THE EXPANDED SOCIAL
SCIENTIST'S BESTIARY

A Guide to Fabled Threats to, and
Defenses of, Naturalistic Social Science

D. C. PHILLIPS
The Microbe

The microbe is so very small
You cannot make him out at all,
But many sanguine people hope
To see him through a microscope.
His jointed tongue that lies beneath
A hundred curious rows of teeth;
His seven tufted tails with lots
Of lovely pink and purple spots,
On each of which a pattern stands
Composed of forty separate bands;
His eyebrows of a tender green;
All these have never yet been seen—
But scientists, who ought to know,
Assure us that they must be so . . . .
Oh! Let us never, never doubt
What nobody is sure about!

—Hilaire Belloc
BEHTIARY: . . . work in verse or prose describing with an allegorical moralizing commentary the appearance and habits of real and fabled animals.
—Webster’s Dictionary

PREFACE: ON GOOD AND BAD BEASTS

Not every change that takes place with the passing of the years can be regarded as an improvement. One case in point concerns the availability of literary genres: Folk in earlier ages had at their command genres that now have faded from use or that linger on only to be sources of amusement. A prime example, of course, is the bestiary. The poet Hilaire Belloc is one of the few writers in the twentieth century to have used this medieval form in any extended manner, and his wonderful “cautionary verses” manage to preserve the moralizing tone of the genre; but the facts that he wrote in simple verse and with humor have given the impression to many that the bestiary is fit only for the entertainment of children.

But the truth is otherwise—there is still a great need for bestiaries addressed to adults, for on the cusp of the new millennium life is fraught with dangers. The momentous political events of the first few years of the 1990s may have made the physical existence of the human species a little more secure (although this is not entirely clear), but the intellectual sphere is not unlike a swamp populated with a variety of exotic beasts—“isms” and “post-isms” abound; ideologies or paradigms are multiplying beyond necessity, and at the same time doctrinal fundamentalism is on the increase; too many people who should know better try to sway others by rhetoric or by appeals to crass self-interest rather than by reasoned argument; clarity and logical soundness of argument seem to be prized by diminishing numbers; and the intellectual ideals of the search for truth and of objective inquiry are held in some quarters to be outmoded inheritances from the past, or worse, they are seen as part of the technology of dominance of some groups, and some viewpoints, over others. The humanities are home to many of these beasts, but the social sciences also offer hospitable environmental niches.
Now, ecologists of the late twentieth century argued that all species of living things, whether beastly or not, have a right to exist; they ought not to face the threat of extinction. And it is probably a counsel of wisdom to adopt a similar policy in the intellectual realm. John Dewey (inspired in this, as in many other matters, by Hegel) would argue that if an intellectual position exists, there must be some problem—situation that inspired it and to which it proffers a solution; moreover, the fact that the position exists and has vocal adherents indicates that at least it has some "truth value." (See Dewey 1956, for a simple discussion of the origin of "schools of thought.") It is hoped that the Deweyan spirit pervades the discussions in this present book; a number of positions are analyzed, and their beastly nature is exposed, but on the whole it has been remembered that beasts are complex and have both good and bad features. Very few of them are condemned outright, and some pains have been taken to indicate the genuine problems or influences that have served as stimulus to their evolution. It should be noted that while some chapters bear the name of an important beast in the title, others do not, and instead refer to some positive influence that the beast-hunter can use with profit. (Of course, beastliness resides to some degree in the eye of the beholder, so the chapters that do not—in the author's opinion—refer to beasts ("Popperian Rules," for example) might be regarded as so doing by those readers with a different orientation. Added to which it must be said that not all beasts are bad; the elephant is nowadays highly regarded, although the Roman soldiers who first met the species when Hannibal used it against them in battle were not so positive in their evaluations.)

