

Pyrhonian Challenges

My official task is to comment on Professor Amico's argument against the "unassailability" of Pyrrhonian skeptical arguments. I am no expert on Pyrrhonian skepticism, but I wonder, first, whether the skepticism that Amico is challenging is really Pyrrhonian. Rorty, for example, has suggested that Pyrrhonism was concerned to show that we could know nothing with certainty and that it "had been troubled principally by the `problem of the criterion'--the problem of validating procedures of inquiry while avoiding either circularity or dogmatism." Descartes, he suggests, thought he solved this problem but in trying to do this--with his doctrine of "clear and distinct ideas," he created a new kind of problem: "the problem of getting from inner space to outer space--the `problem of the external world' which became paradigmatic for modern philosophy."¹

The Pyrrhonists which Amico challenges seem to be what Rorty calls "veil of ideas" skeptics. Against them, Amico convinced me early: As Marvin Farber taught us, there is no such animal as a presuppositionless philosophy. Accordingly, if it is the case that it is required that the Pyrrhonian skeptic "makes no assumptions or presuppositions," then as Amico argues, this philosophy is incoherent.

But this will hardly be satisfactory to "philosophers." Since (as the "philosopher" says) our access to anything depends on perception, memory, etc and since there is no way to know whether these "decisively influence or create what is accessed"--R.B. Perry's "ego-centric predicament"--skeptical objections regarding knowledge of "conceiving independent reality," either that it exists or that it is truly represented by us are always possible. Thus, even if the common person is sublimely realist, "philosophers" can see that Pyrrhonians can counter that a solipsism of the present moment is an alternative.

This leads to my main point: The argument between Amico and his Pyrrhonist is an absolute paradigm of Marvin Farber's distinction between problems which are "methogenic" and those which are "empiriogenic." It is also a paradigm of Dewey's distinction between philosophy as a "device for dealing with the problems of philosophers" and philosophy as a method, cultivated by philosophers for dealing with the problems of people."²

Epistemological arguments are *usually* philosopher's problems. They are *usually* a methogenic, not empiriogenic. Rorty is correct in holding that since it was widely believed that *something* had to legitimate the new science, epistemology became the core of the fairly recent demarcation of philosophy and science. He is right also that metaphysics then had to be something which emerged out of epistemology rather than vice versa. This is part of our history--the Western tradition of philosophy and science. Like it or not, we have it. (There are other traditions which do not.)

But it doesn't follow that we need to reproduce *its* problems or as Rorty would seem to have it, to throw up our hands. Dewey did not. Instead, he tried to shift ground. For him, there was no problem of going from "inner space" to "outer space," so he had to redefine "experience." Epistemology became "inquiry;" "truth" became warranted assertability and "knowledge," he insisted, was best understood as the product of competent inquiries. In effect, he agreed with that version of Pyrrhonism which accepted that we could have no certain knowledge of anything and that the genuine problem was "the problem of validating procedures of inquiry while avoiding either circularity or dogmatism." This leaves unanswered the question: how we are go about this? For me, a naturalistic epistemology is the only game in town; but having said this, there remain a great many serious philosophical problems.

To begin, it is not at all clear that the current defenders of naturalistic epistemology have a sufficiently robust notion of the social. This is a legacy of ruggedly individualistic traditional

epistemology, a legacy which, it is critical to see, also profoundly effects a great deal of work in current cognitive psychology.

This residue is clearly in Quine and is suggested by the attention given to "cognitive faculties" by many who (now) identify themselves as pursuing a naturalist epistemology. Here I would include "internalists" and "reliabilists" like Lycan and Goldman, but also "externalists" and "naturalists" of various stripes, for example, Kornblith and Philip Kitcher. On these views, the social is not denied; but it enters only as regards "the social organization of knowledge"--with Merton identified as having authored "the most important twentieth century work."³ I can here only hint at what is at issue.⁴

The Karam utterance, "I see a kobity now," involves language and language is preeminently social. The utterance parses the world in a way very different than our way and in this world, it makes very good sense. Indeed, not only is the cognitive order reproduced by social mechanisms which authorize and stabilize belief, but even perception is conceptually infected, enormously complicating the problems of a cognitive psychology. That is, versus Kornblith, "cognition is social all the way down."

