Steve Fuller

IS RELATIVISM OBSOLETE?

Ever since Socrates first confronted the Sophists, philosophers have tried to defeat relativism on conceptual grounds as "self-refuting." However, most self-avowed relativists, from the ancient Greek Sophists to present-day sociologists of knowledge, have been drawn to their position on *empirical* grounds, and thus have failed to be moved by Socratic charges of conceptual incoherence. In this respect, relativists are the original naturalized epistemologists (Quine, 1975). But, if anything, this makes their position *more* vulnerable, as well as more interesting, to the various empirical disciplines whose research can bear on the relativist's claims. In what follows, I will argue that relativism is, on empirical grounds, an obsolete position for studying science in society.

The Socratic Legacy to Relativism

That Socrates was the most artful Sophist of them all is a recurrent theme in the history of Western philosophy. The idea is that Socrates outwitted his sophistic interlocutors by using their own rhetorical skills (Bil-

lig, 1987). One trick in particular deserves mention in this context. With only a hint of hindsight, we may say that Socrates managed to persuade his audience to treat *relativism* and *antirealism* as one and the same position. In other words, he got them to confuse the thesis that (epistemic or moral) standards are relative to a given locale with the thesis that standards are none other than what one says they are at a given moment. It is a confusion that we continue to suffer from today. Call it the *Socratic Conflation.* Evidence for the Socratic Conflation may be found in the way philosophy students are most often taught to interpret the Protagorean maxim: "Man is the measure of all things." Whereas today the "man" in the expression is taken to mean the solipsistic individual, who is a standard unto himself ("true for me" truth), the original Sophistic use of *anthropos* referred to the "average man" in a community, in terms of whose standards one could tell whether one was in the right or the wrong.

As a dialectical strategy, Socratic Conflation converts relativism from a positive to a negative thesis. And so, Protagoras advises that when in Athens do as the Athenians

do; however, Socrates interprets him to mean that when not in Athens one need not do as the Athenians do. Protagoras thought he was respecting local customs, but Socrates managed to portray him as merely trying to appease the yokels. We would now say that Socrates obscured for future generations the possibility that relativism might be aligned with realism — that there may be spatiotemporally indexed "facts of the matter." For in successfully reframing Protagorean deference as cynicism, Socrates made it seem as though if a fact is *determinate*, it must also be *universal*. Moreover, Socrates managed to suppress the deep cynicism implicit in his own position. For as soon as Socrates granted the universality of standards, he denied that any particular native understanding of those standards was adequate. Indeed, it was up to philosophy to relieve the natives of their confusions by informing them of the principles that have all along implicitly underwritten their sense of right and wrong.

The fact that Socrates was able to make the Sophists look bad suggests a couple of interesting points about people's psychological reaction to relativism. First, relativism is not the attitude that people normally have toward their beliefs. In fact, it is an attitude that needs to be explicitly cultivated, as when one engages in "disinterested" research into people's beliefs. For example, anthropologists typically have a clearer sense of the differences between their own culture and the cultures that they study than the natives of those cultures would. (Indeed, would it be so far-fetched to say that the discipline of anthropology could only have arisen in the West, which since the time of the Greeks has been fascinated by its own cultural identity?) In a similar vein, David Bloor (1976) and Harry Collins (1981) are quite right in seeing sociologists of knowledge as "professional relativists." Second, people would prefer to think that there were universally shared beliefs or standards — even if they have only imperfect access to them — than to think that such beliefs or standards had merely local purchase on people's actions. Another way

of making the point is to say that what makes norms "normative" is not knowledge of their specific content but the fact that everyone abides by them, whatever their content.

The Sociology of Knowledge Debates: Will the Real Relativist Please Stand Up?