In much the same way, one can imagine a skeptical (and eccentric) intellectual accosting a social scientist and asking—Is social science possible? Rising to the bait, the scientist would most likely reply by saying—"Yes!" and when asked for proof would point to the work that he or she is doing—"Look, I am doing social science, ergo it is possible!" To which the quick-witted eccentric would be liable to respond by pointing to the case of alchemy. For the point is, if we could travel back in time and ask the alchemist if he thought alchemy was possible, the reply would be the same: the alchemist would point to the fact that he was doing alchemy, hence it must be possible. But—armed with twentieth-century hindsight—we would not find this answer acceptable. We would argue that the alchemist was deluded. The alchemist genuinely believed his craft to be possible, but it was not, for it rested upon mistaken theories and erroneous philosophical foundations. And the twentieth-century skeptic would take a similar stance with respect to the answer given by the social scientist—the fact that social scientists think they are doing social science clearly is not sufficient proof that the enterprise is not chimerical. There are numerous skeptics like this in the late nineteenth and twentieth centuries; to cite merely one example, the philosopher A. R. Louch (building to some extent on the work of Peter Winch in his The Idea of a Social Science) has written that "my main intent has been to show that the idea of a science of man or society is untenable" (Louch, quoted in Gellner 1979, 66).

Is there no better answer available to the social scientist? What would satisfy the skeptic? The trouble here is that it is always difficult to argue that something is possible; but there is solace in the fact that it is even more difficult to establish that something is impossible. So, one strategy that is available—although clearly it is not quite as convincing as producing
a direct proof—is to examine the arguments put forward by the skeptics
and to show that, although they are often motivated by genuine and im-
portant concerns, their own arguments do not establish what they think
they do! In short, the attempt can be made to defuse the arguments that
have led skeptics to the conclusion that the pursuit of social science is a
delusion.

This, then, is the program that underlies this bestiary: In the contemp-
orary world there are many who are skeptical about the possibility of
producing a naturalistic social science, that is, a social science that is in
important respects structurally or methodologically similar to the natural
sciences. Some of these skeptical concerns run deeper than others, and
some of the social sciences are more “at risk” from this attack than are
others. It will be argued that these skeptical arguments—these beasts—
fail to achieve their goal, although the social scientist would be wise to take
seriously many of the issues that are raised. The bestiary, in short, is an
indirect defense of the possibility of producing naturalistic social science.

It is clear that it is a matter of more than passing concern to clarify
precisely in what sense of the term social science can or cannot aspire to be
naturalistic. While it can be said, with some justice, that it is up to the
skeptics to make clear what they have in mind and what it is to which they
have objections (obligations that they often fail to fulfill satisfactorily),
nevertheless the defender of naturalistic social science cannot rest entirely
content with trying to pass the buck. Thus, the fifth chapter—“Natural-
istic Ideals in Social Science”—is of some importance in at least giving a
preliminary map of the territory. It could have served as the introductory
chapter to the whole work.
It is arguable that recent advances in the philosophical understanding of science have vindicated many of John Dewey's views on the matter. Scientific reason is not marked off from other forms of human intellectual endeavor as a sort of model of perfection that these lesser activities must always strive (unsuccessfully) to mimic. Rather, science embodies exactly the same types of fallible reasoning as are found elsewhere—it is just that scientists do a little more self-consciously and in a more controlled way, what all effective thinkers do. As Dewey pointed out more than half a century ago, he believed that intellectual inquiry,

in spite of the diverse subjects to which it applies, and the consequent diversity of its special techniques has a common structure or pattern: that this common structure is applied both in common sense and science.

(Dewey 1966, 101)

Thus, it is no surprise that Dewey contributed to the "unified science" movement of the early to middle decades of the twentieth century (a matter commented upon further in the previous chapter). Recent work has shown that scientists, like workers in other areas, are in the business of providing reasonable justifications for their assertions, but nothing they do can make these assertions absolutely safe from criticism and potential overthrow. (There are no absolute justifications, hence the somewhat misleading name sometimes given to recent epistemology—"non-justificationist." This is misleading because it suggests that if there are no absolute justifications, there are no justifications at all.) It is salutary to remember that Dewey preferred not to use the term truth, but rather warranted assertibility, and he recognized that different types of assertions required different warrants. Furthermore, this change of language
highlighted the fact that a warrant is not forever; today's warrant can be rescinded tomorrow, following further inquiry. Karl Popper, too, expressed a similar view; and in impassioned prose he pointed out that "the question of the sources of our knowledge, like so many authoritarian questions, is a genetic one. It asks for the origin of our knowledge, in the belief that knowledge may legitimize itself by its pedigree" (Popper 1985, 52). A little later he added:

So my answer to the questions "How do you know? What is the source or basis of your assertion? What observations have led you to it?" would be: "I do not know: my assertion was merely a guess. Never mind the source, or the sources, from which it may spring. ... But if you are interested in the problem which I tried to solve by my tentative assertion, you may help me by criticizing it as severely as you can." (Popper 1985, 53)

Criticism, for Popper, includes the offering of disconfirming (refuting) experimental data. (For further discussion of Popper's relevance for the conduct of social science research, see chapter 8.)

