SCIENCE AND PSEUDOSCIENCE: CRITERIA OF DEMARCATION

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DRAWING LINES between science and pseudoscience is a complicated matter. In this paper I discuss three of the complications. First, how strict should criteria of demarcation be? Second, can demarcation decisions be made without recognizing a distinction between science as a human activity and science as a collection of systems, theories, hypotheses, and propositions? Third, is there a difference between descriptive criteria and legislative criteria, and does this have a bearing on the current controversy concerning the role of historical studies in the philosophy of science?

DEMARCATION AND ACCEPTABILITY

In this section I shall argue that some proposed criteria of demarcation are inadequate because they are too strict. These criteria would make it impossible for there to be incorrect or unacceptable scientific hypotheses.[1] For example, there is presently a dispute concerning the identity of the first people to inhabit the North American continent. One group of scientists is trying to establish that the first people to cross the land bridge that is now covered by the Bering Strait were the first people in North America. Another group is trying to establish that there already were people in North America who were overrun by the Bering Strait crossers. At most, only one of these hypotheses is acceptable, yet both are clearly scientific. I make this elementary point because some philosophers and scientists have proposed criteria of demarcation so narrow that only *acceptable* hypotheses are allowed to be scientific.

I wish to suggest the following test for whether or not a proposed criterion of demarcation is adequate. It is derived from a similar test stated by Hempel.[2] If, under a proposed criterion of demarcation, a hypothesis is unscientific, then so must be its negation. I shall argue that by using this test, several recently proposed criteria of demarcation are inadequate; that is, they are such that a

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hypothesis is determined to be nonscientific, yet its negation *is* arguably scientific.

As an initial example of a criterion that is inadequate by the above principle, consider the following: Every science that is a science has hundreds of hard results, but search fails to turn up a single one in "parapsychology."[3] The criterion used here may be spelled out in this way: A hypothesis is scientific if and only if it has at least one "hard result." A hard result, we may stipulate, is a confirming experiment or test. In this sense, the hypothesis that gravity obeys an inverse square rule does have hundreds of hard results.

Now, to test the adequacy of this criterion, we may formulate a very weak hypothesis of parapsychology, namely, some people have the ability to "know" what will happen before it happens. (The obvious vagueness of this hypothesis will not affect the present point.) This hypothesis has not one "hard result," according to Wheeler, and therefore is not scientific by the criterion proposed. If the "hard results" criterion is to be adequate, the negation of the precognition hypothesis must be unscientific as well. But, on the contrary, the negation—that is, no one has the power of precognition—is scientific by the criterion; there are hundreds of hard results showing people *not* to have precognition. People fail to predict the future with alarming regularity. So, the proposed criterion fails the test and is thus not an adequate criterion of demarcation.

A second and slightly more complex case is this. Paul Kurtz in his article "Is Parapsychology a Science?" offers the following as "an essential criterion of a genuine science," namely, "the ability to replicate hypotheses in any and all laboratories and under standard experimental conditions."[4] This criterion of replicability is, I shall argue, not an adequate criterion of demarcation.

To see this, consider another hypothesis of parapsychology, that if certain gifted subjects are tested with Zener cards (the familiar cards with simple geometrical shapes on them), they will realize above-chance calls fairly consistently. This hypothesis is unscientific by the replicability criterion because "Some experimentors—a relatively few—are able to get similar results, but most are unable to do so."[5] If we now use the test formulated above, we must ask, by the replicability criterion, is the negation of this hypothesis unscientific? Is the hypothesis that it is not the case that certain gifted individuals will realize above-chance calls unscientific? No, because this hypothesis *is* replicable in any laboratory. What Kurtz should be saying is that parapsychology is unacceptable because its hypotheses are not replicable. He should not, however, assert solely on this basis that parapsychology is pseudoscientific.

Other examples of this use of too-strict criteria are readily found. Several of the criteria given by Lafleur are like this. For example, to require that in every case in which the new hypothesis is in contradiction with established theory, the new include or imply a suitable substitute, is to require that the new hypothesis be as acceptable as the one it would replace. But the question of the scientific nature of the hypothesis is another question altogether.

The point of this adequacy test is that it should be much more difficult for a hypothesis to be accepted as true or highly confirmed than for a hypothesis to be accepted as scientific according to some demarcation criterion. A scientifically acceptable hypothesis has presumably already passed demarcation tests. Many hypotheses are proposed that are scientific but ultimately unacceptable. Failed scientific hypotheses tend to be quickly forgotten, but several examples should jar the memory. The hypothesis of the fixity of the main continental land masses was accepted by geologists for at least 50 years. This hypothesis has recently been rejected in favor of the continental drift hypothesis. The former hypothesis is unacceptable but surely not for *this* reason unscientific. Other examples of failed scientific hypotheses include those concerning the existence of phlogiston and spontaneous generation.[6] Having made this first crucial clarification, let us now turn to demarcation criteria proper and see what differences can be found among them.