Philosophers have continued to re-enact Socrates' original ruse in today's encounters with relativists. A good case in point is Larry Laudan's portrayal of the relativist — designated as a sociologist of scientific knowledge — in his own set of dialogues, *Science & Relativism*. Laudan (1990: esp. 74) is principally concerned with what determines theory choice in science. His version of the Socratic Conflation occurs by having the relativist slide from saying that *nature* does not determine theory choice, to her saying that *evidence* does not determine it, to her concluding that *reason* fails to settle matters. The relativist, then, is made to look like a skeptic and an irrationalist. Laudan makes his job easy by taking advantage of the rhetorical appeals that Harry Collins and other radical sociologists have made to Quine's thesis that data always underdetermine theory choice. By endorsing this thesis, the sociologists unwittingly buy into Laudan's (1977) arationality assumption, which provides a place for social accounts of science only once accounts based on "rational methodology" have been exhausted (Fuller, 1990). The sociologists think that Quine supports their case because Quine seems to believe that the methodological accounts are *always* exhausted. However, this sense of exhaustion leads critics like Laudan to infer that relativists believe that the grounds for theory choice are never more than makeshift.

Now, in Laudan's defense, it must be said that the more radical "reflexivists" among the social constructivists do assimilate their relativism to a form of antirealism that opens them to the above charge. The bluntest form of the charge comes as a *tu quoque:* if it is always left to happenstance which theory should be selected, then doesn't this point

also apply to the relativist's own account of science? To their credit, reflexivists such as Steve Woolgar (1989) readily concede the point, but then try — in classic Pyrrhonian fashion — to convert their dialectical ambivalence into an instrument for destabilizing any presumptions the reader might have about how scientific knowledge is constructed. The "New Literary Forms" that Woolgar (1988) and his colleagues in discourse analysis have pursued in recent years are Borges-inspired attempts to ensure that the reader's ruminations never reach a resting point. Thus, the reflexivists forsake the "cognitive," or "representational," function of language, in favor of exploiting language's ability to provoke and interrupt thought processes. Whatever they may privately think of the efficacy of this project, Laudan and other logically trained philosophers of science can respect it for its self-consistency: at last, relativists gladly eating their own words!

Unfortunately for Laudan, however, the relativists that he explicitly attacks — Bloor and Collins — are not antirealists, and hence have felt no need to exchange empirical assertion for more exotic forms of verbal expression. Given Laudan's Socratic view of relativism as antirealism, it is perhaps not surprising that he argues with thinly veiled contempt against Bloor and Collins. And although, there is a sense (to be explained below) in which these relativists do deny that nature can determine theory choice, they most certainly do not deny that reasons can. Rather, Bloor and Collins restrict the scope in which any set of reasons applies. In other words, they hold that there are no *unconditionally* good reasons for selecting a particular theory. This is normally called the *instrumental theory of rationality:* the justifiability of beliefs is relative to the epistemic constraints under which one operates — in particular, the methods available and the ends toward which inquiry is directed. But this explication puts us dangerously close to Laudan's (1987) own "normative naturalism," whose attendant theory of rationality consists of a set of historically verified hypothetical imperatives.

Truth be told, it may even be argued that Laudan's instrumental rationalist is *more* of a Protagorean relativist than the image of the scientist who emerges from Barnes & Bloor's (1982) Strong Programme in the Sociology of Knowledge. After all, Barnes & Bloor hold that instrumental rationality is fundamental to the human condition, science simply being a particular set of situations and utilities that frames instrumentally rational action at certain times and places. The Strong Programme's four methodological tenets — impartiality, causality, symmetry, reflexivity — ensure that instrumental rationality can figure in the explanation of any human action, if it could, in principle, figure in the explanation of every action (regardless of, say, our approval of the action's consequences). Laudan hardly aspires to such universality. However, this point is often obscured because Laudan samples from the entire history of science for instances of instrumental rationality. But only a few figures and episodes are eligible to be drawn from each period. They include people who, in retrospect, can be seen as having been driven by epistemically appropriate ends — in short, the progenitors we would have chosen as our own. Although a "culture" that encompases both Newton and today's best scientists is more spatiotemporally diffuse than the paradigm cases of culture familiar from anthropology, Laudan's relativism here is unmistakable. Because he sets stricter conditions than the Strong Programme for the presence of rationality in science, Laudan outdoes his sociological foes in contributing to the image of science as a rather idiosyncratic human practice — the very image that one would expect from a relativist!

