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Towards a broader conception of criticism

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Why I wrote the article

I wrote 'Praxeology and its critics: an appraisal' (*HOPE* 16.3:363–79) because I was dissatisfied with the way that much methodological work is done today, and I wanted to provide an example of an alternative way to undertake methodological criticism. In the article I analyze praxeology, the methodological position associated with the Austrian, Ludwig von Mises. Because I criticize both praxeology *and* some of the arguments brought against that position, I knew from the start that the article was not going to be wildly popular. However, it was not my intention to write a confusing article. In this reply to Professors Hirsch and Rotwein, I will attempt to clarify my position and to address some of their criticisms.

Much methodological work proceeds in the following way. A critic, who typically is a member of a particular group within economics (say, Group A), argues that Group B's approach to economics is wrong. The critic accomplishes this by asserting that there is a proper and correct procedure for science. He then asserts that, while members of Group A regularly follow this procedure, members of Group B fail to follow it. Thus, Group B's approach is unscientific and should not be further considered. The critic rests his case. Usually there is a counterattack in which a member of Group B asserts that Group A's vision of science is flawed.

There would be nothing wrong with such an approach if there existed a widely agreed-upon definition of what constitutes legitimate scientific practice. For better or worse, most methodologists have looked to philosophers of science to provide such a definition. However, even a casual reading of the philosophy of science of the last three decades reveals that no widely agreed-upon definition of legitimate scientific practice exists. As a result, the kind of argumentation outlined above is bound to be inconclusive.

Even worse, the endless rounds of debate that are a natural consequence of this kind of approach to methodology make methodological work itself seem pointless. Because methodologists typically look to general models of scientific practice articulated within the philosophy of science for their arguments, they often neglect the richness and diversity of the ways eco-

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nomics is actually practiced. Instead of shedding light on what economics is, methodological attacks and counterattacks involve stylized conceptions of how economics is done. For example, Austrians, Marxists, and other heterodox groups in economics are often dismissed by 'mainstream' critics for not following falsificationism. But it is certainly an open question whether *any* group in economics follows falsificationism.[1] Such criticisms therefore are not very effective against the heterodoxy, nor do they help us to understand the practice of mainstream economics. It is no wonder that practicing economists, who learned how to do economics in graduate school and who have little training or interest in philosophy, find the arguments of methodologists to be rather tiresome. Instead of helping us to understand what economics is all about, methodological debate too often ends up as a confusing cacophony of competing claims which invoke stylized and obscure visions of what economics is or should be.

I wrote my article, then, to suggest that methodology could be done in a different way. It was intended as a suggestive case study, rather than as an exhaustive appraisal of a position. I chose praxeology as my subject because it struck me that the level of debate over the merits of this strange position was particularly low. For example, in his book *The methodology of economics*, Mark Blaug dissects nearly every methodological position that has been put forth within economics in the last two centuries. His analysis for the most part is careful and thoughtful. But when he comes to Mises, instead of offering *arguments*, Blaug offers *invectives:* Mises' position is a "travesty," "cranky," "idiosyncratic," and has "to be read to be believed" (Blaug 1980, 92–93). Though Blaug's language is extreme, his feelings probably fairly represent much of the profession's reaction to praxeology: here, at last, is a methodological position that is clearly 'unscientific,' one that may be disparaged openly. For these same reasons praxeology seemed to me to be a perfect candidate for analysis.[2]

The critics of praxeology

Critics of praxeology, to the extent that they offer arguments, usually focus on one of two things. The first is Mises' strange notion that the

1. For some arguments against the usefulness of viewing economics as a falsificationist science, see Caldwell 1982, ch. 12; and 1984a. The opposing position is perhaps best represented by Blaug 1980.

2. Because praxeology is held in such low repute, I thought it important to emphasize that not all Austrian economists embrace praxeology. This caused some confusion for Hirsch. To clarify matters: I used the term 'praxeology' to refer exclusively to the methodological position of Ludwig von Mises. Mises' position on methodology is the most widely known among *non-Austrians;* it is in this sense the dominant position. Among Austrian contributions in which alternative approaches to methodology are taken, the most current example of apostasy is the book by Jerry O'Driscoll and Mario Rizzo (1985). Unfortunately, I got both the title and publication date of this book wrong in the article.

axioms of praxeology are at once a priori true and empirically meaningful. The second is the equally bizarre notion that empirical evidence can never be decisive against praxeology; that tests of hypotheses may indicate that praxeological reasoning has been wrongly applied, but never that praxeology itself should be rejected.

If one accepts as a premise that any one of a number of empiricist doctrines (e.g., positivism, logical empiricism, operationalism) correctly delineates what constitutes legitimate scientific practice, then Mises' position must be judged to be unscientific, dogmatic, even unintelligible. Most of Mises' critics, including Hirsch and Rotwein in their comments, implicitly or explicitly accept such a premise. *The key to my rebuttal of the arguments of the critics of praxeology is to reject the premise that a single widely accepted and well-defined definition of legitimate scientific practice currently exists.*[3] As a result, criticisms of praxeology that presuppose such a definition are considerably weakened.