Kornblith denies this but paradoxically acknowledges also that "the culturally transmitted conceptual repertoire" will "vary dramatically from culture to culture." Something else must be going on? Evidently, since "in prescientific cultures...much of the culturally inculcated conceptual structure serves as a vehicle for misguided belief and unreliable inductive inference," for Kornblith, as for mainstream sociology of science, the "social" can only explain error.⁵ Presumably, *our* cognitive structures "fit" the world; the Karam's would except for the distorting effects of the social?

This Euro-centric epistemological individualism will not satisfy my Pyrrhonist: The problem is not whether there is an external world causally implicated in my perception, whether it is structured, or whether I am hallucinating, but what licenses the conceptual scheme which allows me to say to the Karam that he is wrong: He is really seeing a cassowary.

There are two problems. First, it is not just that we have a long way to go before we have an understanding of "cognitive processes," but that much current work seems to me to be utterly wrongheaded. Some of this, e.g., information processing models, assumes that "information" comes prepackaged for processing by the organism, but this begs a central question: what counts as information. For many of these theories, "information" is already coded, perhaps so that it already includes "predicates." Other well established work in concept-formation has been shown to be utterly irrelevant since this work examined "conceptual attainment with the perceptual-abstraction phase bypassed."⁶ Fodor, drawing on Meno's Paradox, famously "solved" the problem with an innatist theory, holding that "one cannot learn [i.e., acquire by a cognitive process] a conceptual system richer than the conceptual system one starts with, where learning is a process of hypothesis formation and confirmation."⁷

But we surely are capable of generating new concepts and as surely, we are inductive learning machines *of some sort*. Evolution surely gave us discriminative and comparative capacities, the basis of judging, predicating and subsuming. But it is only with social evolution, with language, that we could have concepts. Even in terms of Gibson's work the world which we directly experience is "coarse grained," giving us as Kant and James had it, "things," and spatial and causal relations.

More generally, much cognitive psychology assumes the basic premises of recent empiricist philosophy: There is some sort of "given" and the only relations between contents of cognitive states are logical functions which hold only between abstract predicates which "represent" what is given. Inference is the regimented concept of the logician who provides the governing models of a rational process. Once this is done, of course, reliabilist theories become plausible. We begin with a true belief--"I see a cassowary"-- and examine the cognitive processes which produced it. For these reasons, of course, Goldman is correct that his "epistemics" is "significantly continuous with traditional epistemology."⁸

It is just here that Dewey provides a powerful and interesting alternative. First, his "psychology" is robustly social.⁹ But even more interesting for me is his theory of inquiry--his substitute for "traditional" epistemology. Although few have seen this, Dewey provided a radical (and accordingly) much misunderstood "logic."¹⁰

Thomas Burke says, "Dewey's conception of inquiry has to be understood not so much as cognitive problem solving but more generally in terms of an adaptive stabilization propensity of organism/environment relations." This starting point led Dewey to totally reject the taken-for-granted Frege/Russell conception of logic assumed by all empiricist philosophy (and subsequently by "cognitive psychology") and to refashion "inference," "propositional content," "kinds" and other central terms in mainstream logical theory. Thus, the logicians concept of "inference" is an abstraction from the more elementary but very much ill-understood capacities of "organisms" to handle "information" which does not require human language.¹¹ Indeed, as Hacking has said, deduction and induction (as understood by logicians) "play little role in the scientific method, no more than the once revered syllogism."¹² Burke summarizes the upshot of this:

The complexity of Dewey's logical theory as a whole is due to the fact that there is more to consider than simply comparing sentences against facts. His focus on inquiry and experience, his reformulation of the notion of facts in information-theoretic terms, his relatively complicated taxonomy of propositions (as distinct, moreover, from judgements), and so forth are all part of an attempt to explain (1) what it means to say that a statement about how things are may or may not correspond to how things actually are, when at the same time, (2) it is not possible to step back and treat this correspondence as it were a matter of comparing the statement against reality (p. 240).

