

University of Nebraska - Lincoln

DigitalCommons@University of Nebraska - Lincoln

Transactions of the Nebraska Academy of Sciences
and Affiliated Societies

Nebraska Academy of Sciences

1982

Philosophy Of Science On The Moral Neutrality Of Scientific Acceptance

Robert Audi

University of Nebraska-Lincoln

Follow this and additional works at: <http://digitalcommons.unl.edu/tnas>

Audi, Robert, "Philosophy Of Science On The Moral Neutrality Of Scientific Acceptance" (1982). *Transactions of the Nebraska Academy of Sciences and Affiliated Societies*. 498.

<http://digitalcommons.unl.edu/tnas/498>

This Article is brought to you for free and open access by the Nebraska Academy of Sciences at DigitalCommons@University of Nebraska - Lincoln. It has been accepted for inclusion in Transactions of the Nebraska Academy of Sciences and Affiliated Societies by an authorized administrator of DigitalCommons@University of Nebraska - Lincoln.

PHILOSOPHY OF SCIENCE

ON THE MORAL NEUTRALITY OF SCIENTIFIC ACCEPTANCE

Robert Audi

Department of Philosophy
University of Nebraska-Lincoln
Lincoln, Nebraska 68588

This paper explores the question of whether scientific acceptance of hypotheses requires making moral or other non-epistemic judgments. Much of the paper discusses the controversy surrounding an influential argument proposed by Richard Rudner to show that scientists *qua* scientists must make value judgments. Isaac Levi's well-known critique of Rudner's argument is examined, and the argument is assessed both in the light of Levi's distinction between accepting a hypothesis and acting on it, and in terms of a partial analysis of what constitutes scientific acceptance. On the basis of this analysis, the question whether scientists may properly accept hypotheses, rather than simply assess their degree of confirmation, is also briefly explored. The paper concludes that none of the arguments considered shows either that scientists should never accept hypotheses or that, when they do, moral considerations must form part of the basis of their decision.

† † †

Scientific method is widely regarded as a way of approaching important questions without prejudice. It is commonly believed that since its proper use is neutral with respect to moral issues it constitutes a court of appeal where people with opposing moral views can settle certain of their differences. If, for instance, competing theories of social justice are supported by conflicting factual claims about the effects of certain methods of distributing benefits and burdens, these claims might be assessed by scientific procedures that do not favor any moral position. The results of scientific inquiry, then, could provide an objective basis for deciding fairly between the two moral positions. To be sure, it is generally admitted that in the choice of research problems or even in the formation of hypotheses scientists may be influenced by their moral views. But this may be plausibly said to affect only the context of discovery, not that of validation: moral commitments may affect—quite properly—what is selected for scientific study, and they may sometimes (and here improperly) affect what hypotheses are created; but when it comes to what hypotheses are scientifically accepted there are rigorous, non-

moral standards of validation which protect us from subjectivity or prejudice.

This conception of the moral neutrality of scientific acceptance has been challenged even by those who accept, as most philosophers of science do, a far-reaching distinction between the context of validation and that of discovery. The issue is of major importance for understanding science and, less obviously, of almost equal significance for understanding ethics. For even those who recognize important similarities between ethics and science as disciplines which develop theories to explain data have tended to take scientific method—as applied to validation—to be morally neutral; and certainly the contrast between normative questions, such as what sorts of actions are right, and factual questions, has usually assumed that scientific hypotheses are paradigms of factual propositions assessable without using any moral notions or presupposing answers to any moral questions. If this assumption is mistaken, then ethics as a normative discipline cannot be understood in contrast to science, if indeed the normative-factual distinction can still be plausibly maintained. Moreover, if, even in the context of validation, scientists must make moral judgments or use moral concepts, the standard view that scientific method provides an *objective* way to study nature is weakened. For while there may well be an objective method for assessing moral judgments, this is highly controversial. If the proper use of scientific method requires making moral judgments, the case for the objectivity of the method must be recast, and clearly appeals to the method as a neutral way of adjudicating between certain competing moral views will be undermined.

