Roger Backhouse’s Straw Herring

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Roger Backhouse’s paper, “The Constructivist Critique of Economic Methodology”, takes as its task the presentation, and refutation, of what he calls constructivism, but which may be better thought of as the set theoretic union of philosophical pragmatism, post-modernist literary theory, post-modern historiography and rhetoric. His paper thus attempts to “refute” some of the most significant developments in the intellectual life of the late 20th century. The refutation appears to Backhouse to be required, because he believes that constructivism is at the heart of a critique of methodology. His article leaves open the question of whether he believes that any critique of methodology is appropriate. For Backhouse the methodologist, the simplest and most sensible way to defend the methodology enterprise in economics is to undermine the critical position of those who question its own self-legitimization. We probably should admire the ambition of his project. At the same time we must look askance at its basic presupposition: if I developed an interesting, and intellectually serious, critique of methodology and based it on an ethical theory linked to the Judeo-Christian tradition, would Backhouse attempt to refute Judaism and Christianity, or merely suggest that they are implausible?

Because Backhouse’s overarching attack on the past quarter-century of work in a variety of fields (linguistic philosophy, literary theory, historiography, the sociology of science, rhetoric, history of science, cultural anthropology, et al.) takes on such figures as Richard Rorty and Stanley Fish, perhaps a reply by an economist is unnecessary. But since the reading Backhouse provides, and the argument he constructs, are potentially misleading to economists who may not have read widely in the literatures to which Backhouse refers, I think some comment is in order. I will, however, restrict my comments to Backhouse’s arguments concerning my own views.

1. Backhouse’s “Introduction” begins with a quote from Robert Solow who says “McCloskey and Klammer are in grave danger of Going Too Far” (p. 2) thus letting us all know that he, Backhouse, is not alone in believing that Rhetoric leads to “a belief that one mode of argument is as good as another.” The problem is that that is not what McCloskey, or I, or anyone who, like us, problematizes the notion “a good economic argument”, has ever said or suggested. Backhouse then goes on argue that these issues are not well-understood, and so he sets out to explain what he calls “constructivism” and its position on methodology.

2. Section II, titled “From Methodology to Rhetoric”, begins with a couple of quotations from the American pragmatist Richard Rorty, particularly from his 1980 book. Backhouse then goes on in two paragraphs to state his interpretation of McCloskey’s complex position, laid out in three books, on prescriptive methodology.

3. Section III, “Against Methodology”, begins by citing some remarks by Stanley Fish (1980) on interpretive communities, and the way such communities read texts. This is followed by a discussion of some ideas I put forward in (Weintraub, 1989) which argues that there is no position outside economic discourse which privileges particular economic arguments, or provides disinterested warrants for economic knowledge claims independent of economics. This is so, I argued, because there is no such position — no position stands outside particular local and contingent interests, thus I had argued that a claim based on such disinterestedness is a claim well-disregarded by economists. Moreover I had asserted that a prescriptive methodology is characterized by its concern for such non-contingent disinterestedness about warrants; prescriptive methodology seeks to authorize claims about the validity or invalidity or certainty or truthfulness of particular economic truths. It seeks to provide a “justification” of those claims. I suggested instead that methodology’s lack of success documents the implausibility of seeking such warrants. In arguing that all knowledge, a fortiori economic knowledge, is local and contingent and connected to a community in which that knowledge was produced or interpreted or otherwise made significant, I was certainly arguing that it was not useful to speak about economic knowledge without also speaking about economists and the communities in which economic knowledge was
produced and communicated.

4. In Section IV, “Constructivism as Post-Modernism”, Backhouse presents a few paragraphs on what a few economists have said about the connection between modernism and postmodernism. Backhouse uses this discussion to suggest that what he calls the constructivist challenge to Methodology is part of a larger set of issues — the post-modernist view of modernism.