Yet, it is generally agreed that Laudan scored a major rhetorical coup by avoiding all association with relativism. He succeeded by highlighting certain claims by Bloor and Collins that suggested the irrelevance of nature to the selection of scientific theories. Perhaps the most notorious of these claims is this often quoted one by Collins (1981: 54): "The natural world in no way constrains what is believed to be." Laudan would like us to

infer from this quote that Collins holds that we are so embedded in our social constructions that nature can never have any purchase on our beliefs. Now, even if this is what Collins was trying to say, such a belief would not necessarily commit him to a social idealism or solipsism. On the contrary, it would be possible to relate this interpretation to a widely held view among ethologists, namely, that, in comparison with other members of the animal kingdom, human beings are sheltered from any direct contact with the forces of natural selection, largely because we are encased in a socially constructed environment within which our behaviors are selectively reinforced. In fact, according to Byrne & Whiten (1987), the perceived complexity of the natural world may be little more than a function of the complex social relations in which one must engage in order to have access to nature. This is true whether one is talking about getting a bite to eat or getting a publishable scientific finding. Byrne & Whiten thus claim to be able to correlate primate intelligence with sociological complexity. But, as I said, we need to appeal to such a thesis, only if Laudan has got his intended sociological targets right. However, the following quote from Barnes & Bloor (1982: 34) would suggest, however, that this is not the case:

> The general conclusion is that reality is, after all, a common factor in all the vastly different cognitive responses that men produce to it. Being a common factor, it is not a promising candidate to field as an explanation of that variation.

This, I would argue, puts an entirely different slant on things. Nature cannot determine our theory choices because it is *always already* a component of those choices. Barnes & Bloor make this point in the course of arguing against a view often supposed by rationalists, namely, that the scientists whose theories have stood the test of time were somehow in closer contact with nature than the scientists whose theories have not. In other words, Barnes & Bloor want to *oppose,* not support, the idea that epistemic differ-

ences reflect ontological ones, which implies that their relativism presupposes, not antirealism, but realism.

Interlude I: An Inventory of Relativisms

The careful reader will notice that I have countenanced at least three different positions that are legitimately called "relativism." For the sake of analytic clarity, I present the following inventory, designed to show three different contexts in which relativism figures in opposition to some other position in science studies debates. And so, what might "relative" mean?

R1: *local (vs. universal):* This is the relativism of Protagoras, Mannheim, and the Strong Programme. It presupposes realism in two senses: (a) there is a fact of the matter as to what is true and false, right and wrong, but this fact is spatiotemporally indexed, often specifically to cultures; (b) all of our thoughts and actions — not just the ones we deem true or right — are grounded in a reality independent of our conceptions, which serves, in Kantian fashion, to convert all questions of metaphysics to ones of epistemology.

R2: *indeterminate (vs. determinate):* This is the relativism of the later Wittgenstein and more moderate social constructivists of science. It is antirealist in the sense that there is no fact of the matter as to what is true and false, right and wrong, until closure is brought to an interpretively open situation. These episodes of closure constrain the justification — though not necessarily the commission — of future action. They establish *conventions.* There are two general reasons why interpretively open situations might call for conventions: (a) a *surfeit* of competing interpretations, as in the variety of trade-offs that can be made when no single theory maximizes all the relevant cognitive criteria or no course of action harmonizes the interests of all the relevant parties; (b) a *dearth* of competing interpretations,

as when certain conceptual (i.e., theoretical) distinctions fail to make any empirical (i.e., practical) difference, until practices are instituted — such as alternative experimental outcomes — that operationalize the distinction.

