

## Naturalizing Epistemology: Reconstructing Philosophy

### Introduction

It is surely plausible to think of the histories of humankind as a series of discontinuous, and sometimes continuous, intersecting movements marked by accidents, some benign, some fortuitous, and some disastrous. In this regard, if nothing else, history is radically contingent—even if looking back, we can often provide altogether satisfactory explanations of what happened and why? One such legacy is the intertwined conceptual and institutional legacies of science and academic philosophy.

Yet not all is well as regards these. Dewey could lament that we had failed to replace old habits of thought with more scientific ways. This was one aspect of the reconstruction in philosophy for which he called. We wonder, not unreasonably, whether the authority of science was but Western provincialism, the rationale for the imperialist obliteration of nonWestern cultures. On the other side, while Dewey was aware that science had been misappropriated and misapplied, he remained optimistic that this could be changed, that democratic processes could be brought to bear on expert claims to authority. This, too, was an aspect of his call for reconstruction. Yet, as above, Dewey's hopes strike us as naive. Wholly disjoined from experts, we stand in terror of their so carefully considered decisions. What, heaven help us, will be the unintended consequences of genetic engineering or a confrontation in Saudi Arabia? If the nineteenth century had Frankenstein, we have energy-obsessed Dr. Strangeloves.

This chapter pursues the idea of reconstructing philosophy; thus, if very indirectly, of reconstructing culture. With Dewey as both guide and foil, the focus is on the implications of the current debate over the effort to naturalize epistemology, that is, to study knowledge scientifically. Dewey was surely correct that we need an alternative to dogmatism and to skepticism; but as perhaps Dewey did not clearly see, we cannot take science for granted. A second goal of this chapter, then, is to raise some questions both about current scientific practices and our understanding of these.

### The Epistemological Problem

We can begin with Barry Stroud's critique of Quine's influential essay, "Epistemology Naturalized."<sup>2</sup> Quine argues that naturalized epistemology is "the empirical study of a species of primates, or, in the particular case, of an individual human subject in interaction with his environment."<sup>3</sup> Thus, now quoting Quine:

This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in certain frequencies, for instance—and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history?<sup>4</sup>

The story continues: we observe the subject as she interacts with her environment. Given then that we know her environment and have an adequate psychology, we then explain her "output," seeing that, in the fullness of time, what she says is true.

But, of course, the situation just described is not the situation of our naturalizing epistemologist, since, of course, like our knower, she is (as everyone!) utterly denied that independent, theory-neutral access to the world which could be the only basis for determining whether inputs from it ever result in outputs which are true. Stroud concludes that Quine simply fails to address what he takes to be the traditional question of epistemology: How do we know that the external world is what anybody says it is?

This problem is not the problem of whether there is an external world or, for that matter, whether it has some structure. As Peirce and Dewey insisted, we can call into question any particular version of how the world is, but Cartesian skepticism cannot be reasonably motivated.<sup>5</sup> But as the foregoing seems at least to show, one can assume an external and structured world, the method of science, and still ask if what is presumed to be known is known.

Dewey might add there that this presumes an absolute conception of reality.<sup>6</sup> On this (commonsensical) view, reality means reality as it is independently of you and me, independently of what it is known as. My skeptic demands that we show that knowledge of this reality is possible. If the only knowledge we can have is from some viewpoint, how can we know whether it—our or some other—is valid? That is, given an absolute conception of reality, it would seem that we are forced to accept a relative conception of knowledge. It may be, of course, that we can justify some viewpoint. Perhaps (as I argue below) there are modes of fixing belief which should be preferred. Such a relationalism could then be contrasted to relativism: understood as the thesis that no viewpoint, no mode, can claim privilege over any other.

It may be doubted that we need an absolute conception of reality. But we surely do—if we want to avoid relativism and to anchor our fallibilism.<sup>7</sup> Even if we can privilege some mode of fixing belief, we will need to aspire to the ideal of grasping the world as it is, independently of what you and I might believe. The problem of historical knowledge is, perhaps, the clearest case. We must acknowledge that we shall never have more than a fragment of the possible evidence and that alternative histories are always possible. But surely what transpired transpired independently of these. It is just this which grounds the limits of all perspectives and thus our fallibilism.

More, we need an absolute conception of reality if criticism and persuasion is not to collapse into sophistry, to be merely a struggle to win opinion. As rhetoricians know, truth-talk plays a vital role in argument, persuasion, and criticism. Indeed, if we could dispense with the conception of an absolute conception of reality, truth-talk might be dispensable, replaced by pragmatic "works/does not work" or "predicts/does not predict." Language (and theories about the world) are surely (our) tools for coping with our world; but for social animals, they could not serve if they did not have a rhetorical function.<sup>8</sup>

There is another form of answer to Stroud. We don't need independent access to the world if we can assume there is a necessary connection between some method, say, the method of science and truth. Thus, with persistent application of our method, our (un-Peircean) individual, given a (Peircean) fullness of time, will arrive at truth. But even this

act of faith does not help us. Since we will always lack independent access, we can never know whether we have arrived!

Here we can pause to consider, even if too briefly, an argument put forward by Michael Friedman. He points out (rightly) that there is no necessary connection between confirmation and truth and that what traditional philosophy of science has to offer on the relation cannot be sustained.<sup>9</sup> In particular, he argues that if scientific method (or any other) is to show that it achieves (or even approximates) truth, the Tarskian theory of truth (shared by all traditional candidates) must be supplemented to include a causal theory of reference. Since our methods cannot guarantee success, "we have to know facts about the actual world if we are to know which method is best; and we have to know facts about the actual world to know even that any given method has any chance at all of leading to truth!"<sup>10</sup> To do this in a nonviciously circular way "we need general laws connecting physics and psychology [sic] with the theory of truth; and it is precisely this kind of generality that a theory of reference tries to provide."<sup>11</sup>

There are two fairly obvious objections. First, even given these laws, it is by no means clear what scientific method is and thus what and how it is to be tested (of which more below). But, second, as Friedman says, we lack utterly such general laws. Worse, I believe that there is little reason to suppose that such are possible. To anticipate, Friedman's program seems, at least, to follow Quine's in being committed to an epistemological individualism.<sup>12</sup>

### **Epistemological Individualism**

There are, I believe, about nine (or forty?) ways into this. One way is to observe, with Dewey, that:

...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 in 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 (LW 1:170)<sup>13</sup>

