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LETTER TO THE EDITORS

THE VERY IDEA OF EPISTEMOLOGY

A Comment on Standards

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In a recent issue of *Economics and Philosophy*, you reflected on the referee reports so far (Hausman and McPherson, 1987). It's an interesting piece. You note that ill temper and misunderstanding occur not between economists and philosophers, as one might have anticipated, but between different schools inside economics or philosophy. You note that the numerous cases of good temper and good understanding from the referees exhibit "McCloskey-ish" scholarly standards, "partly tacit and much more flexible" than the received Methodology. You manage to conclude nonetheless that "the exercise of informed judgment, guided by broad and evolving principles of assessment" still rests on "implicit or explicit *epistemological* principles" (emphasis added). In short, the McCloskey-ish view of what happens and what should happen in science is accurate and even nice, but still must somehow, ultimately, in the end, in the last analysis, "rest" on epistemology.

You asked in the editorial for comments. I'm glad to oblige. At the beginning, though, let me offer some thanks and one complaint. Thanks for the attempt to coin an adjective from my name. The pity is that it doesn't exactly roll off the tongue. Hmm. McCloskeyite? No. McCloskeyish? Gak. Well, never mind.

My one complaint is that your scripting of the McCloskeyean position highlights the antipositivism in the first few chapters of *The Rhetoric of Economics* (McCloskey, 1985). Let me draw your attention to the other seven chapters, the literary analysis of economic texts. It's the main point of the book, yet no student of philosophy has gotten beyond

Chapter 3. The droning debate in Western culture about Methodology and Epistemology is 2,500 years old, after all, and still hasn't arrived at a conclusion. My early chapters, beating the dead horse of positivism, are conventionally methodological (even if they attack methodology), which must explain why methodologists feel comfortable in them. But frankly I think they are a little tedious. Really, how many times must we go over Milton's Article? He himself didn't take it all that seriously, judging from his practice. By contrast, literary criticism, Talmudic commentary, Biblical exegesis, close reading, and rhetoric have illuminated every text of our civilization from Moses and Homer down to Jane Austen and the commercials for Miller Lite. So far no one has seen that economics is a text, too. As Chapters 4 through 10 show, when we use math or metaphors we are *talking*. Gosh. Now *that's* interesting.

But I won't look a gift horse in the mouth: I'm glad we agree that "modernist ritual," as you put it, "narrowly rule-bound," is dead; and that after all, "there just isn't much choice in this interdisciplinary setting but to think hard about the arguments before you." I suppose that's what good scholars have always done. People of sense – this lets out the *Publication Manual of the American Psychological Association*, for example (see Bazerman in Nelson [1987]) – do not spend a lot of time worrying about whether a work has proper Scientific Methodology. You and I agree, then, that we should tell economists that being narrowly rule-bound is out of fashion, and when it was in fashion, it was juvenile. We both want economists to stop mugging each other with positivistic and other Methodological clubs.

We agree, too, that there does exist a "middle ground" between day-to-day, small-*m* method (for instance: Use statistical significance when the problem is a sampling problem) and glorious, humanity-making *sprachethik* (for instance: Do not sneer at arguments merely because the person making them is a friend of Milton Friedman's). The middle ground is called Methodology. Your editorial seems to claim that McCloskey does not believe in its *existence*. I want to make clear that I do. My own experience as an editor, like yours, suggests that "standards for judging papers . . . seem to derive in important measure from *theories of knowledge* . . . that carry normative implications" (emphasis added). Methodology exists all right.

The question is whether it should. You do not argue the point, but leave the impression that you think Methodology in economics and its big brother Epistemology in philosophy are desirable. I merely note that you are being inconsistent. Your own experience as editors suggests the opposite normative conclusion: Methodology and Epistemology spoil conversations; let's get rid of them.