The case against the moral neutrality of scientific acceptance will be explored in Section I. What constitutes acceptance, whether it is essential to scientific inquiry, and

how non-epistemic factors may affect it will be considered. The point of departure is the widely known exchange between Rudner and Levi, to which we now turn.

I

Rudner (1953) provided what is to date perhaps the most plausible case for the view that the scientist *qua* scientist makes value judgments. The force of "*qua*" is largely to imply that moral or other non-epistemic normative judgments are characteristically required for the scientific acceptance of hypotheses. His central argument runs as follows:

1. The scientist *qua* scientist accepts or rejects hypotheses.
2. This requires deciding whether the evidence is sufficiently strong to warrant accepting the hypothesis.
3. The scientist's decision whether the evidence is strong enough to warrant accepting the hypothesis is "a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis." Hence,
4. The scientist *qua* scientist makes value judgments.

Rudner illustrated with reference to the hypothesis that a toxic ingredient in a drug is not present in lethal quantity: "we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards," and "*how sure we need to be before we accept [such] a hypothesis will depend on how serious a mistake would be.*" Rudner (1953) was quite aware of the objection that a scientist's business is only to determine the degree of confirmation of a hypothesis. His reply was that this only moves his point to different territory. "For the determination that the degree of confirmation is, say, *p* . . . is clearly nothing more than *the acceptance by the scientist of the hypothesis that the degree of confidence is p.*"

These arguments have been widely discussed and often criticized. Levi (1960) evaluated them in detail and has discussed the central issues in a number of other places. It will be useful to begin by considering his initial response to Rudner. He first attacked Premise 3. His central criticism was that choosing to accept a hypothesis does not entail choosing to *act* on it in relation to any specific objective. For "a person can meaningfully and consistently be said to accept a hypothesis without having a practical objective." Thus, one can accept a hypothesis in an open-ended situation and hence need not thereby choose to act on it relative to any particular objective.

Levi (1960) also considered another line of reply to Rudner, which, at the time, he drew from Carnap (1950), Hempel (1949), and Jeffrey (1956). On this view, acceptance of hypotheses is not required of scientists; rather, they should be content to assign degrees of confirmation to hypotheses relative to the available evidence, and "anyone who is confronted with a practical decision problem can go to the scientist to ascertain the degrees of confirmation of the relevant hypotheses" (Levi, 1960). Levi rejected this as "like crashing into Scylla to avoid Charybdis," and he attempted to reconcile his view that scientists do accept and reject hypotheses, with the value-neutrality of science. He based his reconciliation on two contentions. The first was that:

The necessity of assigning minimum probabilities for accepting or rejecting hypotheses does not imply that the values, preferences, temperament, etc. of the investigator, or of the group whose interests he serves, determine the assignment of these minima (Levi, 1960).

Second, the value neutrality thesis does not preclude the scientist *qua* scientist's making *any* value judgments. What it requires is that "given his commitment to the canons of inference he need make no further value judgments."

Regarding this last point, Levi did not commit himself on the crucial question "whether the canons of scientific inference dictate assignments of minimum probabilities in such a way as to permit no differences in the assignments made by different investigators to the same set of alternative hypotheses." He has treated this and similar questions at length in more recent writings (e.g., Levi, 1967). A detailed discussion of his views on the issue will not be necessary since our concerns are essentially neutral with respect to specific criteria of confirmation or acceptance and should apply to any plausible set of such criteria. For our purposes, criteria of confirmation and acceptance need to be considered only in relation to the question whether, in evaluating scientific hypotheses, scientists as such must make moral or other non-epistemic normative judgments, where epistemic judgments are, paradigmatically, judgments of the degree of warrant of a statement relative to a particular body of evidence for it. In discussing this, two distinct though related issues will be considered: whether moral or other non-epistemic normative judgments must be made in the scientific acceptance of a hypothesis, and whether they must be made in the scientific assessment of the degree of confirmation of a hypothesis. Before proceeding, however, we need to consider what constitutes acceptance. As often as this notion has been used in recent literature, it remains very much in need of further clarification (see, e.g., Burks, 1977; Kaplan, 1981; Swinburne, 1980; and Teller, 1980).