The epistemological problem that is at issue here was surely propelled by modern science, but contrary to what Quine (and Dewey) imply, acknowledging this is no advantage for the naturalizing epistemologist. It is, indeed, because of modern science that, as William James rightly saw (versus Spencer), the epistemological problem is so intractable. Naive realism could (and does!) sustain epistemological individualism: if you don't believe there are red apples in the world, then just look and see! But if you take modern physical science and Dewey's remark seriously (as I think we must), then it is a rather gigantic system of belief, recognition, and the rest, which has been instituted under the influence of custom and tradition. Given this, how can we be so confident that our beliefs correspond to a world that exists independent of either you or me?<sup>14</sup>

This does not mean that I am deluded in saying "there is a red apple" when I see a red apple. That is not the issue. Plainly, science did not undermine our ordinary ways of thinking and speaking. When a G. E. Moore says, "I know that there are red apples," and

a neuroscientist says "The experience of red apples is the product of physical and biochemical transactions between something and us" and the skeptic says, "Nobody could know that there are red apples," the same words are being used differently.<sup>15</sup> What Moore and the rest of us say, even if true, is not decisive as regards either the epistemological or the scientific investigation of knowledge.

We began with Quine's psychological program; this is the appropriate point to refer to recent sociology of knowledge, to the so-called "strong programme."<sup>16</sup> Its key insight is what Barry Barnes calls "the naturalistic equivalence of the knowledge of different cultures."

Naturalism. . . implies the most intensely serious concern with what is real .... Everything of naturalistic significance would indicate that there is indeed one world, one reality, "out there"; the source of our perceptions if not their total determinant [that is, though not their total determinant], the cause of our perceptions being fulfilled or disappointed, of our endeavors succeeding or being frustrated. But this reality should not be identified with any linguistic account of it, or needless to say, with any way of perceiving it, or pictorial representation of it. Reality is the source of our primitive causes, which, having been presupposed by our perceptual apparatus, produces changes in our knowledge and the verbal representations of it which we possess. All cultures relate symmetrically to this reality. Men [sic] in all cultures are capable of making reasonable responses to the causal inputs they receive from reality—that is, are capable of learning. That the structure of our verbal knowledge does not thereby necessarily converge upon a single form, isomorphous with what is real, should not surprise us. Why should we ever expect this to be a property of our linguistic and cognitive capacities?<sup>17</sup>

Because, like Quine, Barnes and Bloor take science seriously, they believe (a) there is an independently existing world, but (b) they also believe with Dewey that human cognition is always socially mediated.

An example may here be useful. According to Bulmer, the terminal taxa of Karam correspond very well in approximately 70 percent of the cases with species identified by a scientific zoologist.<sup>18</sup> The cassowary is an interesting instance of noncorrespondence. Karam have the taxon "yakt" for birds and bats, but the cassowary is not placed in this taxon. Instead, it appears in a special taxon, "kobity," making it a nonbird/nonbat. For Bulmer, of course, this is an error, an error explained in terms of a Karam willingness (unlike us!) to allow culture to supercede "objective biological facts." The problem is not whether there is an external world nor whether Karam fail to see something which we call a "cassowary." Rather, the problem is: How can we say that reality is not parsed the way the Karam say it is?<sup>19</sup>

The idea that knowledges of different cultures is naturalistically equivalent is both a premise and a conclusion of strong program science.<sup>20</sup> Strong programmers are interested in understanding belief, and therefore, for scientific purposes, beliefs which we think of as rational—including accordingly, those which are fixed scientifically—must be

treated as on the same footing as all others. The belief "I see a Panda now" involves language. Hence social considerations are relevant. Just because our only access to a world is causal, and epistemological individualism cannot be sustained, a naturalistic epistemology interested in explaining knowledge must appeal to social facts. Not only do these enter into concept formation (enormously complicating empirical psychology), but we need to acknowledge that the problem of reproducing the cognitive order could not possibly be explained without a sociological understanding of the relevant social mechanisms, for example, how belief is authorized and stabilized.

On the other hand, because our best science implies that all we can have is a representation of reality, and because there is no way to measure any representation against reality-in-itself, we cannot escape a relationalism. But it does not follow from this that all truth claims are equally good. At this point, we can turn to Dewey.

### **Dewey's Program**

It is not perfectly clear what Dewey would say to a Karam defender of his system of classification. Who here has the truth? He might say that the question is badly posed. He might say that since "true" presupposes an absolutist conception that is neither necessary nor possible, both are right. Although the world is structured, it does not allow us to discriminate between contrary taxonomies. It does constrain these: A culture could not, for example, treat what we have identified as poisonous as foods, for their biology will not allow them to survive. But there is nonetheless plenty of room for alternative and contrary schemes depending on a host of alternative contingent factors regarding beliefs about the gods, the good, etc. On this view of the matter, there are too many truths and it is idle to suppose that any can be privileged.

There is an attractive aspect to this move. Given that peoples have different interests and different ideas about the gods and the good, we need to acknowledge that they may very well be able to justify their beliefs about the way the world is—however strange these may seem to us. There is, unfortunately, an unattractive aspect to this posture. Not only does it disavow an attempt to give any special credence to the claims of science (perhaps not such a bad thing?), but it disavows any effort to provide guidance about how we ought to go about finding out what to believe, including here, lest we forget, beliefs about what is good and right. I think that we can do better. So, too, did Dewey.

### **Dewey's Naturalizing of Epistemology**

Dewey wrote that "the methods of knowing practiced in daily life and science are excluded from consideration in the philosophical theory of knowing" (MW 10:37). Presumably, "the actual process of knowing;" involves "operations of controlled observation, inference, reasoning, and testing." While this seems true enough, it does not help us in the present instance. Surely, Karam do all these things even as they are arriving at a different taxonomy than ours. Are the Karam going about the business of inquiry wrongly? Perhaps what is needed is a more systematic attempt at discriminating the special features of successful knowing. Dewey set this as the goal of inquiry into inquiry.

Thus, he writes:

The position here taken holds that since every special case of knowledge is constituted as the outcome of some special inquiry, the conception of knowledge as such can only be a generalization of the properties discovered to belong to conclusions which are outcomes of inquiry. Knowledge, as an abstract term, is a name for the product of competent inquiries. (LW 12:16)

That is, by examining the outcomes of inquiries, we can discover why these are knowledge and, conjunctively, by examining inquiries we can discover what makes for competence.<sup>21</sup>

It is important to see what this program is and is not. Not only was Dewey not altogether clear regarding what is involved, but more troublesome, the program does not neatly join with work being carried on today in academic disciplines as they are generally practiced. Perhaps Dewey was off the mark, or perhaps the fault is with much current social science.

