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Postpositivistic Science
Myths and Realities

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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 is 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, he believed strongly 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, p. 101)

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—"nonjustificationist." 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 pre-

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ferred not to use the term *truth* but, instead, the term *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 Furthermore, this change of language highlighted the fact that a warrant is not forever; today's warrant can be rescinded tomorrow, following further inquiry.

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 no longer can it be claimed there are any *absolutely* authoritative foundations upon which scientific knowledge is based (hence the other title often given to contemporary epistemology—"nonfoundationalistic"). The fact is that many of our beliefs are warranted by rather weighty bodies of evidence and argument, and so we are justified in holding them; but they are not *absolutely* unchallengeable.

This view of science fits comfortably with what every experienced action researcher and evaluator of social programs has come to understand about his or her own work; these 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 evaluation 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—that it was both the rock-bottom foundation of science and, at the same time, 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 full, but a few of the crucial issues can be highlighted.

Observation

It is clear (to all except some mystics) 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 studied or observed. But it has been recognized for many decades that the positivistic and operationalistic 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 World War II, and there were international symposia on the matter. A consensus was reached (except, of course, for a few diehards—an old story): If the positivist/operationalist view were to be 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. Carl Hempel, a somewhat "lapsed" logical positivist, drew (in his postpositivist years) the following enticing picture that makes absurd the 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, p. 94)

Thus the point was driven home that the theoretical concepts of science have meanings that transcend definition in observational terms, and it was realized that, if this were not the case, science would have trouble in growing and extending into new areas.

There is another issue about the role of observation. It has often been held that observation 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*. That is, according to the operationalist view, theoretical concepts had the status of being shorthand summaries of observation statements, no matter how these theoretical concepts happened to have originated.) The crucial work that challenged the view that observation is the "theory-neutral" basis on which science is erected was that of N. R. Hanson, where *Patterns of Discovery* (1958) has become a classic. Hanson was not the first to say 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 fired most imaginations.

Hanson's theories 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, p. 7). In other words, observation is theory laden—it is not a theory-neutral foundation. Thus, in a famous psychological experiment, sliders 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 be a black four of diamonds). Most commonly this slide was *seen* as a blur or else 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 work; 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 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 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 (Hanson, 1958, p. 23); but, of course, they would insist on talking about what they had observed in different terms. This acknowledgment is evidence that Hanson realized people with different frameworks nevertheless can have some views—or can hold some data—in common, and these things can serve as the basis for further discussion and clarification of their respective positions. Thus there is little comfort here for relativists.

A less extreme interpretation then is that, while we must be aware of the role played by our preconceptions in influencing our observations, and while we have to abandon the view that observation is "neutral" or theory free, there is nothing in Hanson that forces us to the conclusion that we cannot decide between rival claims and cannot arrive at consensus about which viewpoint (or which observations) seem to be most trustworthy under the prevailing circumstances. Israel Scheffler (1967, p. 44) put it well:

> There is no evidence for a general incapacity to learn from contrary observations, no proof of a pre-established 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.

Theory and Evidence

Over the past few decades, it has become increasingly clear that scientific theories are "underdetermined" by nature; that is, whatever evidence is available (or possibly could be available) about nature, it is never sufficient to rule out the possibility that a much better theory might be devised to account for the phenomena that our presently accepted theory also explains. Or, to put it 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 is the correct one!) There are several developments that are worthy of brief comment.[1]

The first point is illustrated by Nelson Goodman's notorious example of "grue and bleen" (Goodman, 1973), although it should be noted that Goodman made slightly different use of this case. A large amount of observational evidence has accumulated over the ages concerning the color of emeralds; all that have been studied 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). 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," that is, "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.

A related issue concerns what happens when new evidence turns up necessitating the making of some accommodatory change in whatever theory is currently the favored one. Postpositivists now generally recognize that 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. Rather, it is to stress that decisions cannot be made using some mechanical procedure.

This 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. It might appear that some 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, while others might want

to preserve this piece and so might advocate changing some other portion of the net to accommodate the new information. Once again scientists must use their professional judgment; decisions about how to modify theories cannot be made mechanically.