Consider your point that the ill-temper and misunderstanding occur *inside* fields, not between different fields. We all have this experience. If a classicist invites me – an ignoramus on things Horatian – to think

such and such about Horace's use of structure in his first three books of odes, I have no trouble being flexible in mind and open in spirit. But when she asks me to think such and such about the Roman economy and ventures an economic thought of her own, I start closing up, since I am supposed to be an expert on things economic: "What's this nonsense, anyway? My Lord, if that's so I'm out of job." If you have nothing in your brain or pocketbook about Horace you are, as we say, "open-minded." It is interference with established modes of thought, not a new thought on an entirely different subject, that generates academic static. The closer the broadcasting bands, the greater is the interference.

Now what's the problem here? Are these "established modes of thought" equivalent to the modest, desirable small-*m* method, the rules of getting regressions right and making demand curves slope down? As you argue, no. On small-*m* matters no one gets very excited. A paper violating some small-*m* rule of method is easily fixed: "[I]t is usually possible for the author of an otherwise worthy paper to spruce up his or her argument suitably."

On the other hand, do the established modes of thought make people so cross because they disagree about some lofty matter of *sprachethik*, some high-level point about the morality of scholarship? No again, although we economists and philosophers really should pay more attention in our writings and in our teachings to the morality of scholarly dispute. It is not only classical scholarship in the age of Bentley that is disfigured by

> the extraordinary spectacle of men of learning and genius, of authority and divinity, brawling about Greek and Latin texts, and calling each other names for all the world like bookies on a racecourse or washerwomen in a back street. For this vehemence of temper and virulence of language were . . . unhappily characteristic of the profession as a whole. (Woolf, 1925, p. 198)

What then causes malicious misunderstanding among scholars – the Bad Referee reports? The cause is not lowly method or lofty *sprachethik*. It is middle-brow Methodology. Like nations and religions, the schools in economics and in philosophy maintain their solidarity and their definitions of barbarians by means of Methodological talk: for example, such-and-such is "serious Scientific work," that is, the way we Hellenes talk; the rest is bar-bar-bar.

What is objectionable about such sneering is not that it gives us standards, worked out in good conversation within the field. Having standards is obviously good, as I argue in detail in Chapters 8 and 9. No one believes that anything goes *in argument*. What is objectionable about such sneering, on the contrary, is that it imports unexamined standards from the outside with no respect for *sprachethik* within economics or philosophy. An American philosopher who starts discussing

Heidegger or Habermas seriously will be sneered at (an eminent American philosopher told me recently without shame that he had never read any Hegel and furthermore didn't intend to). "That Continental stuff is just *not serious work.*" An Austrian economist who uses econometrics or a Marxist who uses continuous production functions or a Minnesota rational expecter who uses macro arguments without micro foundations will simply by ostracized from his tribe. "It's not serious work." These are Methodological convictions. They are not the modest, concrete rules of method ("a *t*-statistic assumes the error is like a drawing from an urn") or the grand, moral rules of *sprachethik* ("really, you should stop sneering ignorantly at Hegelians or post-Keynesians and try reading what they have to say, on the plausible assumption that they, like you, are serious and honest scholars"). The Methodological convictions are something in between, neither rules of art nor rules of morality. They cause only mischief.

We agree, of course, that any conversation that is not going to ramble pointlessly must adhere to temporary agreements about what is relevant. A seriously considered limitation of the argument is always a good idea. Narrowness is not always a bad idea.

(Economists at this stage will trot out an argument from specialization: "Don't you believe in the division of labor?", they will say when, for example, one queries some autistic speciality hostile to every intellectual value in economics except those imported unexamined from the department of mathematics. "Shouldn't I be allowed to *specialize* in arguments from micro foundations of overlapping generation models whose central reasoning is copied out from a handbook of electrical engineering and be allowed to sneer at everything else?" What the economists forget when they use such an argument is that specialization is good *only when accompanied by subsequent exchange.* Exchange requires looking into what other people have produced and buying some of it. Otherwise we in our specializations are just piling up soybeans and microfoundations in the backyard.)