At this point, notice that it is possible to be both an R1 and R2 relativist. For example, most moderate social constructivists, such as Collins and Knorr-Cetina (1981), are R1-relativists with regard to social scientific discourse (and hence are, after a fashion, "local social realists"), but R2-relativists with regard to natural scientific discourse (and hence are "antirealists," in the sense that philosophers of science normally use the term). What this means, in practice, is that these constructivists respect the integrity of science as a culture, but they refuse to privilege the scientists' own understanding of their culture. As Woolgar and other more radical constructivists have observed, this view suffers from a lack of reflexive consistency, since clearly R1 privileges the sociologists' scientific understanding of *any* culture.

R3: *irrational (vs. rational):* This is the original relativism of Edward Westermarck (1912), Max Weber, and the logical positivist *de gustibus non disputandum* attitude toward values. It also captures the Pyrrhonian side of the reflexive social constructivists of science. In a backhanded way, this form of relativism presupposes a *deep* ontological distinction between what is real — and hence representable and cognitively accessible — and what is not. Values fall in the latter category because they allegedly rest on subjective choices and emotional commitments for which no independent rational grounding can be given. Verbal reinforcement (i.e. "ethics") and ritual then serve to routinize these commitments, which — from a more objective standpoint — may no better contribute to a society's survival than would some other combination of behavioral and verbal conditioning. However, the ultimate test of a morality is not what some outside observer thinks, but whether the insiders can "live" with its strictures.

As a point of reference, the history of anthropology has exhibited all three forms of relativism. R1 reflects the "idiographic" commitments of orthodox ethnographic method pioneered by Franz Boas and still dominant among symbolic and cultural anthropologists. R2 captures the reflexive ethnography that "inscribes the ethnographer in her own text" (cf. Clifford & Marcus, 1986), and in that way removes the last epistemic vestiges of imperialism. However, in the process, this move may also eliminate anthropology's traditional object of inquiry, the self-contained alien culture. Finally, R3 may be observed in structural-functionalist social anthropology (Malinowski, Radcliffe-Brown), especially in versions that stress discrepancies between the anthropologist's and the native's perspectives, as in the "latent functions" performed by seeming irrational social practices.

Interlude II: Mannheim's Realistic Relativism

The Canadian sociologists Volker Meja and Nico Stehr have recently provided some assistance by translating the debates surrounding the initial reception of Karl Mannheim's sociology of knowledge in Germany (Meja & Stehr, 1990). Those who have participated in the latest round of the sociology of knowledge dispute — involving Laudan, Bloor, Collins, et al. — would be struck by several turns that the dialectic has taken since Mannheim first met his critics. For whereas today's sociologists of knowledge tend to define themselves as *opposing* philosophy, Mannheim usually tried to blur the difference between the two disciplines. In fact, he displayed his sympathy with the classical philosophical aspiration to universal truth by explicitly opposing antirealist forms of relativism, and proposing instead the doctrine of *relationism,* which states that social conditions determine which truths are epistemically accessible. This doctrine was elab-

orated in a discussion of the social significance of the sort of synthetic thinking championed by Hegel. According to Mannheim, Hegel was part of a generation that was in a position to pull together strands of thought that were left unraveled by earlier generations. Although Mannheim certainly did not consider the Hegelian synthesis as final, he seemed to think that it marked genuine progress in thought that would not have been possible, had Hegel not had specific precursors, and had he not lived in the time and place that he did. The idea, then, seems to be Hegel's very own, namely, that universal truths may be glimpsed only at certain moments in history. Or, to put it as a question: If there are, indeed, universal truths, then why have we not always known them? An interesting way to read Mannheim, which some of his critics picked up on, is as claiming that if one takes *very seriously* the idea that certain things are true for all times and places, then sociology of knowledge simply takes up the traditional tasks of epistemology by explaining the differential access that people living in different times and places have had to those truths.