It might even be the case that, when some counterevidence 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 is faulty. Many such assumptions have to be made in any piece of scientific work. For example, in doing laboratory work, the auxiliary assumption is often made that the chemical samples being used were pure, or that there were no unplanned temperature fluctuations, or that the psychological tests being used were reliable, or that an observer was unbiased, and so on. Scientists can blame one or another of these rather than accept the counterevidence at face value and thereby be forced to change their net.

On the other hand, scientists might simply ignore the counterevidence in the hope that "something will eventually turn up to 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 counterevidence became available. It turns out, 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 (1970, pp. 21-22) 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.

Imre Lakatos (1972) devised his "methodology of scientific research programs" in an attempt to gauge when changes made in an ongoing research tradition are progressive or degenerative.

Scientific Change

Perhaps the most famous feature of the new philosophy of science, however, is its focus upon dynamics. 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, new data accumulate, and old findings are interpreted in new ways. 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, but little consensus, among the postpositivists—witness the work of Kuhn (1970), Popper (1968a), Lakatos (1972), Feyerabend (1970), Toulmin (1970a, 1970b), Laudan (1977), and Newton-Smith (1981). It will suffice to quote a brief passage from Popper to illustrate this major theme in the new postpositivist 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, 1968a, p. 215)

Questions and Answers

There are some who have drawn a dangerous moral from the developments just outlined. Science has fallen from its pedestal; if no knowledge can be totally and unchallengeably justified, then nothing can be disbarred. We have embarked on the rocky road to relativism. 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 evaluation and action research. People 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. Seekers after enlightenment in any field do the best that they can; they honestly seek evidence, they critically scrutinize it, they are open to alternative viewpoints, they take criticism 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, hard, and uncertain game, but it is the only game in town.

Although, to me, this seems a modest, nondoctrinaire, 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, which has been outlined above, "postpositivistic"? Isn't it merely a weaker form of positivism in disguise? (The position certainly shares some features in common with positivism.) It may have come *after* positivism, and that is the chief 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 World War II; 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 logico-mathematical propositions).[2] As was pointed out earlier, one of the serious problems associated with the use of this principle in science was that it made theoretical terms meaningless. The fact is that many theoretical entities cannot be verified in terms of sense experience; neither can laws be confirmed absolutely (for they make universal claims that cannot be verified); but there are few today who would want to argue positivistically that the discourse of subatomic particle physicists or of black-hole theorists is meaningless!

A historical note might be helpful here. In the opening sentences of a paper written in 1956, when positivism was in its death throes, the major logical positivist Rudolf Carnap said that one of his main topics was going to be

> the problem of a criterion of significance for the theoretical language, i.e., exact conditions which terms and sentences of the theoretical language must fulfill in order to have a positive function for the explanation and prediction of observable events and thus to be acceptable as empirically meaningful. (Carnap, 1956, p. 38)

Carnap indicated his optimism (not shared by many others in the mid-1950s) that he would still be able to draw the line that "separates the scientifically meaningful from the meaningless" (Carnap, 1956, p. 40). A few years later, in the same publication series, Grover Maxwell wrote what must be considered the majority antipositivist opinion:

> That anyone today should seriously contend that the entities referred to by scientific theories are only convenient fictions, or that talk about such entities is translatable without remainder into talk about sense contents or everyday physical objects . . . strike(s) me as so incongruous with the scientific and rational attitude and practice that I feel this paper should turn out to be a demolition of straw men. (Maxwell, 1962, p. 3)

Question 2. Aren't contemporary postpositivists clinging to an old and outmoded realist paradigm?

Answer. The 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. Modern realism is not a carryover from positivism but 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—there are many contemporary philosophers for it, but there are many against it.[3] There is even controversy about the precise definition of realism; Arthur Fine (1987, p. 359) 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.

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! Egon Guba has written that educational researchers (if not all social researchers) 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, in press)

Answer. There are several important issues here, some of which were touched upon in the earlier discussion. In the first place, this question seems inspired by an extreme reading of Kuhn—the view that all of us are trapped within a 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* (1977) or of the "Postscript" to the second edition of *The Structure of Scientific Revolutions* (1970)—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. The point is that paradigms (if one accepts this controversial notion[4]) serve as lenses, not as blinders.