Moving now to the details of my critique, two of my arguments may be treated briefly. Some of Mises' critics found his position to be *unintelligible;* they simply were unable to fathom what Mises meant by the phrase, "All human action is rational." I tried to explicate Mises' position, and at least Rotwein thinks I did so successfully. Others have found Mises' position to be *dogmatic*. The dogmatism argument is interesting historically. As Rotwein correctly observes, positivism and other empiricist doctrines were initially conceived to serve as a constraint on the unbridled and often dogmatic speculative theorizing that was rampant in certain forms of nineteenth-century philosophy and which occasionally showed up in the science of that era.[4] The intent of the positivists was to oppose such dogmatism, and such intentions are laudable. Unfortunately, the positivists were unable to come up with a logically watertight and operationally applicable criterion for distinguishing the speculative from the scientific. To act as if such a criterion exists, and to use it selectively against one's enemies, is in my opinion simply another form of dogmatism. In any case, it seems to me that claims and counterclaims of dogmatism accomplish very little in methodological debate.

The key argument against praxeology is that it is *unscientific*. Take, for example, Mises' claim that the 'fundamental axiom' of praxeology, "All human action is rational," is both a priori true *and* empirically meaningful. How might one go about criticizing this claim?

One might point out that Mises has not offered good arguments in support of it. Rotwein points this out when he shows the flaws in Mises' and

3. In the jargon of the philosophy of science, I do not believe that the demarcation problem has been solved.

4. A superb review of some of these issues in another science, biology, is contained in Rosenberg 1985, ch. 1.

Rothbard's argument that attempts to prove or disprove the axiom presuppose its validity. Similarly, I mention in passing (p. 368) another of Mises' flawed defenses, the argument from analogy—that praxeology, like Euclidean geometry, begins from foundations which are both certain and empirically meaningful.

The other major argument offered against Mises' claim is drawn from empiricist philosophy of science. According to the analytic/synthetic distinction, all cognitively meaningful (read "scientifically legitimate") statements are *either* analytic (definitional, hence lacking in empirical content) *or* synthetic (contingent statements of fact, hence testable and empirically meaningful). Now since Mises claims that the fundamental axiom is *both* definitional (analytic) *and* a statement about reality (synthetic), his position is at odds with the analytic/synthetic distinction and thus must be deemed unscientific. This is Rotwein's second argument, one that he feels "should suffice to dispose of Mises' a priorism." Of course, Rotwein's argument presumes that the analytic/synthetic distinction is unproblematic. But contemporary philosophy of science shows that there are both logical and operational problems associated with employing the analytic/synthetic distinction, problems which Rotwein ignores.[5] In my article I also mention that contemporary accounts of the logical status of the rationality postulate in economics have not resulted in a consensus view. My point was not to offer support for Mises' position, as Rotwein suggests. It was merely to show that even within the (presumably more 'scientific') mainstream of economics, problems exist about how to characterize 'fundamental assumptions.'

Mises' other major claim, that tests of hypotheses are not decisive against praxeology, has also been branded unscientific. Critics of praxeology again invoke empiricist philosophy of science to make their case. And again, my rebuttal involved pointing out that there are many holes, *important* holes, in the empiricist accounts of the role of hypothesis testing and prediction in the appraisal of theories. One final point, a point that I emphasize on page 373 of my article, deserves reemphasis. *I am certainly not claiming that empirical work has no role in science*. Indeed, I believe that any adequate account of what science is will give a prominent role to empirical work. My claim is that *no current account of the role of empirical work in science has been adequate,* and therefore arguments which presuppose the existence of such an account are less than effective.

Criticisms of praxeology

Were this the end of my argument I could fairly be described either as a defender of a priorism (or, à la Rotwein, as a flirt) or as an epistemological

5. These arguments are reviewed in the first section of my *Beyond positivism*.

anarchist. As far as methodology is concerned, I am neither a flirt nor an anarchist. I am a pluralist, a view I have defended elsewhere (e.g. Caldwell 1982, ch. 13; 1985; 1986). For a pluralist, the criticism of arguments is extremely important. In the first half of the article I showed the weaknesses of the arguments propounded by praxeology's critics. The next logical step was to reveal the weaknesses of the arguments for praxeology.

As a pluralist, this part of my article was absolutely crucial. My earlier arguments, after all, rested on the premise that empiricist philosophy of science no longer provided an adequate grounding for criticism within economic methodology. Only if I could show that meaningful criticisms could still take place in the apparent void that lies beyond positivism would I be able to avoid the label of anarchist.

