1. The Main Issue

Economics, since its origins in the seventeenth century, has been concerned with understanding the world, and with the provision of advice to policymakers. Economists have posed and attempted to solve abstract problems, but it was always believed that these had some bearing on more practical concerns. During the last 50 years or so, however, the discipline has been transformed by the process Debreu (1991) has termed the “mathematization” of economic theory. The crucial feature of this process has not been simply the use of mathematics, but the way standards have changed. As McCloskey (1991) has put it, economists have adopted the values of the math department. Perhaps the most striking illustration of this is provided in Debreu’s presidential address where he brushes aside the criticisms of the subject made by Leontiev and others with the remark that economics cannot be an empirical discipline, which means that the only criterion we have left is mathematical rigor.

At the centre of this process of “mathematization” lies general equilibrium analysis, defined as the project which involves finding, subject to certain generally agreed constraints, sets of conditions under which it is possible to prove, with the appropriate degree of rigor, propositions such that equilibrium exists, is unique or is stable. The models constructed in general equilibrium analysis (which are only a subset of general equilibrium models; see Blaug, 1992, forthcoming, chapter 8) are acknowledgedly descriptive of no conceivable real-world economy. The intellectual values underlying general equilibrium analysis have, since the 1950s, gradually spread through the profession.

General equilibrium theory has, naturally, been strongly criticized. It has been criticized as being empirically empty (forbidding no state of the world, and yielding no predictions), for being based on unrealistic assumptions (perfect competition, individual rationality, complete markets, the absence of transactions costs), for being inconsistent with obvious facts about the world (the existence of money). To a limited extent these criticisms have been met by extending the theory (notably to deal with incomplete markets, certain forms of imperfect competition and limited increasing returns to scale), but there has remained the need to defend the theory against both heterodox and less-heterodox critics. Debreu’s defence may appeal to mathematicians, but for most economists it is not a viable option. How then, could, general equilibrium analysis, and the “mathematization” of economic theory be defended?

In the 1950s a powerful defence of neoclassical economic theory came from Fritz Machlup (1955), who argued that though much economic theory was not directly testable, it was indirectly testable. Economic theories should be viewed as part of a hypothetico-deductive system which generated testable propositions. When these derived propositions were tested, the propositions from which they were derived could be regarded as indirectly tested. This defence of neoclassical economic theory, however, had the attraction of being consistent with prevailing views in the philosophy of science, which in turn derived from logical positivism. With the collapse of the “Received view” of the philosophy of science (see, for example, Suppe, 1977), however, such a defence of neoclassical economics was left hanging in the air. Out of the proliferation of approaches to the philosophy of science that developed in the 1960s and 1970s, Lakatos’s methodology of scientific research programs proved attractive to economists. In a brilliant application of Lakatos’s methodology, Weintraub (1985, 1988) argued that general equilibrium analysis could be defended as lying in the hard core of a empirically progressive research program.

This situation, however, did not last long. Weintraub’s position being criticised from several fronts. The most noticeable of these was the line of thought inspired by the sociology of science, literary criticism and rhetoric. In particular, Donald McCloskey argued that “Methodology” was impossible, and that economists should abandon it in favor of an analysis of rhetoric, or “implicit” methodology. An implication of this was that many of the arguments used to criticize general equilibrium analysis were, along with any other “Methodological” argument, seen as being indefensible, Lakatosian and other falsificationist
ideas became, in certain circles, unfashionable.

This is the background against which I read the article (Weintraub, 1989) with which I took issue in my paper, and which Weintraub is now defending. His main argument, boldly stated in the title, was that "Methodology doesn't matter". The reason why I regarded the article as so significant, and worth taking issue with, is that he provided a philosophical argument about why "Methodology could not matter." As such it went much deeper than the rejection of methodology, unfortunately typical of many economists, exemplified in Frank Hahn's recent remarks on the subject (Hahn 1992). Whatever his intentions, Weintraub's new position provides another way to defend general equilibrium analysis: one argues that it can be judged only according to the standards of the relevant community, ruling out virtually all criticism as "Methodological". Weintraub's example of Kaldor's criticisms illustrates the point very clearly.