II

At times "acceptance" is so used that it might be supposed that accepting a proposition is equivalent to believing it. But as the term figures in discussions of accepting and rejecting hypotheses, belief is surely only a necessary condition for acceptance. Consider cases in which a scientist decides whether a hypothesis is acceptable and as a result of his reflections accepts it. Here acceptance is surely an event (though not necessarily an action). Accepting in this sense entails assenting to, or in some sense adopting, the proposition, forming the belief that it is true, and, for a time, at least, believing it. There is also a dispositional use of "accept," on which to say that one accepts a hypothesis is, roughly, to say that one believes it and to suggest that one accepts it in the above sense. This is the use in which we speak of the body of hypotheses a scientist accepts at a given time. It might be argued that there is a quite different, dispositional use; that, *e.g.*, a person who walks into an ordinary, well-lighted lecture room accepts the proposition that there are seats in it, even though he has not *assented* to this but has merely seen that it is so. This seems, at best, loose parlance, in which accepting is assimilated to believing. Perhaps what gives this conception of accepting plausibility is the correct point that if the person did for some reason entertain the proposition he *would* accept it.

These considerations suggest that a person is properly said to accept a hypothesis, *h*, only if its credibility has been considered, however unselfconsciously. Perhaps this does not hold in general, but it certainly seems to hold for scientific acceptance. Indeed, it seems typical of the scientific acceptance of a hypothesis that the person not only considers the credibility of it, but forms a belief about its credibility—*e.g.*, that *h* is highly confirmed by the evidence—and comes to believe it in part on the basis of that further belief about its credibility. (This further belief may well not express a numerical degree of confirmation, for most scientists do not suppose degree of confirmation is in general accurately measurable by current procedures, and hence do not in general attribute a specific numerical degree of confirmation to their hypotheses.) Often, moreover, scientific acceptance of *h* will involve not only a belief of *h* and a further one regarding its credibility, but a second-order belief about the warrant (evidence, confirmation, grounds) one has for belief that *h*.

If scientific acceptance does have this twofold character and thus involves, typically, both the belief that *h* and at least one other belief, usually one about the credibility of *h* or one about the warrant for believing *h*, then care must be taken to avoid ambiguity. If we talk, *e.g.*, of the strength of acceptance, a distinction between the strength of the belief that *h* and the strength of the quite different belief that *h* is confirmed by the evidence must be made. Still another thing is the *degree* to which the scientist believes *h* is confirmed by the

evidence. Even when he takes it to be high, say, 0.85, he may suspend judgment on *h*. If it is not believed in such a case, there may be a second-order belief that belief of *h* is barely warranted. Such second-order beliefs may also differ both in strength and in degree of subjective probability.

Evaluation can of course occur in arriving at any of the beliefs just specified, and it may be different in each case. For instance, in arriving at the belief that *h* is well confirmed by the data, the scientist may simply judge intuitively, or may explicitly use certain principles of confirmation. Doing the latter may, depending on the case, be rather straightforward. In arriving at the belief that *h*, however, there may be reliance on special epistemic principles, *e.g.* "Do not accept a hypothesis with a probability on the evidence less than 0.99." A non-scientist or a scientist employing extra-scientific criteria of acceptance might instead (or in addition) use an ethical principle of acceptance, drawn from, say, an "ethics of belief." Such a principle might prohibit belief in a hypothesis whose probability on the evidence is less than 0.99; it might prohibit this only where the matter in question is in some specified way important; or it might require a minimum difference in probability between an acceptable hypothesis and any competing one.