It should be clear, therefore, that none of this means that science is unbelievable, or that "anything goes" or "anything may be accepted," or that "there is no justification at all for scientific claims," or that "there are no standards by which the truth or adequacy (or both) of a piece of science can be judged." It simply means that it no longer can be claimed that there are any absolutely authoritative foundations upon which scientific knowledge is based. Hence, the other title often given to contemporary epistemology—"nonfoundationalist."

This account of science fits comfortably with the view that many scientists themselves hold—especially, perhaps, action researchers in the applied social sciences and evaluators of social programs; these latter are par excellence fields of "the believable," of building the "good case," but where even the best of cases can be challenged or reanalyzed or reinterpreted. Nothing is more suspicious in the field of program evaluation, for example, than a report that is presented with the implication that it has the status of "Holy Writ." Researchers in the "pure" sciences, and in the more laboratory-oriented of the social and human sciences, now have to accept that good science is a blood brother if not a sibling to what transpires in these messier and more open-ended fields of endeavor.

What happened in philosophy of science to build this new and modest view? Or, alternatively, what destroyed the older view?

AN OUTLINE OF RECENT DEVELOPMENTS

The new view of science could not get off the ground until the foundations of the dominant older view, positivism, had been shown to be untenable. The role that had been ascribed to observation—being the rock-bottom foundation of science and at the same time being the final arbiter of what could be believed—was reevaluated; and the relation between scientific theories and evidence was shown to be more complex than had been thought. The related view that science grows by steady accumulation of findings and theories was challenged by the work of Thomas Kuhn and subsequent scholars such as Lakatos and Feyerabend. Obviously, these matters are too complex to discuss in encyclopedic detail, but a few of the crucial issues can be highlighted.

1. It is clear to all except some of the more radical social constructivists—see chapter 11—that if the aim of science is to establish bodies of knowledge about the world, then somewhere in the process of doing science the world must be allowed to constrain or discipline our theories. But it has been recognized for many decades that the positivistic and operationalist view that all theoretical terms of science must be reducible to (i.e., definable in terms of) observational language is quixotic. The status of operationalism in the behavioral sciences was a hot issue in the decade immediately following the Second World War, and there were international symposia on the matter. A consensus was reached (except, of course, for a few diehards—an old story). The point was driven home that the theoretical concepts of science have meanings that transcend definition in observational terms; if this was not the case, science would have trouble in growing and extending into new areas. And it was realized that if the positivist/operationist view was accepted, it would have a chilling effect on theorizing about unobservable mechanisms such as the subatomic events that have won Nobel Prizes for so many physicists. (See the discussion of positivism in chapter 9.) Some logical positivists and fellow travelers even softened their views to make room for meaningful theoretical terms; thus, Carl Hempel, a somewhat "lapsed" logical positivist, drew the following enticing picture, which makes absurd the strict operationalist notion that concepts can each be reduced to a set of observation statements:

Scientific systematization requires the establishment of diverse connections, by laws or theoretical principles, between different aspects of the empirical world, which are characterized by scientific concepts. Thus,
the concepts of science are the knots in a network of systematic interrelationships in which laws and theoretical principles form the threads. . . . The more threads that converge upon, or issue from, a conceptual knot, the stronger will be its systematizing role, or its systematic import. (Hempel 1966, 94)

(It should be noted that there is another—perhaps even more attractive—account of theoretical terms, an account that realists can embrace but that positivists have to avoid. See Newton-Smith 1981, especially chapter 2. See also the discussion of the "causal theory" of the meaning of scientific terms near the end of chapter 3 in the present volume.)