The crucial issue as I see it, therefore, concerns standards of appraisal. My position is that methodologists and historians of economic thought should be concerned with appraising economic theories. Whereas Weintraub accepts the verdict of the relevant community as decisive, there being no basis on which it can be questioned, I contend that, though I accept that there are no simple, infallible rules or formulae, methodologists should be prepared to pass judgement on economic theories. Weintraub seems unable to see this, and hence misinterprets my arguments. The clearest example is in his closing sentence where he writes, " unlike Backhouse, I would always hold open the possibility that methodology will wither away if no one in any community finds methodology interesting" (1992, p. 57). This is a complete distortion of my views. Of course I agree that such a state of affairs might come to pass though, fortunately, there seems little danger of it at the moment, interest in methodology running at an unprecedented level. The difference between us is that whilst I would deplore it, Weintraub believes there are no criteria available which could form the basis for such a judgement. The basis for this belief is what, in my paper, I chose to label constructivism.

2. Constructivism

The target of my paper was, as its title made clear, arguments against economic methodology. My purpose was not that of refuting developments in other disciplines. However, as Weintraub makes clear, his ideas are part of a wider intellectual development. The precise nature of his objections to my argument are somewhat difficult to disentangle, but seem to include the following:

a) Constructivism is not a coherent entity, but a collection of different intellectual phenomena "the set theoretic union of philosophical pragmatism, post-modernist literary theory, post-modernist historiography and rhetoric" (1992, p. 53).

b) I, as a mere economist, attempt "to refute some of the most significant developments in the intellectual life of the twentieth century" (ibid.).

c) I should have treated it like a religion and merely suggested that it was implausible.

I used the label constructivism simply as a convenient label for the group of ideas that I saw underlying the critiques of economic methodology that I wanted to discuss. None of the arguments within the paper in any way depended on this label, so I did not devote any space to justifying my use of the term. However, the notion that these various groups of ideas have important common features was not merely a figment of my imagination. Though he does not include Rorty as a constructivist, seeing him as a realist, Michael Devitt has provided a definition that I could have used.

"Constructivism The only independent reality is beyond the reach of our knowledge and language. A known world is partly constructed by the imposition of concepts. These concepts differ from (linguistic, social, scientific, etc.) group to group, and hence the worlds of groups differ, each world exists only relative to an imposition of concepts." (Devitt, 1991, p. 235.)

The ideas of Stanley Fish and Bruno Latour, and Weintraub, are clearly covered by this definition. I contend, therefore, that the use of the term "constructivism" is an appropriate label for the position I was criticising, that Weintraub's arguments fall into this category, and that his arguments ought to be considered in the context of the wider movement of which they form a part. I would also argue that the label focuses on the most important feature of his perspective: his view of knowledge as the property of particular (though not always very clearly-defined) epistemic communities and the conclusions he draws from this. This view of knowledge provides the basis for his perspective on justification and the notion of truth, and explains his persistent misrepresentation of my position.
3. Justification and Truth

Knowledge is the property of specific communities, created by those communities. It follows that it is interesting to see the way in which claims to knowledge are negotiated, and how perceptions of the world are dependent on such social processes: in other words, how knowledge is socially conditioned. This is a valuable perspective from which to view science, which has taught us much. This far I agree with Weintraub. The constructivist case, however, goes beyond this to argue that knowledge can be discussed only with reference to specific communities. This contrasts with, for example, a realist perspective, from which one can explore not only how communities create knowledge, but also the question of how we might tell whether or not such knowledge is true in the sense of correspondence with reality. One can, as Mäki (1992) has very clearly argued, talk about truth as something distinct from belief. If one posits the goal of seeking the truth (in the sense of that ideas correspond with reality) then one has a basis for exploring how, independently of the practices of particular communities, claims to knowledge might be evaluated. It is Weintraub’s anti-realism that leads him completely to misunderstand my arguments.