Psychological Criteria

Psychological criteria are admittedly criteria of demarcation. But the line they draw is not between science and pseudoscience; it is between scientists and those people purporting to be scientists: cranks. A psychological criterion is one that identifies some psychological state, disposition, or character trait as characteristic of cranks and not scientists. In this section I shall report some such criteria that have been identified and argue that, while such criteria are undeniably useful, they are logically irrelevant to the problem of demarcating science and nonscience.[7]

Laurence Lafleur in his "Cranks and Scientists" lists seven questions that, as he says, "will help us to make up our minds as to whether the person is a scientist or a crank."[8] Only two of Lafleur's seven are actually psychological criteria, however; the others concern, not the proposer of a hypothesis, but the hypothesis itself. Two of Lafleur's seven questions nicely illustrate the *difference* between psychological criteria and criteria concerned with hypotheses, theories, etc. The first is psychological; the second is not.

> Is the proposer of the hypothesis aware of the theory he proposes to supersede?
>
> Is the new hypothesis in accord with currently held theories in the field of the hypothesis, or, if not, is there adequate reason for making the changes, reasons of weight at least equal to the weight of the evidence for the existing theories?[9]

The first of these questions is a good example of a psychological criterion, inasmuch as it concerns the proposer's knowledge. The second, however, is clearly not of this character, as it concerns the hypothesis itself and its relation to established theories.

The other psychological criterion identified by Lafleur is this:

> Does the proposer show a disposition to accept minority opinions, to quote individual opinions opposed to current views, and to overemphasize the admitted fallibility of science?[10]

Lafleur argues, based on these tests, that Immanuel Velikovsky is a crank. He argues that Velikovsky is not aware of the theories he would overthrow and is disposed to accept minority opinions, etc. Lafleur, of course, is aware of the possibility that some scientists may satisfy some or all of his tests. These tests are not absolute. There are probably scientists who would fail these tests and cranks who would pass them.

Martin Gardner in his classic *Fads and Fallacies in the Name of Science* identifies two main traits of cranks.[11] First, they work in almost total isolation from their colleagues. Second, cranks have a tendency toward paranoia. Many of the odd characters one encounters in Gardner's book certainly satisfy these requirements as well as those set out by Lafleur.

All of the psychological criteria mentioned so far are subject to challenge. They are extremely vague and therefore hard to apply. There are bound to be a few isolated and paranoid scientists. But my concern is not with the adequacy or inadequacy of these criteria; rather, I am concerned with a fallacious argument that illicitly mixes the kinds of criteria I am attempting to separate. To establish that a person is a crank, one must appeal to psychological criteria. To establish that a hypothesis or theory is pseudoscientific, one may not appeal solely to psychological criteria.

This distinction is useful in two ways. First, as I have asserted, it militates against fallacious arguments of the form: This person is a crank, so this person's theories are pseudoscientific. Clearly there is no logical force to these arguments; it is not contradictory to imagine a crank proposing a scientific theory or a pseudoscientific theory proposed by a scientist. Yet even though such arguments lack logical force, they do nevertheless have *some* force. The distinction between psychological criteria and logical criteria allows us to formulate the important question: What does the psychology of a person have to do with the credibility of that person's theories? There are connections between scientific theories and the proposers of those theories, connections that need examination. And this examination, which is beyond the scope of the present paper, cannot begin until the former distinction is recognized.

Descriptive and Legislative Criteria

When we move away from psychological criteria and begin examining those criteria applicable to hypotheses, theories, etc., we find immediately a dispute concerning the scope of proposed criteria. Some philosophers have argued that it is a mistake to try to decide whether or not a single hypothesis or theory is scientific. Others have argued that this is both possible and desirable. Those of the former opinion urge that only research programs or traditions can be evaluated regarding their scientific value. Lakatos, for example, says: "It is a succession of theories and not one given theory which is appraised as scientific or pseudo-scientific."[12] More recently Larry Laudan has argued that

> most philosophers of science have mistakenly identified the nature of scientific appraisal, and thereby the primary unit of rational analysis, by focussing on the individual theory, rather than what I call the *research tradition.*[13]

Thomas Kuhn, an early proponent of this view, holds that one necessary condition for a field's being scientific is that it generate puzzles the solutions of which "must be a challenging task, demanding, on occasions the very highest measure of talent and devotion."[14]

These views may be summarized in the thesis that demarcation decisions are only possible for units of analysis significantly broader than the individual theory or hypothesis. For Lakatos, the unit is the research program; for Laudan, the research tradition; and for Kuhn, the puzzle-solving activity of normal science.