But there is another issue about the role of observation. It has often been held that it is the "neutral court" that adjudicates between rival scientific claims; together with this has usually gone the belief that science is actually built upon the foundation of indubitable observation. (The operationalist thesis discussed before concerned the status of theoretical concepts, not their origin.) The crucial critical work here is that of N. R. Hanson, whose Patterns of Discovery (1958) has taken on the status of a classic. Hanson was not the first to have said the things that he said; Wittgenstein used the key illustration that Hanson used, and even Dewey made much the same point. But it was Hanson's work that for some reason fired imaginations.

Hanson's thesis may be stated in one sentence: "The theory, hypothesis, or background knowledge held by an observer can influence in a major way what is observed." Or, as he put it in a nice aphorism, "There is more to seeing than meets the eyeball" (Hanson 1958, 7). Thus, in a famous psychological experiment, slides were made from cards selected from a normal deck, and these were projected for very short periods onto a screen in front of observers. All were correctly identified, except for a trick slide that had the color altered (for example, it might have been a black four of diamonds). Most commonly, this slide was seen as a blur or as a black suit (spades or clubs). A Hansonian interpretation is that there is an interaction between the visual stimulus and the observers' background knowledge ("diamonds are red"), so the final result is that a blur is observed.

Subsequent writers have drawn a variety of conclusions from Hanson's thesis of theory-laden perception (although it should be noted in passing that some special cases where it does not hold—such as optical illusions—have been discussed in the recent literature; see Fodor 1984). For instance, many have taken it as supporting relativism—"There is no such thing as objective truth, for what observers take to be true depends upon the framework of knowledge and assumptions they bring with them." Sometimes an example is given that comes from Hanson himself: He imagined the astronomers Tycho Brahe and Johannes Kepler watching the dawn together; because they had different frameworks, one would see the sun moving above the horizon, while the other would see the Earth rotating away to reveal the sun. However, a closer reading of Hanson provides no succor for such an extravagant relativism, for he explicitly acknowledged that both astronomers would agree that what they actually observed during the dawn was the sun increasing its relative distance above the Earth's eastern horizon (Fodor 1984, 23). This acknowledgment is evidence that Hanson realized people with different frameworks have some views in common, views that can serve as the basis for further discussion and clarification of their respective positions—something a dedicated relativist has to deny.

A less extreme interpretation of Hanson, then, is that while we must be aware of the role played by our preconceptions, and while we have to abandon the view that observation is "neutral" and theory-free, there is nothing to force us to the conclusion that we cannot decide between rival claims and, therefore, cannot arrive at consensus about which viewpoint (or which observations) seem to be most trustworthy under the prevailing circumstances. Israel Scheffler put it well:

There is no evidence for a general incapacity to learn from contrary observations, no proof of a preestablished harmony between what we believe and what we see. . . . Our categorizations and expectations guide by orienting us selectively toward the future; they set us, in particular, to perceive in certain ways and not in others. Yet they do not blind us to the unforeseen. They allow us to recognize what fails to match anticipation. (Scheffler 1967, 44)

2. Over the last few decades it has become increasingly clear that scientific theories are "underdetermined" by nature; that is, whatever evidence is available about nature, it is never sufficient to rule authoritatively between the merits of rival theories. Or to put it in yet another way, a variety of rival theories or hypotheses can always be constructed that are equally compatible with whatever finite body of evidence is currently available. An implication of this, of course, is that we can never be certain that the particular theory we have accepted to account for the evidence is the correct one. (Some radical or "strong" social constructivists make
much of this point, which they misinterpret—see chapter 11.) Recently, however, Laudan has pointed out that it must not be assumed that all the rival or alternative theories that are *logically possible* will be equally plausible—in other words, he cautions that the argument from underdetermination might be overblown (Laudan 1990).