### **Inquiry into Inquiry: I**

It may be best to proceed indirectly and to begin by noting that Dewey's program is not akin to the psychologically oriented programs of Quine or, for example, William Lycan?<sup>22</sup> Quine has not said very much about the sort of psychology he assumes will explain how we know, but we may guess that it is some sophisticated version of behaviorism. By contrast, Lycan is very clear in his commitments to a Fodor/Dennett-inspired homuncularism, a currently fashionable version of cognitive psychology.<sup>23</sup>

But in either case, Quine, Lycan, and the psychologies they presume are epistemologically individualistic and Dewey's psychology was not. Moreover, these writers and the psychologies they want to include are committed to a logical theory which Dewey found to be misdirected.

Throughout his career, from his brilliant essay on the reflex arc, through the 1903 studies in experimental logic, to *How We Think* (1910), to his ill-understood *Logic*, Dewey developed a naturalistic theory of inquiry that totally went against the dominating and now taken-for-granted Frege/Russell conception of logic.<sup>24</sup> In this view, logical relations hold between abstract predicates and inference (deductive and inductive) depends on there being some sort of objective relation between propositions.<sup>25</sup> Because this assumption was a feature of what Dewey called "intellectualism;" he looked at the matter entirely differently. As 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."<sup>26</sup> This basic naturalistic starting point led Dewey to totally refashion "inference," "propositional content," "kinds," and other critical terms in standard logical theory. Thus, as I understand Dewey, "inference" is fundamentally a way of handling information which does not require human language. The logician's concept of inference is not, of course, to be abandoned; it is rather to be seen as a highly useful abstraction, regimented for

particular purposes.<sup>27</sup>

This is hardly the place to develop the radical implications of Dewey's revision. Continuing along lines just suggested, one example will have to suffice. Enormous effort in psychology has been directed at solving Meno's paradox: "If inputs require concepts to be meaningful, then concepts must precede 'inputs' as in nativism; but if concepts (to be at all useful in the real world) require 'input' for their content, then 'inputs' must precede concepts as in empiricism (either of the ontogenetic or phylogenetic variety)."<sup>28</sup> For example, behaviorist learning theory needs to assume that the organism has made the relevant abstraction if it is to be reinforced. But this assumes exactly what needs to be explained. On the other hand, recent cognitive psychology, by conceiving of mind as an information processing system, assumes that information comes sententially prepackaged, ready-made for use by the linguistically apt learner.

If I am correct we can now identify three obstacles to an adequate understanding of knowing: a pervasive intellectualism, an epistemological individualism, and third, a pervasive assumption, shared by the main contending views, that, as Kelly and Kreuger put it "the only relations between contents of cognitive states which makes a process involving those states a cognitive process are the sorts of logical functions used in classical experiments."<sup>29</sup> But, of course, on the standard view of logic, logical functions can hold only between abstract predicates. Meno's dilemma is then inescapable! Indeed, until psychology breaks from those philosophical dogmas which have formed it, we shall not have an adequate psychology of learning, and we shall not naturalistically understand knowing. Dewey's path, if I am right, was the right one.

But even if we accept completely Dewey's picture of inquiry (including a host of details yet to be filled in), this would not, of itself, respond to the problem of judging between the belief systems of the Karam and the Western zoologist. Presumably, Dewey's account applies to both. If only the zoologist gets it right, then something more is being assumed, likely that something called the method of science privileges the findings of the zoologist. But we have yet to see the argument for this.

### **Inquiry into Inquiry: II**

There is an entirely different program which is rightly construed as inquiry into inquiry. Dewey offered (as we noted) that "through examination of the relations which exist between means (methods) employed and conclusions attained as their consequence, reasons are discovered why some methods succeed and other methods fail." This might be understood as meaning that the task is not only to frame theory which describes and explains the general feature of inquiry, but consistent with this, to consider empirical science empirically. It might then be possible to generate warranted methodological rules, thus satisfying the demand that we be able to judge between contrary beliefs and belief systems. This version of a naturalistic program has had some recent advocates, among them, most outstandingly, Nicholas Rescher and Larry Laudan. There are, I think, three main features which distinguish this approach.

First, the Deweyan inspiration is in the effort to avoid a vicious circularity in

which one either justifies outcomes by assuming that means are competent, or warrants the means by assuming the truth of the outcomes. If as Rescher puts it, "justification is here an essentially twoway process—its results legitimate the method as proper and appropriate, and the method justifies its results as 'correct' " then one needs either to break the circle or to show that it is nonvicious.<sup>30</sup>

Briefly, Rescher argues that "any experiential justification of a truth criterion must pull itself up by its own bootstraps—it needs factual inputs, but yet factual inputs cannot at this stage already qualify as truths" "To meet this need, accordingly, Rescher appeals to "truth candidates;" "data which are no more truths than candidate-presidents are presidents. . . ." <sup>31</sup> Rescher then envisages "a feed-back loop" in which "the reasonableness of the over-all process. . . rests not only on the (external) element of success inherent in the factor of pragmatic efficiency, but also on the (internal) factor of intrinsic coherence and the mutual support of self-substantiation that the various stages of the whole are able to lend to one another."<sup>32</sup>

Laudan offers what he calls a "reticulated model of scientific rationality" which explicitly introduces values:

The reticulational approach shows that we can use our knowledge of the available methods of inquiry as a tool for assessing the viability of proposed cognitive claims. . . . Equally, the reticulated picture insists that our judgments about which theories are sound can be played off against our explicit axiologies in order to reveal tensions between our implicit and explicit value structures.<sup>33</sup>

For Laudan, fully in the spirit of Dewey, "axiology, methodology, and factual claims are inevitably intertwined in relations of mutual dependency."<sup>34</sup>

Second, both writers (Rescher explicitly, Laudan implicitly) reject a thesis (or propositional) pragmatism in which the problem is to vindicate particular truth claims. Instead, they opt for a methodological pragmatism in which the problem is to justify methods. Thus, "pragmatic considerations are never brought to bear on theses directly. The relationship becomes indirect and mediated; a specific knowledge claim is supported by reference to a method, which in turn is supported on pragmatist lines."<sup>35</sup> Beliefs arrived at with warranted methods may very well be false. The aim, however, is to find methods which are reliable in the sense that they answer to human purposes, critically assessed.

