ABNER SHIMONY

Vindication: A Reply to Paul Teller

1. One of the few things about which I am optimistic is that we are close to a clear understanding of scientific inference. I think that, as a result of the work of many contributors, all the elements for a complete theory of scientific inference are at hand — although it is risky to say so, especially in view of the dramatic history of deductive logic after it was commonly supposed to have been a complete science. However, even if the requisite elements have all been discovered, there is still hard work to do in properly combining them, for what is needed is not eclecticism but a piece of intellectual architecture.

Professor Paul Teller has criticized[1] the architectural design that I proposed in my essay "Scientific Inference."[2] I tried there to justify scientific inference in two stages. The first stage was a priori, utilizing a kind of vindicatory argument. The second stage was a posteriori and relied upon certain factual propositions about the world, which themselves have been incorporated into our scientific world view as a result of scientific inference. Teller claims that nothing was accomplished in the first stage, and he suggests that the only justification which is possible or needed for scientific inference will be a posteriori. Teller's criticisms of my a priori arguments are forceful and point to weaknesses in my design. It probably was a mistake to make a sharp separation between the a priori and the a posteriori stages in the justification. I do not concede, however, that we must dispense with vindicatory arguments. Indeed, I feel sure that when he works out the details of his a posteriori justification of scientific inference, he will inevitably find that he also must resort to them. The following examination of his criticisms leads

[1] Paul Teller, "Shimony's A Priori Arguments for Tempered Personalism," this volume.

[2] Abner Shimony, "Scientific Inference," in R. G. Colodny, ed., *Pittsburgh Studies in the Philosophy of Science*, vol. 4 (Pittsburgh: University of Pittsburgh Press, 1970).

to the conclusion that a complete and adequate theory of scientific inference must present a subtler meshing of a priori and a posteriori considerations than one finds in my essay.

2. Some remarks on the development of the scientific method will be useful before replying to Teller's specific criticisms. As Peirce pointed out, the essence of the scientific method is the deliberate attempt to arrive at beliefs which are shaped by objective reality.[3] The scientific method is the systematic outgrowth of our natural intellectual propensities toward seeking the truth, which are operative even though we are also endowed with contrary tendencies toward fantasy and delusion, and even though cultural factors almost always curtail objective inquiry. The achievement of systematic procedures for seeking the truth required not only the conquest of these psychological and cultural impediments, but also the discovery of effective procedures. Such procedures as searching for correlations and invariants, proposing and checking hypotheses, introducing theoretical concepts, taking samples, utilizing control groups, taking into account systematic and random errors, and employing probabilistic reasoning are all embryonically present in prescientific native intelligence. Yet their explicit formulation, and their separation from other procedures which are also embryonically present but turned out to be misleading, must be reckoned as discoveries. Consequently, there has been a long and intricate history of scientific method, intertwined with the history of science itself, for the successful procedures were formulated in the course of struggling with substantive scientific problems or in reflection upon accomplishments. The historical development of the scientific method is as good an example of the dialectical uncovering and refinement of knowledge preexistent in the soul as any which Plato himself offers in his dialogues, though the dialectic process required not only discussion and analysis but also experimentation.

My essay, which concerned only the inferential part of the scientific method, proposed a central principle called "the tempering condition": that a seriously proposed hypothesis should be assigned a sufficiently high prior probability to allow it a chance to be accepted at the conclusion of an investigation. Although this principle was first articulated only about thirty years ago, by Harold Jeffreys,[4] the openmindedness which it

[3] Charles S. Peirce, *Collected Papers*, Charles Hartshorne and Paul Weiss, eds., vol. 5 (Cambridge, Mass.: Harvard University Press, 1934), para. 384.

[4] Harold Jeffreys, *Theory of Probability*, 1st ed. (Oxford: Clarendon Press, 1939), pp. 107–8.

prescribes surely evokes in many people the sense of recognition that occurs when a deep-rooted intellectual propensity is stated explicitly. For this reason, the vindicatory argument proposed in my essay, which turns crucially upon the tempering condition, has applicability beyond my particular (tempered personalist) formulation of scientific inference.

3. Teller accepts the tempering condition but offers two objections to my treatment of it. First, he points out that the phrase "seriously proposed" is not fully explicated in my essay, and therefore one cannot tell how restrictive the condition is. Second, he says that no matter how the phrase is explicated, so long as it is restrictive in some way, it is a factual matter whether the class of seriously proposed hypotheses is more likely to include close approximations to the truth than does its complement; and factual matters can never be settled a priori.

