty] thinks all knowledge is only worthy of that name if it can be ‘justified’. He has shown over and over again that it cannot be justified as representational. But since it has to be justified, Rorty sees only one alternative. If knowing does not have an essence (Rorty, 1989, p. 389) then we have the “right to believe” what ‘current standards’ recommend. If words are not transparent to the real (ibid. p. 368) then we must “deconstruct” them and accept that they acquire privileges from the men who use them. In short, Rorty gives us a choice between justificationism and relativism which latter amounts to holding that proposition to be justified which has the consensus of a given community. So the real choice, according to Rorty, is between straight justificationism and relative justificationism. Rorty never considers the possibility that there is a genuine third possibility – the possibility that we consider as knowledge a proposition which is not justified, neither by the fact that it ‘represents’ nor by the fact that it has found the acclaim of a speech community’ (Munz, 1984, pp. 207-8).

Whether or not it is vulnerable to other criticisms (see, for example, Caldwell, 1991) an alternative to both the positions considered by Rorty is a Popperian view, according to which all knowledge is hypothetical. For Popper, as much as for Rorty or Fish, there are no givens: ‘[T]he experiences which are “given to me” such as the experience of seeing something (or, with Descartes, the experience of doubting) seem to offer themselves as the natural starting points. Subjectivists uncritically assumed that upon the basis of these “data” the edifice of knowledge – scientific knowledge – can be erected. But this assumption is incorrect. Nothing can be built on these “data”, even if we assume that they themselves exist. But they do not exist:
there are no uninterpreted ‘data’; there is nothing simply ‘given’ to us uninterpreted; nothing to be taken as a basis. All our knowledge is interpretation in the light of our expectations, our theories, and is therefore hypothetical in some way or other’ (Popper, 1983, p. 102).

These arguments apply not simply to individuals’ experiences (and subjectivism) but equally well to communities’ shared experiences (and theories which see knowledge as pertaining to discourse communities).

The fact that Rorty’s arguments (and the comparable arguments of Fish) are effective only against ‘foundationalist’ epistemologies could explain why, despite his claim to be showing the futility of all ‘Methodology’, McCloskey’s and Weintraub’s explicit arguments take positivism as their target.44 Popperian falsificationism is important not because it is claimed to provide a ‘recipe’ for scientific success (more on this later) but because it provides an alternative to foundationalist epistemology.35,36

7. The Role of the Methodologist

McCloskey (echoing Feyerabend as much as Rorty) and Weintraub are united in seeing methodology (they would call it Methodology) as a foolish enterprise.

‘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. 20).

‘There is no instant rationality, no test we can apply which gives us a precise judgement about the worth of a particular bit of work in economics. ... Kaldor appeared foolish in his exchange with Hahn about the significance of general equilibrium theory: Hahn knew about the theory, and knew about the community, while Kaldor instead had Methodological knowledge that no theory with certain characteristics was worth considering. ... Kaldor’s revealed preferences to the contrary notwithstanding, there is no shortcut to understanding Economics through Methodology’ (Weintraub, 1989, p. 277).

But is it?
The view that methodology is a foolish enterprise rests on the presumption that methodologists are people who presume to know something special about knowledge, to have access to privileged information which is not accessible to ‘ordinary’ economists who do not study philosophy. This, however, is a caricature which collapses the moment it is examined.

Consider the following argument (where I have taken the liberty of describing the study of equality and wealth as economics, rather than politics, and those who undertake this activity as economists!)

‘There is indeed something to be known about knowledge, just as there is something to be known about equality and wealth. There are many different opinions held by different people and all people interested in knowledge or [economics] must pay attention to these opinions and weigh them and evaluate them. Then there are some people who do not engage much in politics or in science themselves but who prefer to specialize in the question as to what we can know or how we can promote or, for that matter, prevent, social justice. Whether we call these specialists philosophers and [economists] or not does not really matter. The only thing that matters is the acceptance of the fact that there is something to be known about all these things; that it is better to know something about them than not to know anything about them; and finally, that practising scientists as well as ordinary citizens must sooner or later face up to the question as to what is known about knowledge or about strategies for promoting equality’ (Munz, 1984, pp. 202-3).

Specialists, including specialists in epistemology or economic methodology are simply those who choose to specialize in these questions. Einstein and Samuelson have reflected on the nature of knowledge, but we do not call them philosophers because they did not choose to specialize in this area.

It may be that methodologists have put forward their arguments, derived from philosophy, with great confidence, and as though they were logically compelling. But so too have many economists. Why should the method of conjecture and refutation not be permissible in methodology as well as in economics?