Thesis pragmatism is highly vulnerable to a wholly idiosyncratic mode determining what counts as warranted assertibility. This is, of course, a long-standing objection to pragmatism. Methodological pragmatism offers hope since beliefs are warranted only insofar as they are the outcome of an explicit method which has been warranted independently of this or that particular belief. As Rescher points out:

Considerations of the suitability and effectiveness of methods introduce an inherently rational orientation which serves to assure the logical properties.

Moreover, methods are intrinsically public, interpersonal, and communal. A method is not a successful method unless its employment is generally effective—otherwise we are talking about a knack or skill rather than a method. A skill can only be shown, it cannot be explained .... This line of thought indicates the fundamentally social dimension of methods. ... They can be examined and evaluated *in abstracto*, without any dependency on particular practitioners.<sup>36</sup>

These are certainly desirable features of this program, even if as I shall suggest, instead of methods, we are better advised to try to warrant practices. But we should first notice that if the program carries, we will have escaped subjectivism, we will have warranted our method and thus beliefs which are determined by means of these methods, but we will not have secured truth. But, of course, Dewey's shift to warranted assertibility was a rejection of the search for truth (understood, as always, in the absolutist sense).<sup>37</sup>

### **Excursus: Truth about the World and Moral Truth**

It will be important to notice the bearing of the foregoing on the question of moral relativism. In my view, this is surely the most important of the troublesome questions raised by relativism. Plainly, I cannot pursue this here. Yet, it seems to me that it is just here that the foregoing is most helpful. The reason is clear enough. The skeptical objection forecloses the possibility of securing a perspectively neutral truth about a world which exists independently of you and me. But since on naturalistic grounds, our ideas about what is good and right are our ideas, the skeptical objection has no force. All that we need as regards questions of the good and the right is warranted assertibility. Moreover, in this context, the public, interpersonal, and communal aspects are fundamental. While the effort to secure ever inclusive representations of the external world cannot secure truth about it, the effort to secure ever inclusive goods is exactly what is called for as regards moral matters.

### **Methodological Pragmatism**

There is a third aspect to methodological pragmatism. It presumes that an empirical study of science will yield clarity about aims and methods, and that there is a way to reflexively test methods against aims, once identified. As far as I know, Laudan (and his associates), have been in the forefront of actually engaging in such research.<sup>38</sup> But I think that on this count, there are some very difficult problems.

First, there is the abstraction science. It is easy to suppose that although there are manifest differences in the sciences, the term, "science" is meaningful because the sciences share in goals, for example, prediction and control, and/or because there is something called "scientific method," again, usually defined in terms of a series of abstractions about the formation, deductive elaboration, and testing of hypotheses. Dewey was, I believe, utterly uncritical in this.<sup>39</sup> Laudan acknowledges that goals do differ and that, pertinent to this and to subject matter, methods (not merely techniques) may vary. Still it would seem that an adequate empirical picture would show some fundamental differences, not only in the sites and goals of the practices of the sciences, but in their methods and standards as well.<sup>40</sup>

Consider first the idea of the goals of these practices. Even a cursory examination would show, I believe, that there are at least four fundamentally different goals currently operative in the sciences.

1. Description: for example, ethnographic work in anthropology; quantitative research in economics or demography; much geography; and taxonomic work in botany and zoology (motivated, I believe, by very different goals than the pre-scientific taxonomy of the Karam!).
2. Prediction and control: behavioral social science, most psychology, meteorology, the engineering sciences, and applied sciences.
3. Understanding: basic science, including work in space/time theory, quantum mechanics, evolutionary theory, some (but surely the smallest part) of theoretical work in psychology and the social sciences.
4. The explanation of concrete events: history and the historical sciences (including here, some social science, some geology, some evolutionary biology, some psychology).

It has been easy to collapse these cognitive goals. For example, by means of the idea that the discovery of laws is the goal of science, it has been easy to believe that explanation, understanding, and prediction are of a piece. But while the point cannot be pursued here, it is easy to show that these are conceptually, and in practice, distinct aims.[These points are elaborated in my *A Realist Philosophy of Social Science (2006)*.] Empirical examination of practice would show, I believe, that those practices which aim at prediction and control (implicitly or explicitly) offer nothing in the way of understanding or in the explanation of concrete events. For example, behaviorist psychology gives no understanding of learning; and it cannot explain the most elementary concrete act, for example, my response, "fantastic," to seeing Guernica for the first time. But—and this is not to be minimized—behaviorist psychology has been an effective tool for manipulation and control.

But the point of this sketch is not to settle issues, but to raise questions for the empirical program that I have called "Inquiry into Inquiry: II" Speaking now within its frame of reference, if the goals are different, then we can expect the methods to be different. For any particular goal, there still will be methods that are most effective and suitable. And we can still endorse the basic Deweyan effort to self-referentially bootstrap. But not all these goals will be pertinent to the problem with which we began. The skeptical objection is plainly irrelevant if the aim is prediction and control. Moreover, it is very easy to justify science as the preferred mode if prediction and control is the goal. Indeed, this is a major motive for continuously attractive instrumentalist theories of science. If, however, one is interested in understanding or in explaining what happens, then inquiry into the practices of sciences with those aims will be pertinent. Laudan and his colleagues look at the "basic" natural sciences (despite their antirealism). If they had looked at most mainstream psychology, indeed at most mainstream social science, things would look very different.

But let us assume that the intention is to get an "empirically well-grounded

picture" of those practices which in fact (and not merely in intention) aim at giving an account of how the world is. Of course, it will not do, as Laudan has himself so strenuously insisted, to accept those descriptions of science that are written with manifest assumptions imported from the two dominating traditions of twentieth-century philosophy of science. These studies cannot count as tests because they are self-authenticating. Nor can we, uncritically, accept what practitioners say are their methods (or for that matter, their standards and goals). As Einstein remarked, "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: Don't listen to their words, fix your attention on their deeds."<sup>41</sup> We have the best chance of getting some understanding about what they do if we can study activities directly. For standard historiographical reasons, things get much more difficult when we consider past practices. Indeed, in the face of these problems, there may be a temptation to assume methods and then to assume that they determine outcomes. We then enter history less problematically, with an eye merely to these.