Mannheim's critics tended to raise doubts about whether the sociology of knowledge was equipped to subsume the philosophical enterprise of epistemology. In retrospect, Mannheim's strategy seemed very much like Quine's (1985) "naturalization" of epistemology. Both held that the relevant special science — be it sociology of knowledge or behavioral psychology — can subsume epistemology by showing that the sorts of positions which traditionally distanced epistemology from the sciences (i.e. absolutism, foundationalism) are empirically untenable. Perhaps more than Quine, Mannheim took this to be not a capitulation of philosophy to the special sciences, but rather a consistent application of philosophical reasoning to the point of transcending the disciplinary boundary separating philosophy from the special sciences. (After all, is it not only the institution of philosophy in the twentieth century — and not philosophical thought itself — that clearly demarcates philosophy from the sci-

ences?) Indeed, Mannheim periodically cast his own interest in the "existential connectedness of thought" as continuous with Heidegger's search for existential structures in *Being and Time.* In this way, Mannheim managed to answer most of his critics' charges of relativism.

However, Mannheim failed to stave off the concerns raised by his Frankfurt School critics, Herbert Marcuse and Max Horkheimer (Meja & Stehr, 1990: 129-157). They located Mannheim's latent relativism in the sociology of knowledge's failure to specify the sense in which a form of thought "reflects" its social conditions. After all, a body of thought, such as Marxism, may be very much a product of its time, yet it may serve, not to reproduce the existing social order, but to radically transform that order so as to enable a completely different sort of thought to be generated in the future. In other words, Mannheim's implicit sociological functionalism dampened the prospect of substantially different consequences following from the political options available in a given time and place. Not surprisingly, then, Marxists have tended to distrust the surface radicalism of the sociology of knowledge as masking a politically quiescent worldview.

The Obsolescence of Relativism

The Frankfurt School's political dissatisfaction with Mannheim's sociology of knowledge can be analyzed in more strictly epistemological terms, and generalized to other forms of relativism. To claim that a knowledge system is adapted, or "existentially connected," to its social context is to suggest that people exert considerable control over their thought processes — probably more than is warranted by the evidence concerning cognitive biases and limitations (cf. Elster, 1983). If we set aside cultural differences that are marked primarily on racial grounds, what is striking about the phenomenon of cultural diversity is just how *invisible* it is to most people most of the time. Consequently, when anthropologists try to get the natives to re-

veal their local customs, the natives often find themselves attending to their behavior in ways that they had never done before. Indeed, when anthropologists "go reflexive," they begin to wonder whether they might be subtly coercing the natives to draw distinctions where none exist. This is not to deny that laying claim to cultural identity and difference is a pervasive social practice. What I question is whether the practice amounts to anything more than a mobile rhetoric that is deployed on various occasions to achieve various ends. In short, while the average anthropologist knows enough to put the native's distinction between "good magic" and "bad magic" in scare quotes, she has yet to learn that the same policy should apply to the more seemingly fundamental line dividing "them" from "us."

If what I have said about the rhetorical character of cultural differences is correct, then Mannheim's question should be turned on its head. Instead of explaining what appears, from the inquirer's standpoint, as the *real diversity* of beliefs, the deeper concern ought to be with explaining the *apparent uniformity* that the different believers themselves experience (or, rather, presume). Recall the realist epistemology that motivates Mannheim's enterprise: if there is indeed one reality, or nature, with which we are always in contact, what explains, then, the difference in access to that reality that is implied by the existence of alternative knowledge systems? Now, let us turn the tables on Mannheim's realist presumption by subjecting it to the same test of epistemic access: if there are indeed deeply diverse knowledge systems, which nevertheless affirm a belief in a common reality, why then should we think that instances of such a belief imply the existence of such a reality? For, if the mere existence of one world were sufficient to cause different people to experience a world that they presume others also to experience, then there should be no diversity at all. However, the fact that diversity exists suggests that cultures unwittingly presume different worlds of one another. The mechanism at work here may be a generalization of the argument

made in Fuller (1989), whereby the illusion of epistemic agreement is maintained by a failure to detect real differences that emerge in the process of knowledge transmission. Thus, if it can be shown that the linguistic means at our disposal to transmit truths over time and space is less than reliable, then whatever invariance we seem to find in knowledge systems accepted across sociohistorical contexts is unlikely to be due to the invariant nature of the truths transmitted, but rather to cognitive mechanisms — both biases and limitations — that mask the differences in interpretation that would have naturally resulted from truths being unreliably transmitted to different times and places.