It is important that so far scientific acceptance has been taken to imply belief. This reflects how acceptance is normally construed. But it may be construed differently, in terms of what, at the time in question, the scientist would defend if the aim were to defend the truth. This is the conception proposed by Kaplan (1981). He offered it, in part, to dissociate acceptance from subjective probability. One reason for this is that a probabilistic rule of acceptance leads, given plausible assumptions, to the Lottery Paradox. Suppose, *e.g.*, the rule is that a hypothesis should be accepted when its probability on the evidence is at least 0.999. Confronted by a fair lottery with 1,000 tickets, one would then believe, of each ticket, that it will lose, while believing that (since the lottery is fair) one of these very tickets will win! Probabilistic rules of acceptance of the kind illustrated must be rejected. But acceptance does not require the belief of an accepted hypothesis to represent it as having a specific probability, so there is little temptation to impose a probabilistic rule of acceptance. Kaplan (1981) has also eliminated subjective probabilities as necessary components of acceptance, but at the cost of separating it, in some cases, from belief—since there are various propositions, *e.g.* obvious logical consequences of some beliefs, which one does not believe but *would* defend if their truth were questioned. Kaplan's notion of acceptance, then, will not be adopted; but such a minimal notion is available and the use of it provides another way to approach the question whether scientific acceptance is morally neutral. Indeed, Kaplan's approach to the logic of acceptance supports the view of scientific acceptance defended here.

Given these distinctions between kinds of acceptance, we are in a good position to evaluate Rudner's argument. Consider his crucial Premise 3: The scientist's decision whether the evidence is strong enough to warrant accepting the hypothesis (*h*) is a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting it. This wording—particularly the phrase “decision whether the evidence is strong enough to warrant accepting the hypothesis”—runs together two questions we have been distinguishing. (1) What is the degree of confirmation of *h* on the evidence? (2) Is *h* acceptable? (Clearly this may be simply a matter of having a certain minimum level, usually but not necessarily expressed numerically, of confirmation on the evidence.) Granted, adopting an epistemic standard for answering questions of the second kind involves evaluation. But Rudner did nothing to show that it need be moral evaluation, or even need be done with possible application to morally significant cases in mind. He talked, however, as if a *separate* evaluative question—“is this evidence strong enough to warrant accepting this hypothesis?”—must come up in typical cases of acceptance, as if for each case there are special considerations, such as the practical implications of acceptance, which must be weighed. Certainly such questions might come up and might lead to reassessment of the relevant epistemic standard; but that need not happen. Moreover, either of the questions—(1) and (2)—that does come up in deciding the scientific acceptability of *h* could lead to such reassessment. But answering them does not require it, and Rudner did not show that scientific acceptance entails the application of standards beyond those a scientist has already adopted beforehand, quite possibly on purely epistemic grounds.

This assessment of Rudner's crucial premise is a good background for evaluating some key elements in Levi's reply, outlined earlier. Surely, as Levi and others have maintained, a hypothesis can be accepted without having in mind any specific practical objective. This point may be sufficient to block Rudner's argument. The general issue might be assessed further, however. Suppose that a scientist does accept a hypothesis with the idea of using it to solve practical problems. (a) He need not and perhaps should not be prepared to stake anything on it—which is the most important case of acting on it. For he may believe that the relevant actions would not be warranted without further evidence, or that until *others* can be expected to accept it no action should be taken on it. Thus, (b) two quite different questions can and should still be distinguished. (i) Is the hypothesis scientifically acceptable? (ii) Given how probable it is on the scientific evidence for the hypothesis, is it reasonable to act on it?

Question (ii) will have different reasonable answers as applied to different actions. It may or may not have moral ramifications. In either case, however, it is not a question for the scientist as such, though competently answering it may

require scientific training or some conceptual sophistication. The plausibility of both (a) and (b) is readily seen in the light of the twofold character of scientific acceptance, stressed above. For instance, if one accepts *h*, while having the second-order belief that acceptance is just barely warranted by available evidence, in believing it, it is easy to see how one might be reluctant to act on it. Apart from acts in which little is at stake, commitment to any action on the hypothesis without more evidence may be avoided. One may embrace before one is ready to trust.