On page 110 of "Scientific Inference" I anticipated the first objection by stating a dilemma: either one gives the phrase "seriously proposed" a precise explication, which would undoubtedly be arbitrary, or one gives no precise explication and thereby leaves too much to subjective judgment. The methodological guidelines which I then suggested in order to escape from this dilemma appear to me to be correct, as far as they go, but they should be strengthened by a posteriori considerations. Something has been learned about controlled activities which foster the native human propensity for seeking the truth. For example, there are rules governing experimental design, which ensure that competing hypotheses are exposed in an evenhanded manner to the facts of nature. Although no strict rules have been discovered governing retrodiction (the proposal of hypotheses), at least some heuristic principles have been learned from millennia of experience: e.g., become immersed in the subject matter, learn the art of using the work of great predecessors as paradigms, employ analogies, try to achieve perspective and to see through irrelevant complications. Social institutions have evolved for the primary purpose of cultivating the techniques which systematize the truth-seeking propensity; and even after proper allowance is made for other motivations, which tend to corrupt universities and professional societies, the existence and scientific success of these institutions are facts to marvel at. Reasonable criteria for classifying a proposal as seriously proposed should take into account the accumulated knowledge concerning the process of inquiry, and therefore these criteria are subject to change with the development (and, one hopes, the improvement) of the procedures of scientific method. There is, I think, a vague but powerful criterion of this sort operative in the subjective judgments of most research workers — that a serious proposal is one made in circumstances which have in the past been favorable to good retrodictions. This criterion could be narrowly construed, to admit only hypotheses proposed by persons with credentials attesting to approbation by those institutions which have been successful in advancing knowledge. It could also be widely construed, so as to admit any hypothesis made in circumstances satisfying the standards of which those institutions are the custodians. For the methodological reasons given in "Scientific Inference" I favor the broader construction, but also I do not believe that imprecision of definition on points of this sort is crucial. (See pp. 110–111 of "Scientific Inference" and also the epigraph.)

In order to answer Teller's second objection, the vindicatory argument which I gave should be modified to take into account a posteriori considerations. I argued that my formulation of scientific inference "is in no way 'a logic of discovery' but rather supports whatever powers human beings may have for making intelligent guesses" (*ibid.*, p. 132). This argument can now be sharpened by recognizing that the accumulated experience with inquiry provides rough criteria for delimiting the class of serious proposals. If we do not give preferential treatment to seriously proposed hypotheses over frivolously proposed and unsuggested ones, where the class of seriously proposed hypotheses is defined by reference to the general character of past scientific achievements, then effectively we are skeptical of human retrodictive powers and of the possibility of systematic increase of knowledge. If our goal is systematic knowledge about the world, then nothing will be gained, and possibly the supreme object will be lost, by such skepticism. Here, then, is a sketch of a vindicatory argument which makes use of a posteriori considerations, instead of trying (as in "Scientific Inference") to avoid an appeal to them.

Teller wishes to dispense with vindicatory arguments such as the one just sketched and to defend the tempering condition on a posteriori grounds alone. The grounds for the conclusion that the class of seriously proposed hypotheses is more likely to include close approximations to the truth than its complement, he claims, "are to be found, no doubt, in the long history of man's search for knowledge" ("Shimony's A Priori

Arguments for Tempered Personalism," p. 181). I am not sure what detailed strategy Teller has in mind for *an* a posteriori justification. He may, like Black,[5] take the inductive arguments of the past to constitute a sample from the entire population of inductive arguments and estimate the percentage of successes in this sample; then, using statistical inductive inference, he could extrapolate from sample to population. Or he may continue the line suggested in section V of "Scientific Inference" of investigating the contingencies of the actual world which favor our inferences. Whatever his strategy, however, I cannot conceive of his arriving at the conclusion he desires about the class of seriously proposed hypotheses unless he makes enormous extrapolations from all his evidence. This is particularly clear if one considers that the truths sought in the natural sciences are mostly universal generalizations, each of which transcends any finite body of data; hence, statements about the distribution of approximations to these truths among various classes of hypotheses are very remote from experimental data. Some kind of vindicatory argument, to the effect that nothing that we desire will be lost and much may possibly be gained by permitting tentative extrapolations, seems to be required in order to complete Teller's reasoning. Otherwise, by maintaining Hume's standards for justification, he will not be able to avoid Hume's skepticism.

4. Teller also objects to the reasons given for the claim that in my formulation of scientific inference "receptivity toward seriously proposed hypotheses is adequately balanced by a capacity to evaluate them critically." He seems to agree that the claim is correct, but on a posteriori grounds; whereas my defense fails because I am unable to give a priori reasons for expecting the tempered personalist evaluations of likelihoods to be on the whole reliable. His criticisms seem to me to be penetrating, even though I cannot accept his conclusion.