Second, there is a 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). The relativist is committed to the view that *all* such differing (and contradictory) views are correct (or could be correct at one time), whereas the realist is committed to the view that at best only one view can be right (of course, all views might have portions that are sound or all might be wrong.)[5]

To make this a little more precise: Suppose that one social group believes that "X is the case," and another group believes 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 the case either that X, or that not-X, but not both. (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 there are multiple realities—that reality is (or could be) both X and not-X—for, if the relativist does not hold this position, 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 true" and "what really is true, whether or not we can determine is truth at the moment."[6]

Third, it is important to note that there are several quite different ues concerning realism, which the neophyte tends to run together, using a great deal of confusion. The issue discussed directly above ncerns whether, and in what sense, multiple realities exist; the ponents of realists here can be correctly labeled as relativists. A fferent issue was discussed earlier: The point in contention was hether or not theoretical entities (such as those postulated in the eories of particle physics, or in Chomskian linguistics, or in theories cognitive psychology) can be said to be real; here the opponents of alists are properly labeled as antirealists. It is crucial to note that ese antirealists are in no sense relativists. Thus it is a serious flaw in holarship to claim that, because, in contemporary philosophy of ence, there is much debate about the viability of realism, relativism ereby takes on more respectable status. The current debates in ilosophy of science are between realists and antirealists, not be- een realists and relativists (Leplin, 1984; Siegel, 1987).

Finally, this third question raises the very important matter of the cial construction of reality. Certainly there is nothing in postpositiv- n per se that requires denying that societies determine many of the ings that are *believed* to be real by their members. Thus an "exotic" ciety may define certain spirits as being real, and the members of at society may accept them as real and act accordingly. A similar ing certainly happens in our own society, and not just with spirits. l a postpositivist would want to insist upon is that these matters n be open to research: We can inquire into the beliefs of a society, w they came about, what their effects are, and what the status is of e evidence that is offered in support of the truth of the beliefs. And e can get these matters right or wrong—we can describe these beliefs rrectly or incorrectly, or we can be right or make mistakes about eir origins or their effects. It simply does not follow from the fact of e social construction of reality that scientific inquiry becomes im- ssible or that we have to become relativists. It does not follow from e fact that a tribe of headhunters socially determines its own beliefs e., the things the members of that group *believe* to be real) that we ereby have to *accept* those beliefs as *true*. What is true—if we have ne our research properly—is that we have accurately determined at the members of the tribe do *believe* in their realities. But that a different issue, which raises no problem of principle at all for

postpositivists. (In a similar vein, it is clear that Freudians believe in the reality of the id and superego and the rest, and they act as if these are realities; but their believing in these things does not make them real.)

It is worth noting that, for decades, postpositivists have accepted this notion of "the social construction of reality." Thus Sir Karl Popper, one of the major postpositivists (it is relevant to note that he 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, crucially, he stressed 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, p. 103). Popper includes laws and customs among "institutions."

Question 4. Given the acceptance by postpositivists of Hanson's thesis concerning the theory ladenness of perception, and given the general nonfoundationalist tenor that nothing can be considered as absolutely certain, and so forth, does it not follow that postpositivists 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 (Phillips, in press). 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 an 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 objectivity of an inquiry does not guarantee its truth—as was shown earlier, *nothing* can guarantee that we have reached the truth. Perhaps an analogy will help to clarify matters: Consider two firms who manufacture radios; one is proud of its workmanship and backs its products with a strong guarantee; 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 postpositivists. 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 co-operation 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, p. 95)

Conclusion

It can be seen from the foregoing discussion that postpositivism 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 (1968, p. 33) wrote, 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.

The danger to postpositivism 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 postpositivism itself.

Notes

1. Many of the following issues are discussed at greater length in Phillips (1987b).
2. For more details on the complicated demise of positivism, see Phillips (1983).
3. A leading postpositivist antirealist is Bas van Fraassen (1980). His *grounds* for antirealism are not those of the logical positivists.
4. It is far from clear that the notion of paradigms as developed by Kuhn is sustainable; see the books by Phillips, Newton-Smith, and Siegel quoted elsewhere in this chapter.

5. It must be stressed that this point holds only for views that are contradictory or opposing, such as "there is an X" and "there is not X." If the views are orthogonal, that is, noncontradictory, there is no problem with them both being true—"there is an X" and "there is a Y" present no problems.
6. There are many other problems with relativism; see, for example, Siegel (1987).

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