Further confusion arises through Weintraub’s view of justification. He defines (1992, pp. 54-55) two meanings of the term. Justification, refers to giving reasons for one’s beliefs. It has to do with persuasion, and depends on what particular communities find persuasive. Justification, involves providing what Weintraub calls “a secure foundation for believing”. He completely rejects the second notion of justification, arguing that persuasiveness is sufficient. He then goes on to make the completely unjustifiable claim that “methodologists” (note that the argument is now applied not simply to capital-M prescriptive methodologists, but to all those engaging in methodological discourse) seek to justify knowledge claims in the sense of grounding them on a secure foundation. That argument is absurd. This can be shown using the counter-example of Popper (he will accept, surely, that some methodologists are Popperian). Popper emphatically did not seek to provide a secure foundation for knowledge. Indeed, one of the legitimate criticisms of Popper is that he went too far in denying that we can ever know anything (see Houseman 1992, p. 177). This is the significance of Weintraub’s ending the history of philosophy with logical positivism, which did involve seeking to provide a secure foundation for knowledge. The problem for his argument is that most methodologists have long since moved on from there (see Caldwell 1982; Blaug, 1980/92).

It is because Weintraub sees only two choices (justification as persuasiveness and justification as providing secure foundations) that he misses the point I was making - that even if we accept that there are social dimensions to knowledge and that the logical positivist attempt to provide secure foundations is dead, it still makes sense to think about how we might know whether our theories are true. Truth and persuasiveness are different concepts (see, for example, Mäki, 1992). Though I did not argue this in my paper, I would go further and argue that how we answer such questions does have implications for the way we do economics (some examples are sketched in Backhouse 1992b). In my paper I did not commit myself to a particular view: I merely indicated a number of possible routes, suggesting that this was an open question, which merited discussion. Weintraub, in contrast, believes that the question is settled: there is no way in which such questions can meaningfully be discussed. To participate in such a conversation is to waste time.

This same failure explains Weintraub’s alarming comments on the role of power in relation to knowledge. Of course I accept that socialisation involves the exercise of power on the part of examiners, referees, editors and so on, and that what a community regards as knowledge may have something to do with power. My point about power not having a role to play in an evaluative methodology, was that the imposition of ideas by force does not make them true in any objective sense. I accept that for Weintraub this is an uninteresting question, because for him truth is simply what the relevant community chooses to believe, but that is a very limited and dangerous view of truth. To suggest that I am implying that views inconsistent with my own are worthless is a travesty of what I wrote.

4. Texts, Literary Criticism and History

Weintraub takes me to task for my remark about the “texts”. The furthest I will go in his direction is to concede that I should have expressed this rather differently. I am well aware that people claim that the economy is a “text” in the sense in which the term is used in critical theory (Backhouse 1991; see also Backhouse, Dudley-Evans and Henderson, 1992). However, I was not concerned with the arguments of Bruno Latour or of others working on the sociology of science, simply with Weintraub’s use of literary theory. The way I think I should have posed the issue was to make the
obvious point that "text" is being used metaphorically, and to ask how far it was appropriate to transfer the metaphor from literature to economics. My suggestion was historians of economics (or science for that matter) have to confront texts which are fundamentally different from each other in a way that is not the case with the texts with which literary critics have to deal. He asks whether I have not seen such issues being confronted in his work and that of historians who hold similar views to his own, and I have to answer "No". The problem with Philip Mirowski and McCloskey as examples is that Mirowski (1988) claims to be a realist, whilst McCloskey has recently published a paper (1991) that falls squarely within the category of evaluative methodology that I wish to encourage and that Weintraub is arguing against.4 Thus I am not sure that their positions are, in crucial respects, the same as Weintraub's.