5. This leads to the main part of his paper, the first portion of which is Section V, “Criticisms of the Constructivist/Post-Modernist Case”. Backhouse states that:

“Central to [the constructivist critique] is the claim that there are no uninterpreted givens...shorn of the claim that there are no secure foundations of 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...position is unsustainable; to show that, contrary to Rorty’s claim, there are things that we can legitimately take as given; or to show how methodology can be defended [anyway].” (p. 18)

Section V of his paper thus deals with “point[ing] out some weaknesses in the constructivist case”. After noting the role that interpretive communities, or what Knorr-Cetina (1991) calls “epistemic communities”, play in the production and dissemination of knowledge, Backhouse goes astray. He simply fails to understand the argument he is attacking, as he begins by asserting that “Problems arise, however, when the attempt is made [by constructivists] to justify [sic] knowledge claims with reference to such communities.”

Backhouse here makes the mistake common to those who assume that pragmatism is just another epistemological enterprise interested in justifying knowledge claims; he fails to read with understanding Rorty, Goodman, Herrnstein Smith, and others who say that one cannot ever “justify” knowledge claims in the sense of grounding them in secure foundations. The pragmatist response to the call to justify knowledge is to ask: What constitutes such a justification? We can legitimately ask how knowledge is produced, how it works in particular circumstances, how it changes, how it grows, etc. The pragmatist seeks to understand, and talk about, the necessarily local and contingent and social nature of knowledge. Thus to the pragmatist, there are two meanings of “justify”. In the first meaning, it is simply “to give reasons for”. It is this sense in which the pragmatist justifies assertions, truths, knowledge claims, etc. The second meaning of “justify”, the one with the epistemological connection, is “to provide a secure foundation for believing”. Call these “justify” and “justify2”. “Justifying2” is one of the few activities the pragmatist does not engage in with respect to knowledge claims: to seek to talk about justification in this sense is already to refuse to consider the pragmatist as a serious person, one who does not mean it when he or she asserts that to talk about “justifying2” knowledge is to talk unproductive blather.

Since no pragmatist will ever engage Backhouse on the topic of justification, he must of course construct his own “constructivist” view that the “community determines knowledge” by quoting a critic of Rorty, Peter Munz. Backhouse goes on to say that the claim that “community determines knowledge” view forces the problem of justification back one stage, because the constructivist must justify the choice of a dividing line between communities — we need a decision criterion else we end up in “relativism”. Note that this problem exists only for Backhouse, for having forced the straw-pragmatist to answer his “justify,” question in a fraudulent fashion, Backhouse can, with good reason, cry “fraud”. My own response, less interesting perhaps than one which people like Rorty or Fish might provide, is that all knowledge is community property, and our wish to speak about that knowledge forces us to speak about communities, and their connection to that knowledge. Communities are not defined except in a Wittgensteinian sense in which we may define a “game”. Communities justify, utilize, interpret, communicate, reject, modify, and appraise knowledge claims, and our discussion of these activities is community-confingent, and recognizes that communities share members, and thus knowledge.

Backhouse goes on to suggest that I take the existence of such communities as a “given”, but he opines that this goes against my fundamental (?) premise: i.e. “This does not, however, solve the problem...[for] there are no uninterpreted givens.” On the contrary however, I assert that there is no problem to be solved, Backhouse’s claim to speak for me notwithstanding. We, and here I mean the community of those of us who wish to engage on these subjects, can argue with one another, by which I mean construct arguments fashioned to persuade one another, about the nature and substance of claims about the connection between knowledge and communities in which that knowledge plays a role. We do not attempt to justify, such claims. We
discuss them. Backhouse may not like that answer, for it is not the kind of argument he seems to understand, but it is the pragmatist’s answer. Faced with a methodologist’s demand to justify, to ground, a belief in particular bit of economic knowledge in the Immutable Ineffable, I too refuse.

Backhouse gives us an insight to his fears with the following remarkable paragraph: “Suppose that defining the relevant knowledge communities were unproblematic. Would the case for grounding [sic] knowledge in such communities then be defensible? There are several reasons why it would not be. One is that knowledge would depend on power...This may be a fact of life, but there seems to be no place for such a concept in an evaluative methodology.” (p. 20)

Put another way, Backhouse recognizes that the ability of members of a community to persuade one another of the merits of one position or another may depend on a number of different arguments, and that arguments from authority are not necessarily without merit, nor are they always unconvincing. Power does matter. I must also point out that members of a community are socialized to that community in sometimes coercive ways — we do after all pass or fail graduate students in economics on examinations which identify whether the students are beginning to “think like economists”, to accept our use of evidence, to accept our definitions of what evidence “is” etc. But Backhouse would regard such arguments as flawed because they fail to satisfy the methodologists’ need for secure foundations of claims to knowledge. It would appear thus that Backhouse’s paragraph simply argues that a position inconsistent with his own is a priori worthless, that it has “no place”.