I said that I saw two problems. The other is: How do we justify "procedures of inquiry while avoiding either circularity or dogmatism?" Internalists, reliabilists and, if I am right, most of the folks in the present volume are either dogmatists or they beg the question. Essentially, they either assume that some privileged beliefs are true or they assume that something vaguely identified as "science" yields truth.

Dewey gives us some options. First, since for Dewey's reliabilism, truth (understood as correspondence) was not at issue, he could reject JTB theories *and* a reliabilism which sought to gain truth. For him an inquiry succeeds only if "those existential consequences which, in virtue of the operations existentially performed, satisfy (meet, fulfill) conditions set by occurrences that constitute a problem."¹³ End of story.

This will not, however, satisfy everyone, especially if we apply the Pyrrhonist test. Did not Dewey assume that something like at least what people assume are the methods of science were

preferred? I doubt it. But even if he did, he did not, I think, crassly assume that *only* science produced knowledge or that it *always* produced knowledge. Although strong programmers have usually been read as vicious relativists who offer nothing in the way of solving the problem of judging belief-generating practices, it is just here where their work joins with Dewey's.

Some of us have lost our faith in modern science--exactly because, I would argue, it is not the engine of human liberation which it was once thought to be.¹⁴ Big Science--industrialized science--is a technocratic science under the control of the few in satisfaction of their own interests. Similarly, social science does more to obscure our social world than to illuminate it. When, for example, we are told by the nation's leading criminologist that we do not know understand the causes of crime or poverty because crimes are committed by people of all sorts and people of all sorts are poor, we can be sure that science is working in the service of power.

Given this, there could no be blanket justification for the practice of "science": there are different sciences operating in different social contexts with different goals, different methods, and different problems. Some of these, for example, those aimed at prediction and control, have little to do with truth in anybody's sense. It may be that if we restrict our concerns to the ideal of a pure science aimed at neither prediction nor control but at understanding, we could argue, as Rom Harre¹⁵ has done, that science depends upon a particular moral order and that familiar methodological rules, e.g., "consider all the evidence" are really moral rules. Unfortunately, as serious sociology of science shows, the social conditions for realizing this moral order are far from ideal. What we more likely have is "shoddy science," "entrepreneurial science," "dirty science," and "reckless science." These are indeed problems worthy of our concern.¹⁶

Peter T. Manicas
Sociology/Liberal Studies
University of Hawai`i at M_noa

Endnotes

1. Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979), pp. 139f.

2. In this essay, which introduced this distinction, Dewey remarked that "I should not anticipate difficulty in showing that this doctrine [that the existence of the past...of a decently stable world and of other selves] is, dialectically, a mass of inconsistencies" ("The Need for a Recovery of Philosophy," *The Middle Works* (Carbondale: Southern Illinois Press, Vol. 10, p. 37).

I can think of sociology of knowledge arguments which would explain the persistent tendency of philosophers to reproduce themselves: One may be reminded of Russell's remark that we are engaged in teaching people to be professors of philosophy so that they can teach people to be professors of philosophy. And, of course, it is better than working for a living!

3. Philip Kitcher, "The Naturalists Return," *Philosophical Review* 101:82 (1992).

4. The so-called "strong programme" departed from mainstream Mertonian sociology of science exactly because it did not assume that science produced knowledge. Strong programmers leave the question of the truth-status of beliefs generated by the sciences to epistemologists. This was, perhaps, a big disingenuous since, plainly, they believed that they were doing science and that it was a worthwhile effort. Perhaps because critics have been terrified of their relativist implications, these programs are widely misunderstood by the very few Anglo-American philosophers who have even considered them. See Peter T. Manicas and Alan Rosenberg, "Naturalism, Epistemological Individualism and 'the Strong Programme' in the Sociology of Knowledge," *Journal for the Theory of Social Behavior* 15: 76-101 (1984), and "The Sociology of Knowledge: Can We Ever Get it Straight," *ibid.* 18: 51-76 (1988).

Phillip Kitchner's "Contrasting Conceptions of Social Epistemology" (in Schmitt, *op. cit.*) is perhaps the most serious effort at criticism. A powerful rejoinder is provided by Helen E. Longino, "The Fate of Knowledge in Social Theories of Knowledge," also in Schmitt.