The rationale for the reliability, on the whole, of these evaluations is more complex than I indicated in "Scientific Inference" and also more complex than he seems to recognize. First of all, it is essential to recognize the character of the tempered personalist likelihood $P_{X,b}(e/h \,\&\, a)$, where X is the person making the evaluation, b is his background information, a is his total body of auxiliary information and assumptions,

[5] Max Black, "The Raison d'Être of Inductive Argument," *The British Journal for Philosophy of Science*, 17 (1966), 286.

e is a body of explicit experimental data, and *h* is the hypothesis in question. Although the tempering condition constrains subjective judgment somewhat in evaluating likelihoods (see pp. 134–35 of "Scientific Inference"), it nevertheless allows a great difference between $P_{X,b}(e/h\ \&\ a)$ and $P_{X',b'}(e/h\ \&\ a)$ (where a is the same in both, but different persons and different background information are involved). In case the difference is great, at least one of these two likelihood evaluations will be unreliable as a guide to actual occurrences. The difference in subjective judgment may be compatible with intellectual integrity on the part of both X and X′. For example, suppose that e is a proposition about a statistic in a sample. Then X may suspect, because of his background *b*, and X′ may not suspect because of *b*′, that under hypothesis *h* and body of assumptions *a* there is a bias operative in the selection of the sample. Now if X and X′ are seriously interested in the truth and wish their opinions to be shaped by objective reality, then the clash of their subjective judgments can often be resolved by changing the experimental situation. For instance, an auxiliary investigation may be performed to check the possible source of bias that X suspects, and if a′ is the total body of auxiliary assumptions and information at the conclusion of this checking operation, then $P_{X,b}(e/h\ \&\ a')$ and $P_{X',b'}(e/h\ \&\ a')$ may be very close to each other. In fact, once two investigators tentatively agree that sampling is random, their likelihood evaluations usually agree quite closely. (If either X or X′ is dominated by motives other than desire for the truth in the matter at hand, the situation is yet more complex, and case studies of flat-worlders etc. show that questions of scientific method and of psychopathology become intermingled. A complete epistemological theory must come to grips with these complexities, or else it will fall into smugness about the wisdom of the scientific establishment. But in this brief reply it does not make sense to discuss them.) Now, of course, bias may remain in the sampling procedure even at this stage, despite the agreement between X and X′ that it does not occur. At this point two different considerations are relevant. The first is a posteriori and was not mentioned in "Scientific Inference": that once serious investigators have attempted to detect sources of bias and are satisfied that none occur in the case at hand, then there is a propensity to reliability in their likelihood evaluations. This propensity is indicated by the large proportion of successful judgments in past cases of this kind. Whether there is more to propensities than frequencies is yet another complex question. I think the answer is positive, but I do not think the answer is crucial in the epistemological analysis of scientific inference. The second consideration is vindicatory and was discussed on pages 135–36 of my essay: that if we allow suspicion of possible but unspecified biases to undermine our belief in the validity of every process of checking a hypothesis, then in effect we have let skepticism block the road to inquiry.

The foregoing is only a sketch of an answer to Teller concerning the reliability of tempered personalist likelihood evaluations, but it suffices, I believe, to show that a posteriori and methodological considerations must be conjoined in an intricate manner.

5. Teller's final criticism is directed against my statement that tempered personalism is sensitive to the truth because it can incorporate any methodological device which analysis or experience indicates to be valuable. He says that I have made an unjustified a priori claim and that one can only judge a posteriori whether methodological changes adopted in accordance with tempered personalist procedures are conducive to achieving true beliefs.

However, my statement about sensitivity to the truth was definitely not intended to be a claim to any kind of synthetic a priori knowledge. Rather, it was a claim that the tempered personalist formulation of scientific inference has neither more nor less structure than can be specified on methodological grounds alone. This formulation is sufficiently open to allow the incorporation of techniques of scientific inference which experience has shown to be successful, where success is judged by accuracy of predictions and by the coherence of the resulting world view. In particular, this openness permits the criteria of "serious proposal" to change, since new evidence may accumulate regarding the circumstances for good retrodictions. On the other hand, the tempered personalist formulation is sufficiently structured to provide the minimal machinery (probability theory and guidelines for assigning prior probabilities) for evaluating the achievements of novel techniques.

We do not know with certainty, however, that successive refinements of procedures of scientific inference in the light of a posteriori judgments about success will lead to a highly reliable instrument and thereby to a true picture of the world. Even if the refinements continue until there is exact meshing of methodology and the scientific picture of the world, it will still be necessary to fall back upon a vindication.

> Conceivably . . . such a meshing could occur in spite of deep-lying errors in the scientific picture, which failed to be exposed in spite of strenuous experimental probing. There would, so to speak, be a probabilistic version of the Cartesian deceiving demon. Despite this conceivable eventuality, a full commitment to a methodology and a scientific picture which mesh in this way would be rational, for no stronger justification is compatible with the status of human beings in the universe ("Scientific Inference," p. 161).

When Teller attempts to give posterior justifications of principles of scientific inference by referring to the history of science, I think that he will arrive at the same juncture and will therefore be forced to rely upon a similar vindicatory argument.