Several issues here are worthy of further comment (these are discussed at greater length in Phillips 1987):

a. The first point is illustrated by Nelson Goodman’s notorious example of “grue and bleen” (Goodman 1973). A large amount of observational evidence has accumulated over the ages concerning the color of emeralds; all that have been studied thus far have been found to be green. It might be supposed, then, that this amounts to irrefutable evidence for the hypothesis “all emeralds are green.” But the *very same* evidence also supports the hypothesis that “all emeralds are grue” (where “grue” is the name of a property such that an object is green up to a certain date, for instance, the year 2000, and blue thereafter—a thesis that readers of the new edition of this *Bestiary* will be able to empirically test!). The fanciful nature of this example is beside the point; it nicely illustrates the underdetermination of theory by available evidence, for it shows that a general theory (“emeralds are green”—i.e., always have been, and always will be”) necessarily goes beyond the finite evidence that is available (“the finite number of emeralds observed to date have been green”), thus leaving open the possibility that some ingenious scientist will come up with an alternative explanation for the very same finite set of data (maybe the crucial date is the year 3000).

b. A related issue here is that when new evidence necessitates that *some* accommodatory change has to be made in whatever theory is currently the favored one, there is *no one specific change that is necessitated*. Different scientists may change different portions of the theory—they are free to use their professional judgment and their creativity. It would be a mistake to interpret this as indicating that scientific theories are a matter of mere whim or individual taste; to stress that judgment is required is not to throw away all standards, it is just to stress that decisions cannot be made using some mechanical or algorithmic procedure. It is appropriate to point out here that John Stuart Mill had inklings of this point as long ago as the mid-nineteenth century; for, according Hilary Putnam, Mill said

> that one cannot do science by slavishly following the rules of Mill’s *Logic*. (There is no general method, Mill remarked, that will not give bad results “if conjoined with universal idiocy.”) (Putnam 1987, 73)

This general point is often made in terms of the “Duhem-Quine” thesis. Scientific theories, indeed vast areas of science, are interrelated; the image of science as a huge fishnet is a predominant one in much recent writing. It is this network as a whole, rather than little portions of it, that has to withstand the test of dealing with whatever evidence is gathered. Thus, it might appear that a piece of recalcitrant data offers a serious challenge to one particular section of the net, but the threat cannot be localized in this way; one scientist may react to the data by altering the “obvious” portion of the net, but others might want to preserve this piece and so might advocate changing some other portion of the net so as to accommodate the new information.

c. It might even be the case that when some counter-evidence turns up, scientists might decide to make no accommodatory changes at all—a course of action (or rather, a course of inaction) that receives the blessing of the new philosophy of science. For one thing, it might well be the case that one of the auxiliary assumptions that have to be made in any piece of scientific work is faulty, and scientists can blame one or other of these rather than accept the counter-evidence at face value and be forced to change their net. In doing laboratory work, for example, it is often assumed that the chemical samples that are being used are pure, or that there were no temperature fluctuations, or that the testing equipment was reliable, or that an observer was unbiased, or . . . (See Popper 1985, chap. 10, for an early discussion of auxiliary hypotheses.)

On the other hand, scientists might ignore the counter-evidence in the hope that “something eventually will turn up that will explain it.” It was a traditional tenet of methodology that a scientist must abandon a theory, no matter how attractive it might appear, once some counter-evidence became available. It now appears, however, that there are good reasons to suppose that it can be quite rational to adhere to the theory even under these adverse conditions. Paul Feyerabend has been the most forceful writer on this and related issues:

> The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science gets into considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events . . . On the contrary, we see they are necessary for progress. (Feyerabend 1970, 21-22)
Even Popper, the arch proponent of falsification, has stressed that negative or refuting evidence is never absolutely binding; the scientist has to make a methodological decision to accept the evidence as valid—and sometimes it is reasonable not to take this action. But, of course, Popper recommends that we adopt the rule that, in general, refuting evidence be accepted (see Popper 1985, chap. 10). Imre Lakatos devised his “methodology of scientific research programs” in an alternative attempt to gauge when changes made in an ongoing research tradition were progressive or degenerative (Lakatos 1972).