Having disposed of the argument that
methodologists need claim any privileged position, what of the allegation that they seek to impose ideas from another discipline (philosophy)? The first point to make is that the argument rests on a particular definition of interdisciplinary boundaries, but it is not clear that these are of any significance. Different discourse communities may in practice exist, but there is no reason, a priori, why ideas from one community may not be used in another. The second point concerns generalization. In any discipline there is always the question of how far it is appropriate to generalize, but we normally regard this as something that needs to be settled by considering the evidence. A similar case can be made here: we need to look in detail at the nature of economic knowledge to see how far concepts and issues that arise when we consider 'knowledge in general' are useful. There is no reason not to ask the question. Rorty would argue that by showing the inadequacy of the metaphor of mind as mirror he has shown that there is nothing of interest to be said about knowledge in general. The arguments presented above show that even if we accept Rorty's view of this metaphor, the conclusion that there is nothing of interest to say about methodology does not necessarily follow.

The final point that needs making about the role of the methodologist is that critics of 'Methodology' appear to equate it with simplistic recipes for success in economics. In other words, that claiming to understand something about the nature of economic knowledge is the same as claiming to know how to create new economic knowledge. Economists who claim to understand market behavior in terms of profit-maximizing models, but who are prepared readily to admit that they cannot advise firms how to maximize profits ought not to make such a mistake.37

To illustrate these points, let us return to Weintraub's assessment of the controversy between Hahn and Kaldor. It is certainly plausible that, at least in the minds of most general equilibrium theorists, Hahn won the argument. Kaldor was objecting to general equilibrium theory on the grounds that its assumptions were unrealistic and he failed to appreciate that the methodological arguments (concerning the status and nature of general equilibrium theory) that Hahn would use to defend it would, as far as many economists were concerned, simply render his objections irrelevant. It could thus be argued that Hahn had a better understanding of the community within which general equilibrium theory was acceptable. Kaldor was an outsider to this community. In addition, he was objecting to general equilibrium theory on methodological grounds. It is very misleading to conclude from this, however, that Kaldor's argument failed because it was 'Methodological'.38 Just as there is a community in which Hahn is seen to have won, there are other communities of economists (not just 'post Keynesians') in which Kaldor's objections are being seen as decisive. Weintraub's judgement on the foolishness of Kaldor's challenge follows only if the judgement of the community of general equilibrium theorists is taken as correct, and that outsiders are not qualified to pass judgement on insiders' theories: in other words, if the status quo is accepted.

The lesson to be drawn from this debate is not that 'methodological' objections to a theory are foolish, but that if we are to have a constructive debate over the merits of an economic theory we need to state our objectives and the criteria we use to decide whether theories are helping us achieve those objectives. In other words, we need to discuss methodology.

The conclusion to be drawn from this is that methodology as traditionally understood, including methodology that draws on philosophy, even epistemology, is nothing other than, to use Weintraub's term, 'methodological discourse' which does not confine its horizons to those of a particular, arbitrarily defined discourse community that we call economics.39 To term such activity 'Methodology' and to argue that it should wither away so that a blinkered form of 'methodology' can thrive would seem to be a wilfully misleading use of terminology. Had more precise terminology been employed, for example attacking not 'Methodology' or even 'prescriptive methodology', but rather, say, 'dogmatic, foundationalist methodology', the nature of the argument would have been much clearer. It would also have made it clearer that it applied to only a small part of the methodological literature.40,41

8. Conclusions

This paper has summarized some of the main arguments put forward as to why the discipline of economic methodology should be allowed to wither away. In order to present these arguments fairly, sections 2 to 4 were devoted to presenting these arguments at some length and in their own terms. It was suggested that these arguments were centered on the notion that we have no
knowledge that is not the result of interpretation; and on Rorty's criticism of the view that knowledge involves accurate representation. It was then argued that these objections to methodology were not as decisive as their proponents claim: that there were several ways in which a case for methodology could be made and that there were serious problems involved in trying to use the notion of knowledge as grounded in discourse communities as the basis for normative statements concerning the construction of economic knowledge. Finally, the argument that in seeking to specialize in methodology, and in taking ideas from philosophy, one is necessarily claiming a privileged access to knowledge was shown to be incorrect. It is thus reasonable to conclude that the case for abandoning methodology is far weaker than its proponents claim. Some excessively dogmatic methodological claims may need to be reconsidered, but methodology as a discipline with roots in both philosophy and economics should not be allowed to wither away.42