He goes on to suggest that there are problems with the constructivist view because there are different communities, and there is no particular way to decide which community is correct. Phrasing the issue in his terms, what is the “normative implication” of the fact that there are different interpretive communities? He may not like it, but the answer I would give is that there are no normative implications beyond the fact that communities have norms and standard modes of talking and acting.

He partially sees this, but appears to be uncomfortable with what he sees, as can be seen in his remark that: “None of this is to say that the notions of interpretive communities or schools of thought may not be an invaluable tool for analyzing the history of economic thought...This, however, takes us into historiographic issues.” (p. 20)

In contrast, I would like Backhouse to consider that “analyzing the history of economic thought” is one possible enterprise (Is it perhaps the only enterprise?) which can approach the topics he himself wishes to understand. For if his game is “justifying, or grounding economics knowledge claims”, then he can never win. If instead his game is appraising knowledge claims, criticizing such claims, understanding such claims, then that game can be played by either doing economics, or writing history. In doing economics, one is engaged daily in constructing and evaluating knowledge claims. And in doing the history of economic thought one is continually engaged in the process of reconstructing the way economists have authorized, and interpreted, such claims.

The final paragraph of Backhouse’s Section V, on literary theory, misunderstands the idea of the “text” as that term is in fact used in literary criticism. He writes that: “What are the ‘texts’ in economics discourse?...[P]resumably the ‘texts’ include not only written documents...but also statistics...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.” (p. 22)

Backhouse here needs to read more on the subject. He might start with the basic idea of “text” as it is used in (Fish, 1980) which he cites, or with sociologists of science like Bruno Latour who has argued that even the economy is a text in the sense in which the term is used in critical theory: “The state of the economy, for instance, cannot be used unproblematically to explain science, because it itself is a very controversial outcome of another soft science, economics...It is extracted out of hundreds of statistical institutions, questionnaires, polls and surveys, and treated in centers of calculation. Something like the Gross National Product is an n-th order visual display which, to be sure, may be combined with other paper forms, but which is no more outside the frail and tiny networks built by economists that stars, electrons, or plate tectonics.” (Latour, 1987, p. 256)

The study of the interrelationship among various
texts, or as Barthes would have it "the textuality" of various documents variously written, spoken, and pictorial, is the hallmark of what is called critical studies. It is not the case that it "needs confronting" by economists, but rather that Backhouse does not appear to see it being confronted in my writing, or the work of Don McCloskey, Philip Mirowski, Jane Rossetti, Vivienne Brown and others in different ways.

6. In his next Section VI, "The Case For Methodology", Backhouse's argument flies off the rails. His first sentence sets up what Barbara Herrnstein Smith calls "the straw herring" of scepticism. He writes:

"The claim underlying the arguments against methodology discussed in this paper is the claim that we cannot know the truth about anything." (p. 22)

This misconstrues the constructivist argument. For me, I can confidently assert that I know many truths, even many economic truths. Further, I am using "know" in a coherent, sensible, and straightforward fashion: I can confidently discuss the truth of either the proposition 1) that rent control does more harm than good, or 2) that we do not observe disequilibrium in markets, or 3) that Whig history is not the only, or necessarily best, way to write the history of economic thought. I know these truths in the same way, generally, as I know all truths, contingently and contextually. For Backhouse baldly to state what is not true, namely that I cannot ever speak of knowing the truth about anything, is to deny that I am currently constructing an argument which will eventually conclude that Backhouse is wrong, and that when I have finished presenting my evidence, reasons, and argument you, the reader, will know that that is the truth, for I will have convinced you. That is so even though I know that Backhouse says I am using "know" incorrectly.