From an epistemological standpoint, Mannheim's all too easy "adaptationist" approach to the role of knowledge in society is the product of two distinct conflations: (a) between a culture's system of beliefs and its beliefs about those beliefs; (b) between the consequences of one's beliefs regarded abstractly as a system of thought and their consequences regarded concretely as the product of linguistic transmission and other forms of social interaction. In the case of (a), the inquirer's "clarity" about a culture's system of beliefs may give a highly misleading picture of what members of the culture make of those beliefs, if their "meta-beliefs" are sufficiently different from the inquirer's. Thus, Mannheim and other methodological relativists fail to consider why they alone (and not the cultures they study) enjoy the privilege of being relativists. The case of (b) points to Mannheim's tendency to ignore the material, unintentional (sometimes counter-intentional) character of knowledge-based action. This point will become increasingly important in our critique, as it highlights the empirical ambiguities involved in trying to demarcate a region of spacetime "relative" to which a certain knowledge system is "legitimate" or simply just "operative." Both (a) and (b) appear most noticeably as a blindspot about the critical role of intellectuals in society, one which prevented Mannheim from appreciating the normative project of the

Frankfurt School. In particular, because the relativist thinks of culture as an historically and geographically well-bounded unit, every epistemic standpoint must be either "inside" or "outside" the culture under study. The former is said to be "naive," the latter "critical." Taking the metaphor of standing "outside" a culture to its most literal extreme, Mannheim (1940) ultimately characterized the intelligentsia as "free floating." Although the Frankfurt School is not normally regarded as the most realist or materialist of Marx-inspired intellectual movements, its Hegelian reliance on a reflexive, or embedded, conception of critique makes it just the right antidote to Mannheimian relativism.

Here is what I take the Frankfurt critique of relativism to be: once it is realized that knowledge is embodied in action (or, more precisely, in the disposition of people to act), and that action has consequences that transcend the intentional horizon of the original agents, then it is possible to gain critical leverage over the members of one's own culture — namely, by having come *after* them in history. Of course, this is not to preclude the possibility of today's critic being surpassed by one in the future who can comprehend the first critic's blind-spots. The point is, rather, that one cannot underestimate the epistemic advantage that accrues to someone who stands at the end of a sequence of events. Sometimes, in a Popperian vein, this state is said to enable one to "learn from mistakes," but this way of putting matters is too strong, as it suggests standards of performance that do not vary over time, completely accurate recall, and other historically and psychologically implausible assumptions. More modestly, the critic need only say that she see things her predecessors had not. In any case, the burden of proof is squarely on the relativist to explain how history is incorporated into societies that have existed for any length of time. That is to say, relativists typically forget to include a notion of *institutional memory* (cf. Douglas 1986) in their conception of culture. As a result, they end up treating all moments in the history of a culture as epistemic equals: as far as rel-

ativists are concerned, one may have come at any point in the sequence.

One conclusion that emerges from this argument is that there is something empirically misbegotten about ongoing epistemological disputes between relativists and realists or rationalists. Do particular communities devise standards for evaluating knowledge claims? The answer is, of course, yes. But, *pace* relativists, it does not follow that those standards are used primarily to judge current members of that community. In other words, the context of *evaluation* and the context of *conduct* are quite different. If one is already a member of good standing in the community, then charity is more likely to operate in interpreting any disparity in the person's behavior. Thus, outrageous sounding hypotheses may be entertained by a scientific community a little longer when a Ph.D. utters them than when a mere B.A. does. However, if one has yet to prove oneself, then stricter, more "official" standards of evaluation apply. Under those circumstances, accidents and innovations are more likely seen as the products of ignorance and error. And as our critique of Mannheim suggested, such official standards also figure in judgments made about one's predecessors. In any case, these standards may well be quite different from the norms that implicitly govern the behavior of the community's own members, when they are not under especially tight scrutiny. Because a community's official standards tend to be used to judge various sorts of people who had nothing to do with their design or ratification, the standards achieve an aura of "independence" that gives heart to the realist — especially if a very wide array of people are so evaluated. Here the relativist rejoinder is on target: "independence" in this sense mainly reflects an absence of resistance to the evaluation made of the groups in question. It remains to be shown whether there is anything else going on (cf. Latour, 1987). Of course, there are many possible reasons for this lack of resistance, including the relative powerlessness of the groups in question and the indifference of those who are in power. (Who