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Notes
1. A number of terms could be used as labels for this position. In recent work Rorty uses the term 'anti-representationalist', and 'anti-foundingationalist' is clearly another that could be used. Whilst either of of these terms would be satisfactory, however, the word 'constructivist' has the advantage (brevity apart) of emphasizing the positive side of the argument. A popular term is 'postmodernist' but, as is argued below, this term is more ambiguous, and in any case has the disadvantage of saying nothing about the positive content of the ideas expressed.
2. It may be that constructivist or anti-representationalist positions have been so widely criticised in philosophy, that it is not felt necessary to rehearse the arguments against them. This, however, does not appear to be the case in the literature on economic methodology.
3. There is now a growing literature in which such themes are discussed in relation to economics, involving philosophy, literary criticism, discourse analysis and linguistics. See, for example, Samuels (1990), Lavoie (1991) and Henderson, Dudley-Evans and Backhouse (1992).
4. It is not intended to imply that arguments about the social construction of knowledge are original to any of these authors. Though each of these four authors has added something of his own, many of the ideas have a long pedigree. Within economics, institutionalists, Marxists and others have often used such arguments.
5. This section is not a comprehensive discussion of postmodernism in economics. It is concerned simply with placing the arguments discussed earlier in the paper in a slightly broader context.
6. Though they did not elaborate on the point, the existence of a variety of ways of defending methodology was pointed out by Coats and Caldwell (1984).
7. Accepting this claim has enormous implications for the history of philosophy. The relevance of this for the history of economic thought is explored in Backhouse (1992).
8. Some of McCloskey's wider arguments concerning methodology and rhetoric are discussed in Backhouse (1993).
9. Caldwell (1990) claims McCloskey has both a weak and a strong version of his thesis. The interpretation presented here corresponds to the strong (and dominant) version.
10. Note that although Weintraub states that 'appraisal' is being substituted for 'interpretation', he substitutes 'appraisal' on the first occasion, and 'theorizing' on the second.
11. Given the house-style of the Scandinavian Journal of Economics, which is to capitalize the major words in headings, we do not know whether the heading is intended to read 'Methodology' or 'methodology', a major problem with this terminology.
12. It is worth nothing that this is a somewhat unusual definition of positivism.
13. The section heading comprising these words has all words except 'and' capitalized. We thus do not know whether he intended to refer to 'methodology' or 'Methodology'.
14. This argument neglects competition and entry. If prediction were possible we would expect any until returns at the margin were reduced to zero. It also presumes (with no justification) that all economic predictions yield opportunities for profit. Finally, it ignores the fact that knowledge is to a great extent a public good. Thus I may be able to predict, correctly, that a 1 per cent reduction in the marginal rate of income tax will raise consumer spending by $x million, but be unable to use this to make any money, either because there are no transactions I can undertake where my knowledge gives me an advantage over other people, or because there are a thousand other economists offering the same prediction.
15. That positivism is indeed McCloskey's main target is made even clearer in McCloskey, forthcoming, chapters 1-2. Wendt argues that McCloskey, unlike many of the others discussed in this paper, takes epistemologists and philosophers of science as paradigms' (1990, p. 48).
16. Though she adopts a broader view of modernism, Dow at one point uses argument that seemingly equates modernism with logical positivism (forthcoming, p. 152).
17. Though Wendt is drawing out ideas from Amariqigio (1990), on which his paper is a comment, his definition has the advantage of greater precision and clarity.
18. Note that nothing is said here about the direction of any causation involved.
19. It might be thought that Caldwell's 'critical pluralism' counted as post-modern. Caldwell, however, does not make absolute claims, regarding critical pluralism simply
as something appropriate for a specific situation and in regard to a limited set of problems (see Caldwell, 1988, p. 241). Caldwell can thus argue that though searches for appraisal criteria have so far borne little fruit, ‘this does not mean that the search should be abandoned’ (1991, p.26). This position is not post-modern.

20. Given the confusing terminology preferred by McCloskey and Weintraub, the question arises as to whether this word is intended to start with a capital M, for they could respond to the arguments presented here by claiming that I am talking about methodology (lower-case m), or methodological discourse, to which they have no objection. The answer is that methodology as the term is used here is intended to include positions such as those of Popper and Lakatos or, in economics, Blaug. If these are viewed as foundationalist, then it is ‘Methodology’ that is being defended here, even though it is methodological discourse that is being defended. Note that these ideas are being defended as being worthy of consideration in much the same way as one can defend theories as being scientific without accepting that they are true.

21. The nature of my debt to Munz (1984) is best explained by a story. On reading McCloskey (1983, 1986) and on hearing an early version of Weintraub (1989) my immediate reaction was that their criticisms were cogent only against positivism, and that Poprian methodology survived intact. Soon afterwards a colleague referred me to Munz’s article, but I found it so dense that I gave up after a page or two. It was only then that I read through Philosophy and the Mirror of Nature, after which I returned to Munz’s article. His arguments suddenly made sense, and I have made great use of them. My change of attitude to Munz’s paper is a tribute to the clarity of Korty’s writing.

22. Economists not well versed in the history of philosophy should note that Rorty’s interpretations are open to question. Munz, for example, argues that he gives seriously misleading impressions of many philosophers, notably Wettenstein and Kant, the interpretation of whose works is crucial for his thesis. I make this point not to argue for Munz against Rorty on this point (I am not qualified to make such a judgement) but simply to warn fellow economists to be cautious about what they take on trust!