I shall refer to such criteria of demarcation as *descriptive* criteria. Such criteria are essentially temporal; they refer to historical periods. They are often couched in terms of scientific progress—a notion that makes no sense if divorced from the temporal. On the other hand, proposals of rules or maxims for deciding whether or not a new hypothesis or theory is scientific are termed legislative criteria by Grünbaum. Legislative criteria are "regulative ideals," similar in some respects to moral principles. Legislative criteria are essentially atemporal; they can in principle be applied to a hypothesis without regard to historical content or considerations.

Hempel's criterion of testability-in-principle, stated in *Philosophy of Natural Science*, will serve as an illustration of a legislative criterion.

> But if a statement or set of statements is not testable at least in principle, in other words, if it has no test implications at all, then it cannot be a scientific hypothesis or theory, for no conceivable empirical findings can then accord or conflict with it.[15]

Notice that Hempel's criterion is applicable to statements or hypotheses and not restricted to "series of theories" or traditions. Notice also that one does not have to place a hypothesis or theory in a historical context before one can apply this criterion. In this sense, it is atemporal. Notice, finally, that Hempel's criterion is not a generalization based on an examination of hypotheses agreed to be scientific. Hempel is defining "scientific hypothesis."[16] He is using "cannot be scientific" in its strongest possible sense: hypotheses that are not testable in principle ought not to be considered scientific. Hempel's argument for his criterion is nowhere based on a historical study of scientific hypotheses. Rather, it is presented as a definition or, more broadly, as a characterization of "empirical import."

Making a distinction between descriptive and legislative criteria of demarcation makes it possible to diffuse a persistent controversy in the philosophy of science. This controversy is embodied in a basic criticism Kuhn makes of Popper's criterion of demarcation (one that I regard as a legislative criterion) and the reply Popper himself makes to this criticism. Kuhn argues that

> a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may be just in that part of science which Sir Karl ignores.[17]

Kuhn's criticism is that, historically, the testing required by Popper's criterion does not often occur. Popper's response? It ought to! In his revealingly entitled paper "Normal Science and Its Dangers," Popper says of normal science that unfortunately it does exist but it should not. He says normal scientists have been "taught badly" and that we "ought to feel sorry for" them.[18] Such normal science is dangerous to both science and civilization.

Clearly, Kuhn's criterion and Popper's are of quite different kinds, in conflict only insofar as Popper thinks the history of science supports him and insofar as Kuhn thinks his criterion is a defining characteristic of science. What I am suggesting is that we regard Kuhn's criterion as descriptive and Popper's as legislative, thus reconciling the dispute.[19] The general point is that there is room for both descriptive and legislative criteria. Both kinds are necessary given the dual endeavor of explaining and describing scientific progress and formulating (or reconstructing) the aims and goals of science. Legislative criteria are especially important given the inapplicability of descriptive criteria in dealing with newly proposed hypotheses and theories. Sometimes it is necessary to decide and not wait and see if a tradition or research program develops.

The distinction between legislative and descriptive criteria is subject to challenge in three respects. First, it may be objected that some legislative criteria are descriptively adequate as well. Popper in particular seems willing to extend his criterion of falsification beyond mere legislation to description of scientific progress. Second, some will object that some descriptive criteria are as legislative as any: in Feyerabend's terms, they result from an "ideology."[20] Third, if the history of science is not the touchstone for the adequacy of legislative criteria, there seems to be no way to decide among competing legislative criteria.

In response to these challenges, let me first rehearse the relevant differences between the two kinds of criteria. First, they differ in range of applicability: descriptive criteria are not designed to deal with new hypotheses and theories, whereas legislative criteria are so designed. Second, I have said that legislative criteria are much more like moral principles than are descriptive criteria: they say how science ought to be viewed. And third, legislative criteria are definitions, whereas descriptive criteria are generalizations based on the examination of actual scientific theories and practices.

The first objection, that legislative criteria might be descriptive as well, is easily rebutted once one sees that it is simply false that hypotheses and theories have been considered scientific if and only if they have satisfied some criterion such as testability-in-principle or falsifiability. The whole point of constructing a criterion of demarcation is, I take it, to counteract widespread misuse of the concept of "scientific hypothesis." If in the past and present scientists have as a matter of fact followed (albeit unknowingly) a testability or falsifiability-like criterion, it would be of little or no interest to formulate it. It is only because demarcation decisions have been historically confused and idiosyncratic that there is clear need for an adequate criterion of demarcation. It thus seems to me that a legislative criterion cannot be at the same time a descriptive criterion.