Laudan's program risks this. Thus, he seems (at least) to begin with hypotheses regarding methodological rules which derive from philosophies of science. The idea then is to enter into history and seek either confirmation or disconfirmation of these. But there are now two additional problems. First, if as Laudan asserts, science changes, it is not clear how much generalizing will be possible<sup>42</sup> and thus, to what degree we can regard conclusions drawn from such inquiry as tests. For example, consider but the institutional differences between Newton's scientific research and the big science of today. Assuming (what is likely contrary to fact) that the goals are comparable, can we be confident that abstracted methodological rules effective then would now be effective?

Second, as recent studies surely show, agents making decisions in science are complexly affected. Not only are they capable of self-delusion (like everyone else), but rules, even if they are crisply formulated and form a consistent set, need to be applied concretely. This is hardly to say that methods are irrelevant. Rather it is to assent to Kuhn's view, rejected by Laudan, that methodological criteria rarely if ever determine choices between rival theories. As Kuhn (and strong program writers) have insisted, this is not to deny rationality; it is to affirm that rationality is both changing (as Laudan admits) and concrete, exactly in a more Deweyan sense that we cannot explain choices by subsuming them under rules.

One thrust of my argument has been against philosophers (and those influenced by these) who, despite the best intentions, have been unable to free themselves from the shackles of traditional epistemology. Another thrust has been to sympathize warmly with pragmatic approaches, but to suggest that among the most outstanding of these, there are serious problems to be faced. Before concluding, I summarize:

First, if we are to understand knowing naturalistically, we need to rethink, in Deweyan terms, the psychology and logic of knowing. This will require, if I am correct, some important changes in the mainstream practices of empirical psychology.

Second, since knowledge is inescapably a social product, we need to welcome

the efforts of recent sociology of scientific knowledge. Our "naturalism" cannot be "half-hearted."

Third, having achieved a better understanding of the production of knowledge, if we are to seek warrant for beliefs (or to prescribe norms for belief), we will need to embrace a Deweyan approach to the fact/value dichotomy. We need to acknowledge, straight out, as Sleeper puts it, that since "all judgment is practical... there is no gulf between intellectual and practical judgment."

Fourth and finally, instead of trying to warrant methods, we will be better advised to try to warrant practices. I conclude with a sketch of what I mean by this.

### **The Warranting of Practices**

Practices are, roughly, ways of doing. Practices include the beliefs of practitioners, the tools they use, their explicit goals, and much else besides. Practices are institutionalized (structured) activities, activities which presuppose habits in Dewey's sense, dispositions which carry the legacy of training and custom. The shift from methods to practices has consequences:

First, we will not be stymied if, as seems to be the case, much of what is known by practitioners is not formulable in terms of rules, but is tacit, craft-like, and learned at the side of experienced mentors. One learns how to use the tools, not merely the instrumentation, but the special languages, for example, the mathematics, and the standards for employing them. One learns what counts, what are the pitfalls, what are the ongoing standards of adequacy. Indeed, understanding these is precisely what would count as understanding a practice.<sup>43</sup>

Second, the shift to practice allows us to acknowledge that structured activities have unintended consequences. This includes not merely the uses to which basic work can be put, but the potential that outcomes may be surprising and hence not subject to control, and that intentions may be frustrated and transformed. Third, as part of the picture, we can include the real possibility that actors engaged in a practice can have false consciousness: they may have beliefs which are essential to the practice in the sense that if they have believed otherwise, they would not do what they do, but these beliefs might be false in the sense that actors may not fully understand just what they are doing. For example, they may believe that they are Popperians or instrumentalists when in fact they are not; they may believe that they are explaining when they are merely controlling; they may believe that their research is uncontaminated by interests foreign to their aims, etc.

A fourth advantage of this shift is that it allows us to incorporate as critical the fact that scientific practices are enmeshed in, effecting and effected by, a host of other practices: economic, educational, and political. Thus, the political economy of "big science" is critically relevant to understanding how its problems get defined and how it approaches and resolves them.<sup>44</sup> The issue is not merely that the goals and methods of the practice of big science are not autonomous, but that nonscientific factors are playing critical roles in constituting these practices.

Finally, we can be sensitive to the fact that scientific practices are very differently constituted, not merely between and among disciplines, but across time. Given this, a global defense of science may not be possible. On the other hand, we do not need a global defense. We need only to learn by inquiry what it is that makes a practice warrantable.

### **Harré's Justification of Theoretical/Experimental Physics**

With these considerations in mind, there is at least one recent work to which we can point. Rom Harré is a trained physicist/philosopher who takes fully to heart the idea that (a) we had best look at scientific practices, (b) that a defense of practice in theoretical/experimental physics is not, *tout court*, a defense of science or of scientific method and (c) that such a defense must recognize the skeptical challenge with which we began. Plainly, this is not the place to detail Harré's important work, but I believe that (with some minor emendations), it is entirely congenial to the views of this chapter.

Begin with (c). Harré rejects what he calls "truth realism;" roughly that a belief is true if and only if it corresponds to reality. Sensitive to arguments from Hume to Laudan, he defends "referential realism;" roughly, the idea that "some of the substantive terms in a discourse denote or purport to denote beings of various metaphysical categories such as substance, quality or relation, that exist independently of that discourse."<sup>45</sup> In terms of our earlier discussion, not only is there an external world, but given what we know, the most plausible causal theory of perception is Gibsonian. That is, "while one must concede that there could not be psychological laws which explain how someone came to see a pencil, it does not follow that there could not be psychological laws which explain how someone came to see long, thin things, causal sequences, and other generic perceptibles."<sup>46</sup> On this view, these are natural "affordances," possibilities of action ecologically offered to naturally evolved species and found in the exploration of the ambient array. But plainly, this story will not suffice to explain our relevant taxonomies since, as above, Karam see (mediately) kobity and we see birds (or if we are birdwatchers, we see cassowary).

We need to make room for the social component of knowledge. Harré exploits Dretske's explication of "seeing that..." Thus,

1. S sees b.
2. The conditions under which S sees b are such that it would not look the way it does look, say L, unless it were P.
3. S, believing 2, takes b to be P.

Here, b is a Gibsonian invariant; condition 2 introduces S's corpus of prior belief. That "b is P" is knowledge, but, plainly, it is relative to the corpus of beliefs held by S, and there is no way to find some original, terminal, or foundational belief! We have found a toehold on the world, but we have not secured an absolutist conception of knowledge. Nor have we secured science.