23. Of course, if one is prepared to make assumption that the process of creating economic knowledge is working well, there is no need to ask such questions, and there is no need for methodology. This paper, however, is based on the premise that we cannot necessarily make this assumption.


25. In this paragraph we express the problem as simply ‘Who is in the relevant community?’ In practice we would normally answer such a question by providing criteria such as publication in relevant journals, membership of learned societies, and so on, but this makes no difference to the principles involved.


27. Dow has indicated that this is the sense in which her remarks about schools of thought are to be interpreted.

28. Similar remarks could be addressed to Caldwell’s ‘critical pluralism’. If by this he means simply that we should debate alternative methodologies critically, and that his criteria are those that should be used along with any others we may consider relevant, that is fine, but it leaves him open to the charge that ‘critical pluralism’ is no more than a name for sitting on the fence. If, on the other hand, his implication is that debate between different schools should be conducted only in the terms he outlines, that is different: it goes against his claim that we can never know that any particular methodology is the right one, for he is making a definite meta-methodological prescription.

29. Although this is not an issue that is pursued here, it is worth noting that Rorty’s interpretation of the literature on hermeneutics, in particular Gadamer’s work, is not universally accepted (see Munz, 1984, pp. 213-6.)

30. McCloskey’s analysis of economists’ rhetoric is an important contribution which can stand on its own merits, even if his critique of ‘Methodology’ is rejected. An interest in rhetoric and discourse analysis is quite compatible with the views expressed in this paper (see Henderson, Dudley-Evans and Backhouse, 1992).

31. Whether or not the term ‘text’ is being used metaphorically clearly depends on what its ‘non-metaphorical’ meaning is. As an outsider to the field of literary theory, it seems to me that Fish’s understanding of a ‘text’ as process involving author and reader is going beyond the conventional understanding of the term. I would thus consider the term to be being used metaphorically, even in literary theory. After all, most of the arguments would apply to spoken (tape recorded?) words as much as to written.

32. Note that ‘realism’ is not the same as ‘realisticness’ (see Mäki, 1989).

33. It may, of course, be argued that methodology is concerned with generalizations concerning the relationship between knowledge and empirical constraints, and that experience has shown that there are no such generalizations to be obtained. This is an argument that needs to be taken seriously, but it is a different type of argument, to explore which would take us beyond the scope of this paper.

34. McCloskey describes the official rhetoric, to which he is objecting, as ‘an amalgam of logical positivism, behaviorism, operationalism, and the hypothetico-deductive model of science’ (McCloskey, 1983, p. 484). Similarly Weintraub characterises ‘Methodology’ as foundationalist, being based on ‘Philosophers’ attempts to justify knowledge claims’ (1989, p. 266). It is also significant that his brief summary of the history of philosophy ends with logical positivism.

35. McCloskey (forthcoming, chapter 1) clearly recognises that Popper’s Logic of Scientific Discovery (1934) constituted the ‘knell’ of logical positivism. Yet he does not explore the implications of much economic methodology, including writings of both ‘methodologists’ and working economists, having been influenced by Popper. We have simply the intriguing statement that ‘Karl Popper has played a role on both modernist and antimodernist sides’ (1986, p.21). Similarly, Rorty (1991, p.23) refers to Popper’s work as being based on a pragmatist view of truth, but containing ‘lingering positivist elements’.

36. Although it is not pursued here, it is possible to argue for an evolutionary theory of knowledge, which should appeal to someone who, like McCloskey, attaches great importance to success as a vindication of claims to knowledge.

37. Similarly. Caldwell has argued that the goal of methodology is not to teach how to do economics.

38. The word ‘misleading’ is used rather than ‘wrong’ because there is a sense in which Weintraub’s argument is, almost trivially, correct. If we define as ‘methodological’ any arguments which are not acceptable to a given
discourse community, then outsiders whose arguments are unacceptable to that community will always lose, and their arguments will always be 'methodological'. This, however, is a distorted usage of the term 'methodological'.

9. Weintraub's terminology is used because 'methodological discourse' is clearer than McCloskey's 'methodology' here, though the criticism applies as much to McCloskey - who used the argument first - as to Weintraub.

10. It is tempting to argue that they are being inconsistent in trying to settle methodological disputes by appealing to 'Meta-Methodology' (with capital 'M').

11. This view is compatible with Caldwell's position (see 1990).

12. Our conclusion might be different if we were prepared to accept, with McCloskey, that 'the economy of the intellect' is running well, exhibiting no significant cases of market failure. In such circumstances, though methodology might be feasible, it would not be needed. If, on the other hand, we are open to the possibility of 'market failure', methodology is needed to complement the analysis of what economists actually do.

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