speaks for the past but zealous exegetes?) But such powerlessness and indifference should never be confused with outright acceptance of the evaluation's (cf. Fuller 1988: 207-232). In short, from an empirical standpoint, the battle between relativists and realists is most fruitfully seen as being about how people come to speak for other people (not necessarily themselves).

A crucial anti-relativist assumption in the forgoing analysis is that the principles governing a society need not coincide with actors' construals of what those principles are. This would seem to commit me to an especially virulent form of sociological realism — "eliminative sociologism," as patterned after Paul Churchland's (1979) anti-psychologistic "eliminative materialism" (cf. Fuller, 1992). In other words, there is a fact of the matter about a society's epistemic practices that may elude that society's members. Indeed, members of the society may normally act on the basis of an empirically false "folk sociology" that functions as a kind of "false consciousness" (cf. Fuller 1988, Appendix B). With the exception of cultural anthropology, the social sciences have typically justified their existence with a claim of this sort. In any case, epistemologists need to explain how it is that knowledge producers continually do things with which other such producers find fault, whether it be an error, a failure to persuade, or simply a failure to communicate. One plausible way of casting this situation is to say that the identity of epistemic practices is very much like the identity of stock market trends: they are constituted in the course of being anticipated, or "guessed at," where the guesses pertain to what other relevant people will guess. Because feedback from the guesses is often delayed and imperfect, the market is prone to display considerable volatility, which would lead to complete financial collapse, if government did not insure the legitimacy of the transactions. This "Keynesian" perspective provides some justification for the office of epistemologist as someone who does something useful that individual knowledge producers or knowledge producing communities could not themselves do. Moreover, we need a Keynesian — rather than a strictly socialist approach — to knowledge production because the fact that all the knowledge producers do not have the same sense of what the epistemic practices are, and indeed none may have a particularly good grasp, does not prevent the emergent result of their activities turning out much of the time to good epistemic effect. Yet, to say that the knowledge enterprise often works by means of an "invisible hand" is not to downplay its social character. If anything, it is to reinforce it, for if everyone had the same epistemic practices, then it would be possible to study a randomly selected individual to understand how the entire knowledge production process works (cf. Wrong, 1961).

While the last point may seem obvious, it nevertheless cuts against the desirability of a political stance traditionally associated with relativism, one whose most articulate proponents have included Jean-Jacques Rousseau and Paul Feyerabend. The stance goes by a number of equally misleading names, including "libertarianism," "anarchism," and even "democratic communism." However, the outlines of the view are clear enough. Communities are portrayed as voluntary associations sufficiently well-bounded — and perhaps even spatiotemporally isolated from other communities — that both the possibilities and outcomes of actions are surveyable by the members of that community. In that case, action is readily treated as a projection of the collective beliefs and desires of the community. If the community's actions have unforeseen negative consequences on other communities, then it follows, in this view, that the community in question is too large, or at least is having impact on those from whom consent has not been secured. The proposed remedy is for the community to restrain itself in some way, perhaps by splitting up into smaller, more homogeneous units that can survive without unwittingly involving the lives of others. The flaw in this political vision is twofold. On the one hand, it overlooks the point I earlier raised against Mannheim, namely, that apparent uniformi-

ty in beliefs can mask real diversity that, when transferred to the political arena, turns out to be a major source of "betrayal" and "disappointment" (cf. Hirschman, 1982). On the other hand, the politics of relativism neglects the fact that people with beliefs radically different from one's own can do things in remote times and places that end up limiting, if not jeopardizing, one's ability to act.