We can imagine a discourse, Harré calls it Realm I discourse, which made

reference only to the states and relations of beings known in actual experience (the heaven of empirical realisms!). Could such a discourse sustain a science? No doubt, human communities have put considerable attention on classifying beings in Realm I discourse, but as is now sufficiently clear, the boundaries "which serve to maintain discrete groupings in any human classificatory practice cannot be justified without reference to unobservable properties and structures of the beings in question."<sup>47</sup>

Harré's problem is now clear. Can he justify theoretical/ experimental physics as a preferred mode of fixing belief about the external world? Grasping fully the idea that "the science we consume, so to speak, is the final product of the complex interplay of social forces and cognitive and material practices" (and not the product of a "logic engine"), he argues that one must acknowledge that scientific communities control their products "by the informal yet rigorous maintenance of a moral order."<sup>48</sup> Indeed, on Harré's view, a great deal of the best work by philosophers of science is most usefully understood as sketching an ideal moral order, not an ideal (or still less, real) epistemic order. On this reading, the (epistemic realist) manifesto, "Scientific statements should be taken as true or false by virtue of the way the world is" as a moral principle becomes: "As scientists, that is, members of a certain community, we should apportion our willingness or reluctance to accept a claim as worthy. . . only to the extent that we sincerely believe that it somehow reflects the way the world is." Similarly, the idea that we should seek falsification cannot be sustained as an epistemic principle. But it has manifest merits as encapsulating moral injunctions: for example, "However much personal investment one has in a theory, one should not ignore contrary evidence."<sup>49</sup>

For Harré, then, "facts" are socially constructed, but not only are they not constructed from whole cloth (the burden of referential realism), but to the extent that the ideal moral order is functioning, then the results are to be preferred.<sup>50</sup>

### **Why We Are Epistemically in Trouble**

Harré appeals to empirical studies of scientific practices to identify what, pertinent to the problem of knowledge, is distinctive of these and, then, why, ideally speaking, they should be preferred. Speaking as a naturalizing epistemologist, this is all that we can demand. He does not, to be sure, say very much about the social conditions that would seem to be requisite to sustaining the ideal moral order. In general terms, these are the ideas that we familiarly associate with Peirce and Dewey, critically, the ideas of publicity and access.<sup>51</sup> But it is also clear that under conditions of industrialized science, it is just these conditions that are now under threat.

"Shoddy science" becomes possible when published papers are not being read and thus not subjected to critical scrutiny.<sup>52</sup> But since they easily become part of the construction of facts, how can we know what to trust? "Entrepreneurial science" allows contractors to establish huge mission-oriented, capital-intensive enterprises in which researchers lose all independence and everyone else is denied access. Since these products are not assessed by consensus, why should they be trusted? "Runaway technology" can produce "reckless science." Here ready to access to millions of dollars aimed at some specific technical power, for example, the manipulation of genetic

materials, can produce shoddy science now accompanied by the risk of catastrophic consequences. Finally, there is "dirty science" in which opportunities to fund research projects aimed at realizing understanding are converted into technologies for state purposes of destruction, or control, or manipulation.

We thus come full circle. We are stuck with our history. With the invention of modern philosophy as a discipline pretending not only autonomy but a privileged role in the intellectual division of labor, philosophers unwittingly conspired in mystifying a world in which science has played a profoundly important role. Seventy years after Dewey's called for reconstruction, the need is, if anything, even more urgent.

### Notes

<sup>1</sup> I have made two efforts at this, in *A History and Philosophy of the Social Sciences* (New York and Oxford: Basil Blackwell, 1987) and *War and Democracy* (New York and Oxford: Basil Blackwell, 1989).

<sup>2</sup> Quine's 1969 essay and Stroud's 1981 "The Significance of Naturalized Epistemology" are reprinted in Hilary Kornblith's influential anthology, *Naturalizing Epistemology* (Cambridge, Mass.: MIT Press, 1985), cited in what follows.

<sup>3</sup> *Ibid.*, p. 77.

<sup>4</sup> *Ibid.*, quoted from "Epistemology" (Quine's picture, presumably, is that, in Carnapian fashion, our knower can continually revise C-functions or in Popperian fashion, she can continue indefinitely to conceive hypothesis which she tries to falsify. These two programs in philosophy of science have been the most influential epistemologies in our century, but, as Laudan observes, they "have run into technical difficulties which seem beyond their resources to surmount" ("Progress or Rationality: The Prospects for Normative Naturalisms," *American Philosophical Quarterly* 14, 1 (January 1987), p. 19.

<sup>5</sup> See John Dewey, "The Existence of the World as Logical Problem" (MW 8:94-95). Of course, there are other forms of skepticism. See, e.g., Thomas Nagel, *The View From Nowhere* (New York: Oxford University Press, 1986), p. 71. J.E. Tiles, in *Dewey* (London: Routledge and Kegan Paul, 1988), quotes Russell's complaint that 'Professor Dewey ignores all fundamental skepticism. To those who are troubled by the question: 'Is knowledge possible at all?' he has nothing to say" (p. 14). Tiles retorts that this is not fair: "what Dewey had to say was that the question lacked foundation" (p. 14). But "the question" is ambiguous. Whether there is an external world which is at least partly structured cannot be motivated. But whether knowledge is possible, given our history, does have a point. See below.

<sup>6</sup> See Tiles, *Dewey*, pp. 70-76. 116-23, 127-29. My account departs, however, from Tiles (and from Dewey?). Holding to an absolute conception of reality does not commit one to an absolute conception of knowledge. It is not my contention that we could

describe the world "from no point of view." Knowing is necessarily a relation between a situated knower and "the world." But unless being depends upon knowing, this does not make whatever is at the object end either featureless or unknowable. On the other hand, Dewey was correct to insist that objects of knowledge (the character of things as known) were produced by inquiry. But because they are not produced from whole cloth (either by individuals or groups!), the skeptical problem arises.

<sup>7</sup> Fallibilism, according to Nagel in *The View from Nowhere*, holds that our beliefs "go beyond their grounds in ways that make it impossible to defend them against doubt" (p. 68). Nagel here is defining skepticism, not fallibilism! It is hard to say how much disagreement in epistemology turns on different usages.

Peirce's limit conception of truth provides an anchor, but at a cost. See below. Dewey seemed at least to subscribe to Peirce's conception. See John Dewey, *Logic: The Theory of Inquiry* (LW 2:345).

<sup>8</sup> See David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1974), p. 32ff. In his *Dewey*, Bloor holds that Dewey's fallibilism was secured by the idea that inquiries comprise a continuum and suggests that Dewey was correct to de-emphasize Peirce's limit theory (pp. 106-8). I doubt this. Where there is no doubt, there is no inquiry. But the limit notion of truth does, at least, give the dissenter a rhetorical tack which is otherwise lacking.