5. Hilary Kornblith, "A Conservative Approach to Social Epistemology," in Frederick F. Schmitt (ed.), *Socializing Epistemology: The Social Dimensions of Knowledge* (Lanham, Maryland: Rowman and Littlefield, 1994), p. 98.

6. David Kelly and Janet Kruger, "The Psychology of Abstraction," *Journal for the Theory of Social Behavior* 14: 43-68 (1984). Compare here Chomsky's critique of Skinner's *Verbal Behavior*.

7. J.A. Fodor, *The Language of Thought* (New York: Random House, 1975). Richard Shweder was led to conclude that much psychology has "the look, taste and smell of a Platonic undertaking" (*Thinking Through Culture: Expeditions in Cultural Psychology*, New York:

Cambridge University Press, p. 77).

Versus Shweder, psychology can be an "autonomous" science if it restricts itself to the hugely important task of providing an understanding of our powers as humans: how we learn, the mechanisms of human perception, language acquisition, memory, etc. This research program will yield nothing about the performances (acts) or realized competences of persons since these always involve both biography and social factors. See Joseph Margolis, Peter T. Manicas, Rom Harre and Paul F. Secord, *Psychology: Designing the Discipline* (Oxford: Basil Blackwell, 1986) and my *A History and Philosophy of the Social Sciences* (Oxford: Basil Blackwell, 1987), Chapter 14. As noted, we need also to reject the assumption that psychology and naturalized epistemology: 'S knows that p iff...' should be concerned with the beliefs of particular individuals, e.g., those of Hitler. Imagine the biography that would be necessary to do this!

8. Alvin I. Goldman, "Epistemics: The Regulative Theory of Cognition," in Kornblith, *Naturalizing Epistemology* (Cambridge, Ma.: MIT Press, 1985), p. 217.

9. He writes: "...the whole history of science, art and morals prove that the mind that appears *in* individuals is not as such individual mind. The former is itself a system of belief, recognitions, and ignorances, of acceptances and rejections, of expectancies and appraisals of meanings which have been instituted under the influence of custom and tradition (*Later Works* 1:170).

10. See Thomas Burke *Dewey's New Logic: A Reply to Russell* (Chicago and London: University of Chicago Press, 1994). Among the many treasures of Burke's account are his Deweyan criticisms of representational and computational models of cognition and his defense of an "ecological psychology" as inspired by Gibson.

11. As Patricia Churchland insisted, we need to reject the idea that "where there is cognition, there is also linguistic representation, no matter how far phylogenetically or ontogenetically, the creature is from overt language" ("A Perspective on Mind-Brain Research, *The Journal of Philosophy* 57: 189 (1980).

12. Ian Hacking, "Language, Truth and Reason," in Martin Hollis and Steven Lukes, *Rationality and Relativism* (Cambridge: MIT Press, 1982), p. 57. Similarly: "It matters not what truth is, when we employ the mechanics of the model theory of modern logicians. Their machine [and so too the cognitive machine] works well so long as we suppose that the class of sentences that have truth values is already given' (*ibid.*).

13. "Propositions, Warranted Assertibility and Truth" (1941), in *Later Works* 14: 168-188).

14. As early as 1893, Dewey fully acknowledged this. In agreeing with Ernest Renan's *The Future of Science*, he wrote: The forty years since Renan wrote have not done much to add to the human spirit and the human interpretation of the results of science; they have rather gone to

increase its technical and remote character." Indeed, "Renan does not seem to have realized sufficiently the dead weight of class interest which resists all attempts of science to take practical form and become a 'social motor' (*Early Writings* 4: 16).

15. Rom Harre, *Varieties of Realism* (Oxford: Basil Blackwell, 1989).

16. See Jerome Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford University Press, 1971). We have shoddy science when published papers are not read; entrepreneurial science allows contractors to establish capital intensive enterprises in which researchers lose all independence and everyone else is denied access. Ready access to millions of dollars aimed at producing some specific technical power produces reckless science. We have dirty science when funded research aimed at achieving understanding are converted into technologies for state purposes of destruction, control or manipulation.