In both his 1989 article and in his comment, Weintraub extols the virtues of the history of economic thought as a place where issues relating to the interpretation and evaluation of claims to knowledge can be made. I agree, but just the same issues arise there. If we write history from Weintraub's perspective we rule out what I, and many economists, would regard as important questions. The exchange of views in Backhouse (1992b and d) and Weintraub (1992a) covers these issues.

5. Do We Need Methodology?

Methodology is unavoidable. Economists are continually having to make methodological judgements and it is important that these are made as explicit as possible so that they can become the subject of rational discussion. Given the situation I outlined at the beginning of this paper, in which we need to face up to difficult questions about where economics is going, and about the role of parts of economics such as general equilibrium analysis, rational choice theory and game theory, methodology is particularly important at the moment. A good example is provided by recent developments in the theory of industrial organisation, where game theory has transformed the subject. If, like Debreu, one places great stress on mathematical rigor, one applauds these developments. If, on the other hand, like Franklin Fisher (1989, 1991) or Dennis Mueller (1992), one asks what the new theory predicts that the old theory did not, or whether more progress might be made if economists were to entertain a wider range of behavioral assumptions, one is led to a much more sceptical view. It is not possible to make a decision without, either implicitly or explicitly, making a methodological judgement.

The reason why I believe we need "methodology" is because I believe that the criteria underlying such judgements need to be discussed. When we enter into such a discussion we are asking questions which are conventionally termed philosophical. It is thus natural to listen to those who specialise in thinking about such questions and who have thought about them in other contexts: to philosophers. In the light of what we learn from philosophers we may then go back and draw conclusions about how we ought to be doing economics. This is certainly prescriptive methodology, in which "philosophy" is playing a role. To seek to portray such activities as necessarily arrogant, dogmatically seeking to impose criteria from outside, is absurd. Indeed, as the arguments which I presented using quotations from Munz showed, the whole concept of "inside" and "outside", on which Weintraub's argument is based, is untenable. In seeking answers to the methodological questions that we have, of necessity, to answer, we should not rule out any sources of insight, whether or not these are, according to conventional academic boundaries, considered part of another discipline.

I reacted so strongly to Weintraub's 1989 paper because I saw it as seeking to close off a discussion that I believe is important for the health of the discipline. As I thought I had made clear, I do not know the answers, and maybe we will never find them. We should nonetheless keep trying.

Notes
1. There is, of course, a contradiction inherent in this argument. An argument about why methodology does not matter must itself be a methodological argument.
2. As for Weintraub's other criticisms, they seem inconsistent with his argument against "Methodology". Weintraub makes use of ideas from, amongst others, Fish, Latour and Wittgenstein, but objects to my criticising them. (Does this mean that he, also an economist, accepted his own advice and took up these ideas uncritically? Surely not.) Is this not a clear example of someone using an explicitly "Methodological" (in Weintraub's sense) argument? At the same time he claim that my questioning constructivism amounts to end discussion.
3. In the sense of Weintraub's justify, (defined below).
4. cf. Devitt (1991), pp. 256-7. Perhaps I should not have applied this label to McCloskey. The problem is that some of McCloskey's arguments imply a constructivist perspective, others do not.
5. His comment gives completely avoids what I see as the
fundamental problems for his view of knowledge raised by Munz. I used quotations simply because I felt that Munz's words expressed these points more clearly than words I might have used.

6. An exception is Weintraub (1988), but that paper was an exercise in Lakatosian methodology of the type which I wish to defend and which Weintraub (1989) is arguing against.

7. Like Mirowski, McCloskey has claimed to be a realist (1988). This claim has, however, been vigorously contested by Mäki (1988; 1992).

References
Hermeneutics by Don Lavoie, History of Economic Thought Newsletter 47, pp. 5-10.