<sup>9</sup> Michael Friedman, "Truth and Confirmation," in *Naturalizing Epistemology*. Friedman offers what seem to me to be fatal objections to positivist views and to those views, like Peirce's, which seek to ensure a connection between confirmation and truth by giving a special meaning to truth. This would include Popper and at least some contemporary versions of instrumentalism—perhaps Larry Laudan. As regards the theory of reference, see my sketch of Harré's approach, below.

<sup>10</sup> *Ibid.*, pp. 155-56.

<sup>11</sup> *Ibid.*, p. 161. Notice that sociology is omitted. Presumably, what it has to offer is irrelevant to epistemology?

<sup>12</sup> Quine waffles on just what he is claiming. Susan Haack holds that Quine is "ambivalent" between a reformist "Modest Naturalism" in which epistemology is an integral part of empirical belief and a revolutionary "Scientific Naturalism" "according to which epistemology is to be conducted wholly within the natural sciences." See her "The Two Faces of Quine's Naturalism," ms, nd. On his more notorious ambiguities regarding the "validation" of claims to knowledge, see Ken Gemes, "Epistemological Vs. Causal Explanation in Quine, or Quine: *Sic et Non*," ms, nd.

<sup>13</sup> Another is to observe (versus Quine) there is no way (as far as we can know) to go from "molecules upon our sensory surfaces" to the rat perception of, e.g., an edible object, to the (linguistically modeled) belief that there are red apples in the world. We return to this.

<sup>14</sup> See my "Modest Realism, Experience and Evolution;" in *Harré and His Critics* ed. Roy Bhaskar (Oxford: Basil Blackwell, 1990).

<sup>15</sup> Barry Stroud, "The Significance of Naturalized Epistemology" *Naturalizing Epistemology*, p. 76.

<sup>16</sup> See P. 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 (1985), pp. 76-101; and "The Sociology of Scientific Knowledge: Can We Ever Get It Straight?" *Journal for the Theory of Social Behavior* 18 (1988), pp. 51-76. As we indicated in the latter essay, there are important differences between researchers as regards questions in philosophy, between (say) Barnes, Harry Collins, and Steve Woolgar. Confusion over the claims of Barnes and Bloor is now joined by confusion over these differences.

<sup>17</sup> Barry Barnes, *Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977), p. 25.

<sup>18</sup> I take this example from Barry Barnes, "On the Conventional Character of Knowledge and Cognition," *Philosophy of the Social Sciences* 11 (1981), pp. 303-33. Barnes puts it to the same purpose. See R. Bulmer, "Why is the Cassowary not a Bird?" *Man* 21 (1967), pp. 4-25.

<sup>19</sup> See Derek Bickerton, *The Origin of Language* (Chicago: University of Chicago Press, 1991).

<sup>20</sup> Thus drawing the rage of philosophers. It is presumably one thing to explain irrationally fixed belief by appeal to sociological facts; it is quite another thing to suppose that rational belief needs these. Presumably, one must contrast my belief that some figure presently in my vision is the Virgin Mary with my belief that some figure presently in my vision is a panda. Anthony Flew, now speaking for countless epistemological individualists, thinks that the former belief admits of a sociological explanation, but that if it is being argued (and it is!) that "intrusive, non-social, physiological, and biological facts" are not sufficient to explain this latter belief, then the view is "manifestly preposterous and in its implications, catastrophically obscurantist." "A Strong Programme for the Sociology of Belief," *Inquiry* 25 (1982), pp. 366-67.

<sup>21</sup> Dewey continues: "Through examination of the *relations* which exist between means (methods) employed and conclusions attained as their consequence, reasons are discovered why some methods succeed and other methods fail" (LW 12:17).

<sup>22</sup> See his *Judgment and Justification* (Cambridge: Cambridge University Press, 1988).

<sup>23</sup> According to Lycan, "occurrent beliefs are sentencelike representations stored and

played back in our brains" (ibid., p. 6). A belief, then, "is epistemically justified if and only if it is rated highly overall by the set of all-purpose, topic neutral canons of theory-preference that would have been selected by Mother Nature for creatures of our general sort. . . !" (p. 160).

<sup>24</sup> See Ralph W. Sleeper's important *The Necessity of Pragmatism* (New Haven, Yale University Press, 1986); and Thomas Burke, "Dewey on Defeasibility," in *Situation Theory and Its Applications*, eds. R. Cooper, K. Mukai, and J. Perry, (Stanford: CSLI Publications, 1990), pp. 233-68.

<sup>25</sup> Confirmation theory is the skeleton in the closet of empiricist epistemologies of this century. For some of the key papers, see P. T. Manicas, ed., *Logic as Philosophy* (New York: D. Van Nostrand, 1972).

<sup>26</sup> As Burke writes:

The basic scenario is that a given organism/environmental system is constantly performing certain operations as a matter of course employing sensory mechanisms, scanning, varying, probing, and otherwise moving about and altering things. Inquiry is initiated by some unsettling perturbation. . . . None of this needs be "deliberate." Dewey's picture of inquiry is supposed to describe general architectural and dynamic features of virtually any constituent subsystem of living animals, characterizing the simplest cellular life-functions as well as the most complex motor activities." ("Dewey on Defeasibility," p. 236.)

Classical epistemology is intellectualist in that it misconstrues "experience" and then conflates "having of an experience" with knowledge. Experience is "an affair of the intercourse of a living being with its physical and social environment"; it is not primarily "psychical;" nor "a knowledgeaffair"; and it is "pregnant with connexions" and "full of inference" (MW 10:6).

<sup>27</sup> Compare Barnes and Bloor, "Rationalism and the Sociology of Knowledge," in *Rationality and Relativism* eds. Hollis and Lukes, (Cambridge, Mass.: MIT Press, 1982), p. 44. For a provocative treatment in the context of recent philosophy of mathematics, see Mary Tiles, *Mathematics and The Image of Reason* (Cambridge: Cambridge University Press, 1991).

<sup>28</sup> M. T. Turvey, R. E. Shaw, E. S. Reed, and W. M. Mace, "Ecological Laws of Perceiving and Acting: In Reply to Fodor and Pylyshyn (1981), *Cognition* 9 (1981), p. 285. See also W. B. Weimer, "The Psychology of Inference and Expectations: Some Preliminary Remarks," in *Minnesota Studies in the Philosophy of Science*, Eds. G. R. Maxwell and A. R. Anderson vol. 6 (Minneapolis: University of Minnesota Press, 1975).