The problem comes when the narrow, temporary agreement is graven in stone, and elevated to a Methodological Commandment. It will be sweet work for psychologists, say, to talk long and hard about Observable Behavior, temporarily setting aside arguments from introspection. But if they make the methodological rule permanent, throwing introspection into a nonscientific outer darkness forever and ever on merely Epistemological grounds, they fall into absurdities. Two behaviorist psychologists make love. One says to other, "*You* enjoyed that. Did I?"

What I wonder about is the claim, asserted repeatedly in philosophy since Plato, but never examined directly, that some temporary and practical narrowing of the conversation should be made permanent because it satisfies an Epistemology forever true. I, therefore, cannot make out

what you are arguing when you say that scholarly "standards . . . are [not] a mere ratification of whatever practices turn out to be effectively persuasive." I cannot see why you view "effective persuasion" as "mere," to be spurned. It seems good enough to me. It had better be good enough, since it's all we've got.

Effective persuasion would of course be spurnable if there were something better. But I reckon there isn't. Justified true belief is an admirable ideal. If people are not made to justify their assertions and do not believe them, the conversation will, of course, be a poor one (as is the very conversation of epistemology, on precisely these grounds). But there is no route aside from human persuasion to know whether the third part of the definition applies: True. Epistemology has not yet solved its self-imposed problem – its only problem – of getting outside human conversations to decide what to believe on grounds other than what is persuasive to humans.

The mischief arises from the very idea of epistemology, the idea that there is an intellectual free lunch out there waiting to be seized that will allow us to decide whether such-and-such is true for all time. I repeat that I'd rather quietly drop the whole matter and get back to economic history or rhetorical analysis. The insistence that every issue is philosophical is one of the rare excesses of philosophers, parallel to the economists's sin of economism. It amounts to insisting that we should never get off the subjects proposed by Plato (just as economism insists that we should never get off the subjects proposed by Adam Smith). That Plato emphatically wished to change the subject away from rhetoric has long been a disability in trying to reintroduce rhetoric. But I'm saying that his subject comes from arbitrary definition, not from something we should respect.

The crux is that nothing can provide the "knowledge" defined by epistemology. This "knowledge," as distinct from the effective persuasions by which we run our lives, is "whatever it is that is in the mind of God" or "what we will know at the end of history" or "what we will never, ever come to disbelieve." It is hopelessly nonoperational. The project of demarcating the world once and for all into what is Knowledge and what is Mere Superstition seems likely on past form to lead to absurdities, such as the demarcation into earth, air, fire, and water. The categories have present use, mainly the use of letting our less secure philosophers batter certain people for being uselessly Nonscientific (for instance, Vergil, Jesus, Dante, Darwin, Marx, Freud). But it is just as well to bear in mind that the categories have *only* present use. As Borges put it, "obviously there is no classification of the universe that is not arbitrary and conjectural. The reason is very simple: we do not know what the universe is" (p. 104). God does. We don't. Niels Bohr said once that physics was not about the universe, but about what we as human beings can say about the universe. Better stick to saying useful things

about our sublunary life, about how we can effectively persuade each other for here and now. Rhetoric is man's project; Epistemology is God's.

I think that the early experience of Hausman and McPherson as editors argues for a richer, rhetorical theory of how people argue than the one provided by Epistemology of the usual sort. The recent exceptions in philosophy I can think of are philosophers thinking in rhetorical terms, such as Keith Lehrer and Carl Wagner's *Rational Consensus in Science and Society* (c. 1981) or Douglas Walton's *Arguer's Position: A Pragmatic Study of* Ad Hominem *Attack, Criticism, Refutation, and Fallacy* (1985). The workaday principles of assessment in moral or scientific matters constitute a "rhetoric of inquiry," for which see Nelson et al. By saying they "constitute" a rhetoric, I mean merely that a rhetorical and literary approach will probably reveal more about a field, seeing it more clearly and understanding its workings better than the philosophical approach hitherto popular and embodied in the title of this journal.

But the older and philosophical approach does not, therefore, have zero marginal product. It, too, contains arguments, which in some rhetorics may effectively persuade. Philosophy is embedded in rhetoric. And to apply the rhetoric, it's hard to see how the usual talk about Epistemology persuades.

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