As sociologists — though not yet sociologists of knowledge — have turned increasing attention to the "globalization" of the human condition, some interesting diagnoses have been offered for the persistent popularity of relativism. Most have pointed to the "reactive" character of relativist epistemology and politics, partly born of resentment and partly of nostalgia. In particular, these diagnoses point to a sense on the part of relativists that they are losing control of their own fates to forces that they do not fully understand. Thus, Wallerstein (1990) interprets nineteenth century nationalism, with its emphasis on a historically segregated, geographically bounded "homeland" or "society," as a backlash to the homogenization processes of the capitalist world-system. Moreover, as Sztompka (1990) has suggested, relativists try to foster the illusion of distinct peoples with distinct causal lineages by artificially maintaining local modes of understanding, long after contact with other cultures have rendered them obsolete. Indeed, the evolution of trade languages ("pidgins") into more generally applicable forms of communication might prove a useful source of models of how people from different communities come to understand, accept, and express the fact that they are bound together in a common fate. It is this ecologically minded ethic, rather than respect for local sovereignty, that is likely to foster the mutual calibration of interests and standards that characterizes the global consciousness appropriate for our times (cf. Parfit, 1984).

In conclusion, I would hate to leave the impression that I see no use whatsoever for relativism in today's world. On the contrary, I believe that a certain form of relativism is in fact quite necessary for "the pursuit of truth," in the way a realist might understand that expression. First, there is the point recently elaborated by Stich (1990), that given the infinitude of truths for any possible domain of inquiry, to urge that one simply "maximize the truth" is to offer no guidance for action, since virtually anything one might do is compatible with the injunction (including: covering up errors in the short term in the hope that they will cancel each other out as one approaches the truth). Indeed, most of the truths that philosophers have regarded as epistemically most valuable have emerged as unintended consequences of other things that one sets out to do, specifically as the resistance that comes from the mismatching of means to ends (Popper, 1972). One might call this the *counter-pragmatic* or *disutilitarian* theory of truth. As a social phenomenon, truth first appears as individual disutility, but in such a way as to contribute to the maximization of group utility. In more prosaic terms, if everyone benefits from one person's error, then a truth has been produced.

Thus, while it is clear that I believe the pursuit of truth is best understood as a social practice, I do not draw from that the relativist conclusion that any social practice has to be accepted as it is. In fact, the sorts of practices that advertise themselves as pursuing "truth for its own sake" may be the very ones whose social organization is most epistemically suspect, precisely because they do not receive enough external resistance. One need not impute conspiratorial thinking to the forces of Big Science to observe a couple of contexts in which the rhetoric of autonomous inquiry has transformed even *de jure* realists and rationalists into de *facto* relativists. One is the widespread belief among science policy advisors, that if an expensive scientific project does not actually harm the citizenry and offers the vague hope of beneficial technologies, then it deserves, *ceteris paribus*, to be maintained at current levels of funding. Another is the somewhat subtler phenomenon of scientists acquiring "adaptive preference formations" as they come to identify the epistemically relevant

aspects of their craft with those over which they have relatively direct control, which are, of course, manipulable by the political environment in which scientists find themselves (Fuller, 1989: 161-162). Thus, it would be interesting to see if scientists would continue to see such a sharp difference between the "intellectual" and "economic" value of research, if they were solely responsible for raising and distributing their own capital. Both rhetorical contexts impede the pursuit of truth by encouraging inquirers to turn a blind eye to their social setting. The remedy demands social practices that counteract these in the strategic manner of someone who truly believed that knowledge is a product of its social organization. Perhaps a good name for this remedy would be *counter-relativism*.

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Steve Fuller
Science Studies Center
Virginia Tech
Blacksburg VA 24061-0247
USA