<sup>29</sup> David Kelly and Janet Kreuger, "The Psychology of Abstraction," *Journal for the Theory of Social Behavior* 14 (March 1984), p. 64.

<sup>30</sup> Nicholas Rescher, *Methodological Pragmatism* (Oxford: Basil Blackwell, 1975), p. 27.

<sup>31</sup> *Ibid.*, p. 28. See Guy Axtell, "Logicism, Pragmatism and MetaScience," in *Philosophy of Science*, forthcoming.

<sup>32</sup> *Ibid.*, p. 36.

<sup>33</sup> Larry Laudan, *Science and Values* (Berkeley: University of California Press, 1984), p. 62.

<sup>34</sup> *Ibid.*, p. 63.

<sup>35</sup> Rescher, *Methodological Pragmatism*, p. 73.

<sup>36</sup> *Ibid.*

<sup>37</sup> Of course, the pragmatist, rejecting the problem of knowledge *überhaupt* and alive to differences in aims, cognitive and otherwise, is open to the possibility that different communities with different aims, cognitive and otherwise, might well be justified in their beliefs about the world. Thus, it is not clear that Karam methods, perhaps informed by and tested against goals which, for example, emphasize harmony with the natural world, are not justified.

<sup>38</sup> Expanding on work in his *Progress and Its Problems*, Laudan has provided a "test" of "realist axiology and methodology" in his *Science and Values*, chapter 5. He construes realism as a truth-realism and then argues that a great deal of what physicists have believed to be true has been given up. Accordingly, realist methodological advice cannot be historically vindicated. However, as Harré says, this conclusion is vulnerable to a "modest" objection: "While physicists perhaps have not been able to keep their stock of deep fundamental theories unscathed by later developments, there has been a continual refinement and growing repertoire of very plausible items of information about many kinds of being whose existence can no longer be seriously called into doubt." (*Varieties of Realism* [Oxford and New York: Basil Blackwell, 1987], p. 41). Indeed, once realism is construed as by Harré, not only can Laudan's bullet be dodged but an excellent case for what Harré calls policy realism can be made: "If a substantive term seems to denote a being of certain natural kind (and some special conditions are satisfied by the theory in which that term functions) it is worth setting up a search for that being" (p. 59). That is, by including in their working vocabulary a robust referring expression, there are "features of theories which historical experience shows are good bets for having anticipated experience. . ." (p. 60).

For other suggestions for rules worth testing, see Rachel Laudan, Larry Laudan, and Arthur Donovan, *Scrutinizing Science* (Holland: Kluwer Academic Publishers, 1988).

<sup>39</sup> I have suggested that Dewey held to an instrumentalist theory of science in which

prediction and control exhaustively defined the goals of the sciences. See my "Pragmatic Philosophy of Science and the Charge of Scientism," *Transactions of the C.S. Peirce Society* 24, 2 (Spring 1988), pp. 179-219. In his antirealism as regards theoretical terms, he shares much with my colleague, Larry Laudan.

<sup>40</sup> Think of astrophysics at Princeton's Advanced Institute and at Rome ARDC, research in solid state physics at Stony Brook or at Roswell, N.M., DNA/RNA research at Cold Spring Harbor and at Texas Medical Center; biochemists working at Max Factor, or on bonding metals ions to antibodies at Scripps Clinic, or neurotransmitters at the University of Hawaii; economists at the Bureau of Labor, the American Enterprise Institute, or Cambridge University, England; psychologists at Merrill Lynch, in the social welfare services of the City of New York, at the New School for Social Research, at MIT; unfunded anthropologists in Thailand and anthropologists working for AID in Thailand. One could easily go on.

<sup>41</sup> Quoted by G. Holton, "Mach, Einstein, and the Search for Reality." in Ernst Mach, *Physicist and Philosopher*, eds. R. S. Cohen and R. J. Seeger (Dordrecht: Reidel, 1970). The text is from Einstein's 1933 Herbert Spencer Lecture.

<sup>42</sup> Feyerabend surely goes too far here. Harré points out that Feyerabend aims his guns at "the logicisms of the alleged inductive method and the fallibilism of Popper," but this target is too restricted. More importantly as regards the present context, "there may be more than one but not indefinitely many contexts of enquiry, in each of which different methodological and metaphysical principles, each cluster of which could be taken as defining a scientific inquiry, could be rationally defended" (*Varieties of Realism*, pp. 24-25). This would, I think, still undermine Laudan's program.

<sup>43</sup> Compare, of course: Michael Polanyi, *Personal Knowledge* (London: Routledge and Kegan Paul, 1958); Jerome Ravetz, *Scientific Knowledge and Its Social Problems* (New York: Oxford University Press, 1971).

<sup>44</sup> Merely by way of illustration, what are we to make of the fact that sixty billion dollars will be spent in the 1990s on a half-dozen projects—a space station, a human genome project, a supercollider. And what are we to make of the criticism that "big science has gone berserk," that "good minds and a lot of money are going into areas that are not relevant to American competitiveness, American technological health, or even the balanced development of American science" (*New York Times*, Sunday, May 27, 1990, p. 1).

<sup>45</sup> Harré, *Varieties of Realism*, p. 67.

<sup>46</sup> *Ibid.*, p. 154.

<sup>47</sup> *Ibid.*, p. 179. They will, for example, require dispositional attributions. The clearest examples are in everyday discriminations, for example, salt and sugar. But consider also classifications of modern zoology, sustained (or not) by appeal to beliefs

deriving from neoDarwinian theory (just as the Karam classifications are sustained [or not] by appeal to beliefs which run past Realm I discourse).

<sup>48</sup> *Ibid.*, p. 12.

<sup>49</sup> *Ibid.*, p. 90.

<sup>50</sup> have, of course, omitted all of his argument in favor of policy realism. This takes us onto more familiar ground in the philosophy of science.

<sup>51</sup> See Dewey's remarkable *The Public and Its Problems* (LW 2). Dewey pointed out that the conditions for assessing claims were, in general, being eroded. Thirty years later, C. W. Mills picked up this theme: As "experts" constrained dialogue, "publics" were being converted to "masses:"

<sup>52</sup> The term "shoddy science," the analysis, and the other categories that follow are thanks to Jerome Ravetz, *Scientific Knowledge and Its Social Problems*, chap. 10.