To highlight the difference between criticism that presupposes the truth of empiricist philosophy of science and criticism that does not, I chose the (in retrospect probably unfortunate) labels of *external* versus *internal* criticism. This labeling scheme clearly confused Hirsch. My point was a simple one, really. I wanted to see if I could come up with criticisms of praxeology that, unlike the ones mentioned above, did not depend on an acceptance of empiricist philosophy of science for their force. I sought out criticisms that, if they were successful, could not be dismissed by praxeologists because they began from empiricist grounds. To clarify things for Hirsch, I do not believe and never claimed that internal criticism is more fair or more meaningful than external criticism. Nor did I ever suggest that internal criticism is the best or the only form of criticism; indeed, as a pluralist I embrace all forms of criticism.

Since Hirsch finds my examples unenlightening, I will try to make them clearer. Take the praxeological claim that the fundamental axioms are both a priori true and empirically meaningful. One might ask which of the half-dozen or so axioms are to be granted this status. One might ask what "a priori true" means, and what arguments exist to support this position. (Thus, I would view Rotwein's first argument as a form of internal criticism.) But the crucial argument against the praxeological position on axioms involves the operant conditioning example. Mises claims that the statement, All human action is rational, in the sense of purposeful, is both definitionally true *and* empirically meaningful. One can set up definitions in any way that one likes. But if this axiom is also empirically meaningful, it may be restated as, There exists no human action that is not purposeful. The only exceptions that Mises permits are individuals who are in "a vegetative state." Mises had no problem overcoming the usual counterexamples, for one can easily argue that habitual, or impulsive, or addictive behavior is purposeful. However, one cannot argue that operantly conditioned behavior is purposeful, nor that the conditioned subject is a vegetable. This counterexample doubtless sounds trivial, even silly. But note

that praxeologists stake their entire position on the claim that the universal statement, All human action is purposeful, is both always true and empirically meaningful. A single counterexample, if accepted as a legitimate counterexample, is sufficient to refute praxeology. As such, the operant conditioning counterexample must be taken seriously.

Even if the counterexample is not accepted, there are other ways to attack praxeology. The route that shows the most promise is to question the "verbal chain of logic" that leads from the axioms to the conclusions. I personally believe this to be the most promising route, for if one can show that a position's axioms, no matter how well-defended, lead nowhere, one has provided a devastating criticism. I offer a few examples of how such a criticism might be undertaken. Hirsch echoes some of these arguments, and adds a few of his own (e.g. the *ceteris paribus* argument). It is interesting that Hirsch, who first denigrates internal criticism, should offer internalist criticisms of his own. This establishes in my mind his sincerity when he says that he does not understand what I mean by the term internal criticism.

Conclusion

It should now be clear that, though praxeology was chosen as the subject of my case study, I could as easily have chosen Marxism, or Institutionalism, or some other non-traditional approach to economics. My goal was not to conclusively establish or reject the claims of praxeology or of its critics. My goal was to broaden our conception of criticism within economic methodology, to show that we can go beyond the limited conception of criticism that is implied by various empiricist approaches within the philosophy of science.

Note that *some* of the arguments against praxeology mentioned by Hirsch and Rotwein would qualify as internal criticism. Thus I am not calling for a radical transformation of the way we do methodology. I am simply pointing out that certain arguments against praxeology, those that presume the truth of certain propositions from the philosophy of science of four decades ago, should no longer be considered sufficient to defeat the position.

My argument rests on the premise that empiricist philosophy of science has not resolved the demarcation problem. Put more precisely, I am arguing that, were any of the proposed solutions to the demarcation problem uniformly applied in economics, very little if any of economics could be considered scientific. My article may be viewed as one possible response to that dilemma.

There are other responses. One might ignore the dilemma, as Blaug, Hirsch, and Rotwein do. Or one might turn to the analysis of texts or of rhetoric, as Donald McCloskey (1983) suggests. Or one might try to find some layer of phenomenal reality that underlies social phenomena that better fits the empiricist doctrines, as Alexander Rosenberg (1980) tries to do. My response to the dilemma, to employ a critical pluralism to try to better understand the strengths and weaknesses of various arguments made by economists and methodologists, is but one of many.

But it seems clear to me that *some* response is necessary, if methodological work is to be taken seriously. It is telling that both Hirsch and Rotwein misunderstood this article, that they thought I was trying to defend a priorism. Their misunderstanding is understandable, since the major function of methodological discourse has been to buttress arguments for preferred theories, or to aid one in attacking the theories of rivals. And indeed, in the past and today, those attracted to methodology are often mavericks within economics, people who seek to establish a new way of approaching their subject.

This *role* that methodological study plays within a discipline is itself an interesting sociological phenomenon worthy of study. As Kuhn points out, this role for methodology helps to explain why interest in methodological work waxes during periods of revolutionary crisis (when mavericks proliferate) and wanes during periods of normal science. But as a person who takes the study of methodology seriously, I have a goal—to make methodology matter. From my current point of view, the real contribution that methodology can make is to help us better understand economic science in all of its diversity. And until it accomplishes this or some other similar goal, methodology has no special claim. It will continue to be used by iconoclasts and ignored by the mainstream. Such an outcome is one that I wish to avoid.

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