But are descriptive criteria really legislative criteria in disguise? This is the second challenge, and I think it has much more merit than the first. What it amounts to is this. Even historians of science have preconceptions about what ought to be counted as scientific—their "ideology," as Feyerabend says. This point is well worth making because the most severe critics of legislative criteria seem at times to forget their own ideology. For example, Lakatos criticizes what he calls Popper's "falsificationist morality" for not noting that

> scientists frequently and rationally claim "that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding."[21]

Feyerabend points out that Lakatos is no less guilty of moralizing than is Popper, insofar as the capacity to generate a research program is valuable only with regard to a given (if widespread) ideology.

While I agree with Feyerabend's criticism of Lakatos and by extension other descriptively-minded philosophers, I do not think it at all vitiates the distinction between legislative and descriptive criteria of demarcation. After all, this distinction concerns the formulation and range of application of demarcation criteria and not the biases or ideologies of their proposers. The three differences noted remain differences, even granting Feyerabend's point. The only qualification necessary is that legislative criteria tend to hide their allegiance.

The third and final objection is that, as legislative criteria are statements of what science ought to be, there seems to be no way of deciding between competing criteria. Are proposers of legislative criteria left in the position pictured by Adolf Grunbaum—"The philosopher who presumes to sit on the legislative pedestal may be left to contemplate his own normative navel"[22]? This issue goes well beyond the scope of this paper, but it would require for a solution the development of an ethics of criteria, that is, a systematic statement and ranking of various proposed legislative criteria and a comparison of their relative merits and faults.

1. Cf. Robert Weyant's comment: "The general point which I am attempting to make here is that the science/pseudoscience distinction is misleading when we take it to be a dichotomy without alternative possibilities. The distinction is blurred if 'incorrect' science and proto-science are included within the pseudoscience category." ("Metaphors and Animal Magnetism," in Marsha P. Hanan, et al., eds., *Science, Pseudo-science, and Society* [Waterloo, Ontario: Wilfrid Laurier University Press, 1980], p. 82.)

2. Carl G. Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965), p. 102.

3. John Archibald Wheeler, "Point of View: Drive the Pseudos Out..." *Skeptical Inquirer* 3 (1979): 12-13.

4. Paul Kurtz, "Is Parapsychology a Science?" *Skeptical Inquirer* 3 (1978): 14-32.

5. Ibid.

6. John M. Ziman, *Public Knowledge: An Essay Concerning the Social Dimension of Science* (London: Cambridge University Press, 1968), pp. 92 *ff.*

7. Psychological criteria are one variety of what have been called "extrinsic criteria." Other varieties are sociological, political, or even economic criteria. Cf. Stephen Toulmin: "We shall find the disciplinary or intellectual history of the enterprise interacting with its professional or sociological history, and we can separate the 'internal' life-story of ideas from the 'external' life-stories of the men whose ideas they are, only at the price of oversimplification" (*Human Understanding: The Collective Use and Evolution of Concepts* [Princeton, N.J.: Princeton University Press, 1972], p. 143). My point in this section is that, unless this separation is initially made, we would fail to see how these factors *do* interact.

8. Laurence J. Lafleur, "Cranks and Scientists," *Scientific Monthly* 73 (1951): 285.

9. Ibid.

10. Ibid., p. 286.

11. Martin Gardner, *Fads and Fallacies in the Name of Science* (New York: Dover, 1952), pp. 8-14.

12. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), p. 132.

13. Larry Laudan, *Progress and Its Problems: Toward a Theory of Scientific Growth* (Berkeley and Los Angeles: University of California Press, 1977), p. 5.

14. Thomas S. Kuhn, "Reflections on My Critics," in Lakatos and Musgrave, *Criticism*, p. 246.

15. Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 30.

16. I am not claiming that the three characteristics of being applicable to individual hypotheses, being atemporal, and being stipulative are necessary features of all legislative criteria. They are features of Hempel's and Popper's, however.

17. Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?" in Lakatos and Musgrave, *Criticism*, p. 6.

18. Karl R. Popper, "Normal Science and Its Dangers," in Lakatos and Musgrave, *Criticism*, pp. 52-53.

19. Cf. Toulmin: "Of course, the logician's formal analysis might be defended, as merely presenting an abstract ideal or aspiration—the utopian vision of an ultimate goal—without offering practical indices for judging the rights and wrongs of actual scientific concepts. . . . For such an analysis to carry conviction even as a utopian vision, however, the resulting image of a 'perfect theory' must—like that of a perfect society—have some sort of bearing on the task of constructing better actual theories and societies." (*Human Understanding*, p. 230; see also chap. 8.)

20. Paul Feyerabend, *Against Method: Outline of an Anarchist Theory of Knowledge* (London: Verso, 1975), p. 203.

21. Lakatos, "Falsification," p. 176.

22. Adolf Grünbaum, "Is Freudian Psychoanalytic Theory Pseudo-Scientific by Karl Popper's Criterion of Demarcation?" *American Philosophical Quarterly* 16 (1979): 136.