Backhouse constructs his own refutation of scepticism by providing a long quotation from Hookway's book, Scepticism, which asserts that we can safely ignore scepticism. For Backhouse the issue appears to turn on empirical evidence. Backhouse goes on to say that

"it is going too far [recall Solow] to argue that there is no such thing as empirical evidence. The constructivist arguments end up treating all knowledge as similar in kind..." (p. 23)

This statement, which makes unattributed assertions about unnamed constructivists, leads Backhouse to the view that it is best that we "ignore the constructivist arguments and take the existence of empirical evidence for granted." (ibid.) But who ever argued that empirical evidence did not exist? Not I, not Rorty, not Latour, or Woolgar, o. McCloskey, or Klamer, et... To say that we construct knowledge is also to say that we construct empirical evidence; that is, we use language to distinguish evidence from non-evidence, to utilize it in constructing arguments, etc. "Evidence" works in certain ways in certain communities for certain local and contingent purposes, and what is "empirical" itself is contested from time to time. We talk about those contests as we talk about and write about evidence when its meaning and nature is not contested.

Backhouse concludes this section by telling us that

"Rorty's argument is that knowledge needs to be justified... and he merely seeks to replace one form of justification with another." (p. 27)

To argue this point, which denies Rorty's own claim that we cannot ever ground knowledge claims Backhouse constructs no argument himself. Instead he runs together a set of quotes from Peter Munz a critic hostile to Rorty's position, which ends up saying that "Rorty never considers the possibility that..." He continues this argument with a quotation from Karl Popper who, Backhouse suggests, has a more sensible notion that "all knowledge is hypothetical". Again, Backhouse simply will not allow Rorty, or any "constructivist", to escape the methodologists' necessity for grounding and justifying in the sense of justify. Knowledge is well-conceived by Popper as hypothetical if it is unfalsified but nonetheless "grounded", but this still forces knowledge into categories of justified or not justified. This argument is thus entirely irrelevant to the individuals like me whose positions Backhouse wishes to criticize.

7. Finally, in Section VII, "The Role of the Methodologist", Backhouse takes up my argument that there is no shortcut to understanding Economics through Methodology. Backhouse ends up by saying that

"The lesson to be drawn from this debate is not that 'methodological' objections to a theory are foolish, but 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." (p. 32)

On the contrary I do not believe we need to discuss methodology. The process of clarifying arguments made by ourselves and others is not methodology
in Backhouse’s sense of the noun. Backhouse asks for a constructive debate, but a constructive debate appears in Backhouse’s vision to require objective, meaning certain and non-contingent, criteria to decide which argument is better. This is not a debate at all, but rather a monologue. I will not participate in such a conversation about such criteria: it wastes time.

Which is not to say that some people might enjoy, or otherwise find merit in, discussing methodology. Let us be specific here. Consider a “theory” like general equilibrium theory discussed by Backhouse. Now let us state objectives for the theory, and criteria we use to decide whether the theory is helping us. Suppose we pick the objective of “predicting oil prices”, and the criteria of “predictive accuracy to within 5%”. Is this “Methodology” or “methodology”, or even “methodological discourse”? I think it is not any of these, but it is rather argumentation concerned with whether “predictive accuracy” is or is not worth having, where worth is contextual and based on those who are doing the valuation for themselves or others at a particular time and place. And this is often characteristic of a discussion the context of which is the worth or value of the theory itself. That discussion is, perhaps, methodological in a limited sense of the term, a sense more restricted than that which methodologists concerned with justification, usually prefer. But a discussion which attempts to ground, or justify, or otherwise evaluate the truth claims of one or another methodological discussion, is Methodological as I use the term. Of course some people enjoy discussions of Methodology. Others do not. But it is time to dispense with Methodology’s claims for privilege: we can perhaps agree that such claims are in fact special pleadings which we need not take seriously. We can perhaps agree that Economics needs methodology or Methodology no more or less than Economics needs Poetry or Soul or Ethics or Mathematics or A Feminist Perspective or An Historical Sensibility.

Thus Backhouse’s final clause, that “methodology as a discipline with roots in both philosophy and economics should not be allowed to wither away” is best construed as a plea for tolerance, one which I support conditionally. For unlike Backhouse, I would always hold open the possibility that methodology will wither away if no one in any community finds methodology interesting.

References
Fish, Stanley. 1980. Is There a Text In This Class?. Cambridge: Harvard University Press.

Note
* Discussions and correspondence with Roger Backhouse.