3. Perhaps the most famous feature of the new philosophy of science, however, has been its focus upon the dynamics of science. The process of scientific change has come under increasing investigation since Kuhn’s work on scientific revolutions popularized the notion of “paradigm clashes.” Science is not static; theories come and theories go, and new data accumulates and old findings are interpreted in new ways. As Newton-Smith put it, “viewed specie eternitas scientists (even physical scientists) are a fickle lot. The history of science is a tale of multifarious shiftings of allegiance from theory to theory” (Newton-Smith 1981, 3). And involved in all this is the question of the rationality of change—what justifies scientists in throwing out old ideas and accepting new ones? There has been much debate here, but little consensus—witness, for example, the work of Kuhn, Popper, Lakatos, Feynabend, Toulmin, Laudan, and Newton-Smith (see previous citations, plus Laudan, 1977; and Newton-Smith 1981). It will suffice to quote a brief passage from Popper to illustrate this major theme in the new positivist philosophy:

I assert that continued growth is essential to the rational and empirical character of scientific knowledge; that if science ceases to grow it must lose that character. It is the way of its growth which makes science rational and empirical, the way, that is, in which scientists discriminate between available theories and choose the better one. (Popper, 1961, 215)

QUESTIONS AND ANSWERS

There are some who have drawn a dangerous moral from the developments just outlined. Science has fallen from its pedestal; and if no knowledge can be justified totally and unchallengeably, then no claims to have attained knowledge can be disbarred. The rocky road to relativism is embarked upon. But it is possible to retain a hopeful outlook and even to relish the challenge that this new picture of science presents. It is here that we can obtain succor from the fields of program evaluation and action research in the applied social sciences. Investigators here do not lose heart, yet they are faced with a reality that (we now realize) closely parallels that of “pure” scientists; and some even thrive on the uncertainties of their field. The ideal that is embraced seems to be this: Seekers after enlightenment in any field do the best that they can; they honestly seek evidence, they critically scrutinize it, they are (relatively) open to alternative viewpoints, they take criticism (fairly) seriously and try to profit from it, they play their hunches, they stick to their guns, but they also have a sense of when it is time to quit. It may be a dirty and hard and uncertain game, but with no fixed algorithms to determine progress, it is the only game in town.

Although to the present author this seems a modest, nondogmatic, unsurprising, and eminently reasonable position, there are many who feel uneasy and who continue to raise questions about it. So it might be fruitful to grapple with some of these directly.

QUESTION 1: In what sense is the new position that has been outlined here “postpositivist”? Isn’t it merely a weaker form of positivism in disguise? It may have come after positivism, and that is the only reason for calling it “post” positivism.

ANSWER: In no sense is the new philosophy of science—broad and ill-defined though it is—closely akin to positivism (or, more especially, to the most notorious form of positivism, logical positivism). Logical positivism became discredited in the years immediately following the end of the Second World War; few if any philosophers these days subscribe to its core tenet, the “verifiability criterion of meaning,” according to which a statement is meaningful only if it is verifiable in terms of sense experience (excepting logical mathematical propositions). (For more discussion of this topic, see chapter 9.) One of the serious problems associated with the use of this principle in science was that it made theoretical terms meaningless, for the fact is that many theoretical entities cannot be verified in terms of sense experience; but there are few today who would want to argue that the discourse of subatomic particle physicists or of black hole theorists is meaningless! The fact of the
matter is that the logical positivists were, by and large, antirealists who held—or came close to holding—some form of instrumentalism.

QUESTION 2: Aren’t contemporary postpositivists clinging to an old and outmoded realist paradigm?

ANSWER: This question embodies a serious confusion. The old positivist view was antirealist; as explained in the previous answer, the logical positivists (on the whole) denied the reality of theoretical entities and indeed claimed that talk of such entities was literally meaningless (some took refuge in a position about theories similar to the one cited earlier from Hempel). Modern realism is a recent, postpositivistic development. Furthermore, there is little consensus within the philosophical community; whether or not realism is viable is a hotly debated topic—many contemporary philosophers are for it, but many are against it. (A leading postpositivistic antirealist is Bas van Fraassen 1980; but his grounds for antirealism are not those of the logical positivists.) There is even controversy about the precise definition of realism: Arthur Fine has written:

Given the diverse array of philosophical positions that have sought the “realist” label, it is probably not possible to give a sketch of realism that will encompass them all. Indeed it may be hopeless to try, even, to capture the essential features of realism. (Fine 1987, 359)

There is a nice passage in Hilary Putnam’s “Paul Carus Lectures” that highlights these complexities:

Thus, it is clear that the name “Realism” can be claimed by or given to at least two very different philosophical attitudes (and, in fact, to many). The philosopher who claims that only scientific objects “really exist” and that much, if not all, of the commonsense world is mere “projection” claims to be a realist, but so does the philosopher who insists that there really are chairs and ice cubes . . . and these two attitudes, these two images of the world can lead to and have

led to many different programs for philosophy. (Putnam 1987, 4)

QUESTION 3: Well, old or new, many influential postpositivists are realists. Aren’t they overlooking the fact that multiple realities exist, and aren’t they overlooking the well-known fact that each society constructs its own reality? If you accept these two points, you cannot be a realist! Consider merely one example; Egon Guba has written that social scientists are studying phenomena that are

social in nature. There is no need to posit a natural state of affairs and a natural set of laws for phenomena that are socially invented—I shall say socially constructed—in people’s minds. I suggest an ontology that is relativist in nature. It begins with the premise that all social realities are constructed and shared through well understood socialization processes. It is this socialized sharing that gives these constructions their apparent reality. (Guba 1990, 89)

ANSWER: There are several important issues here, some of which were touched upon in the earlier discussion. (See also the discussion in Bunge 1992, section 9.)

1. In the first place, this question seems inspired by an extreme reading of Kuhn—the view that all of us are trapped within some particular paradigm and that we cannot converse rationally with those in other paradigms because our beliefs are incommensurable. Even the later Kuhn—the Kuhn of The Essential Tension—did not accept this extreme relativism. (Furthermore, such relativism seems contradicted by everyday experience within science. Freudians do understand—but, of course, disagree with—Skinnerians, and neo-Marxist social scientists understand colleagues of more conservative bent, and vice versa.)

2. Second, there are a number of things that get run together illiciely in discussions about “reality.” First, there is a simple confusion here between, on the one hand, the fact that different people and different societies have different views about what is real (a fact that seems undeniable) and, on the
other hand, the issue of whether or not we can know which of these views is the correct one (or, indeed, whether there is a correct one at all). From the fact that we might not be able to reach agreement (an epistemological matter), it does not follow that there is more than one “reality” (an ontological matter). Second, it is clear that on some issues what is regarded or judged to be “real” depends upon which conceptual apparatus is available—one group, for example, may only have concepts or classificatory schemes that recognize three types of snow, while a different group might make distinctions between ten types, and the result will be that the two groups differ with respect to the question “How many types of snow exist?”

Now, the relativist seems to be committed to the view that all such differing views are correct—that is, there really are three types of snow and ten types of snow; whereas the realist is only committed to the minimal (and more informative) view that something, “snow,” really exists although different groups conceptualize it differently. The realist (qua realist) is not forced to say which of these conceptualizations is “correct”; indeed, it is a viable position for the realist to say that it is a silly question to ask which conceptualization is correct, for different conceptualizations do different work in different communities.

There is, however, another type of situation where the realist will want to take a stronger stand. This is the (perhaps rare) situation where groups or different individuals are using terms in the same sense. To make this a little more precise: Suppose that one social group believes that “X is the case,” and another group believes—in the very same sense of X—that “not-X is the case.” The realist holds that both of these views cannot be correct, although, of course, some people believe one or the other of these to be true; it is either the case that X or not-X, but not both, is true. (The realist does not have to believe that we can always settle which of these views, X or not-X, is true; the issue is whether both or, at best, only one can be true.) The relativist has to hold that, in this situation, there are multiple realities—that reality is both X and not-X—for if the relativist does not hold this, then his or her position dissolves into the realist position. Stated thus boldly, it can be seen that the relativist case here hinges on obscuring the distinction between “what people believe to be true” and “what really is true, whether or not we can determine this truth at the moment.” (This confusion is discussed further in Phillips and Burbules 2000; and, of course, here are many other problems with relativism—see Siegel 1987.)

3. Third, it is important to note that this issue dividing realists from relativists is not the same as the issue (discussed earlier) that separates realists from antirealists; the second of these is the issue (broadly speaking) of the reality or otherwise of theoretical entities (that is, the status of the entities referred to in theories such as those found in the field of particle physics). There is, as might be expected with such complexities, a tendency for the neophyte to run these two sets of issues together! (See, for the second of the two issues mentioned, Leplin 1984.)

4. Finally, this third question raises the very important matter of the social construction of reality, touched upon briefly earlier (and discussed in more detail in chapter 11). Certainly, there is nothing in postpositivism, per se, that requires denying that societies determine many of the things that are to count as real for their members; what things are taken to be real depends upon the concepts and classifications available within those societies. Thus, a “primitive” society may define certain spirits as being real, and the members of that society might accept them as real and act accordingly. A similar thing certainly happens in our own society and not just with spirits. All a postpositivist would want to insist upon is that these matters can be open to research: We can inquire into the beliefs of a society, how they came about, what are their effects, and what is the status of the evidence that is offered in support of the truth of the beliefs. And we can get: these matters right or wrong—we can get our descriptions of these beliefs right or wrong, or we can be right or make mistakes about their origins or their effects. It simply does not follow from the fact of the social construction of reality that scientific inquiry becomes impossible or that we have to become relativists. And certainly, it does not follow from the fact that a tribe of headhunters socially determines its own reality, that we thereby have to
accept that reality as true. What is true—if we have done our research properly—is that the members of that tribe actually do believe in their own "realities." But that is a different issue, one that raises no great problem of principle for positivists. Thus, Popper, one of the major positivists (and the man who claimed to have been the person who killed positivism), stressed that his philosophy "assumes a physical world in which we act," although he added that we may not know very much about it. But, he wrote, it was also necessary to "assume a social world, populated by other people, about whose goals we know something (often not very much), and, furthermore, social institutions. These social institutions determine the peculiarly social character of our social environment" (Popper 1976, 103). Popper includes laws and customs among "institutions."

QUESTION 4: Given the acceptance by positivists of Hanson’s thesis concerning the theory-ladenness of perception, and given the general nonfoundingist tenet that nothing can be considered as absolutely certain, and so forth, does it not follow that positivists have to abandon the notion of objectivity? Hasn’t it been stripped of any meaning that it might have had?

ANSWER: Certainly not! The notion of objectivity, like the notion of truth, is a regulative ideal that underlies all inquiry. (For further discussion of this issue, see chapter 7.) If we abandon such notions, it is not sensible to make inquiries at all. For if a sloppy inquiry is as acceptable as a careful one, and if any inquiry that is careless about evidence is as acceptable as an inquiry that has taken pains to be precise and unbiased, then there is no need to inquire—we might as well accept, without further fuss, any old view that tickles our fancy.

Now, it is true that the fact that an inquiry is objective does not guarantee its truth—it was shown earlier that nothing can guarantee that we have reached the truth. Perhaps an analogy will help to clarify matters: Consider two firms that manufacture radios; one is proud of its workmanship and backs its products with a strong guarantee; while the other firm is after a quick profit, practices shoddy workmanship, and does not offer any warranty to the buyer. A consumer would be unwise to purchase the latter’s product, but nevertheless it is clearly understood that the first firm’s guarantee does not absolutely mean that the radio will not break down. The fact that this situation exists is not taken by consumers as invalidating the notion of a warranty, nor is it seen as making each purchase equally wise. And the very same situation exists in science.

The Popperian account of objectivity is widely, though not universally, accepted by positivists. The following sentences capture the essence of his approach:

What may be described as scientific objectivity is based solely upon a critical tradition which, despite resistance, often makes it possible to criticize a dominant dogma. To put it another way, the objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism, of the friendly-hostile division of labour among scientists, of their cooperation and also of their competition. For this reason, it depends in part, upon a number of social and political circumstances which make criticism possible. (Popper 1976, 95)

CONCLUSIONS

It can be seen from the foregoing that positivism is a broad, complex, and dynamic approach to understanding the nature of science. There is little unanimity on important issues among its “adherents” (if people can be said to adhere to so amorphous a position)—but this is a healthy feature and not a weakness. Paul Feyerabend wrote, more than a quarter-century ago, that unanimity of opinion may be fitting for some church, or for the followers of a tyrant, but it is most unfitting for science (Feyerabend 1970, 33).

The danger to positivism comes not from internal dissension, but from outside—from those who draw false, and often oversimple, conclusions from some of the very same developments that have produced positivism itself.