All this can be illustrated by the drug example. The scientist may accept the hypothesis of its non-toxicity with the hope of eventually using it widely as medication and with the intention to test it further, yet—as Levi would agree—justifiably decide not to support its general use. This illustrates the distinction just made: the scientist, as scientist, answers the question of scientific acceptability positively, but, as a responsible moral agent, negatively answers the question whether, *given* the scientific evidence, it is reasonable to put the drug into general use. This should not, however, be described as a refusal to “act on” the hypothesis [as Levi (1960) suggested]. The refusal is less general than that, and it is relative to a context. Thus, the scientist's acceptance of the hypothesis might carry a willingness to act on it in some situations, *e.g.* where forced, in a life-or-death situation, to decide whether to use it. Presumably here the scientist would, for himself at least, use the drug; and surely he might act on the hypothesis at least to the extent of arguing that it deserves further testing.

This sort of relation between acceptance and action may be part of what motivated Rudner's argument: surely it is plausible to hold that accepting a hypothesis implies a disposition to act on it in *some* possible circumstances. But this disposition is consistent with a second-order belief that the evidence is just barely sufficient to warrant believing *h* and does not justify staking anything on *h*. Thus, the toxicity example and similar ones do not undermine the distinction between questions (i) and (ii), or show that (i) cannot be non-morally and objectively answered. As suggested, one reason why this distinction is missed may be that acceptance of *h* is often mistakenly construed as simply believing it. But in fact acceptance carries varying degrees of conviction that *h*, and varying beliefs about the credibility of *h*. A rational person neither acquires conviction whose strength is disproportionate to his assessment of the evidence, nor, in acting, stakes more on an accepted hypothesis than is warranted by his degree of conviction in accepting it or by the specific probability, if any, he attributes to it.

These points bear on a recent attempt by Gaa (1977) to undermine the view that science is morally autonomous. We are asked to imagine a situation in which a scientist studying

a freshwater lake system arrives at what are taken to be a number of scientifically acceptable hypotheses about relations among the constituents in the system. Now suppose that a policy-maker needs to decide what, if anything, to do about "nuisance algal bloom" in bodies of water of the relevant kind.

Since a probability value . . . on the relevant hypothesis is also needed in making the decisions, a hypothesis concerning what that probability value is, must be accepted—and the costs associated with doing so are, in general, different. Presumably, the costs of error in the policymaker's case are much higher than those of the scientist. . . . Now, the autonomy thesis requires in such a situation that the scientist should ignore the needs of the policymaker—since the needs of the former come first (and, indeed, alone), there is no reason to gather more evidence (Gaa, 1977).

The upshot is that "in the kinds of situations just delineated, the scientist *qua* scientist should act unethically."

Whereas Rudner argued that scientists *qua* scientists make value judgments, Gaa argued in addition that if they do not, then living up to the moral autonomy of science thesis may require them to act unethically. Two points may be made in reply.

First, the phrase "the costs associated with doing so," *i.e.*, with accepting such a value, implies willingness to act on the hypothesis, if only by leaving the nuisance algal bloom alone. But by itself it does not imply this; it implies it only given (among other things) a further judgment of how well confirmed the hypothesis must be to justify acting on it. That judgment will be in part based on moral considerations, but it is not one the scientist as such should make. One might reply that even apart from such a judgment, the policy-maker must act on the hypothesis, since he must put a chemical in the lake or not. It is true that this must be done or not, but neither action need be *based on* the hypothesis; hence, neither need be acting on it. For example, the lake might simply be left alone on the ground that there is no good reason to do otherwise.

The second point is that nothing in the moral autonomy thesis, or in the view that scientific acceptance is morally neutral, entails that "the scientist should ignore the needs of the policymaker." Surely Gaa forgot here that the autonomy thesis concerns the scientist *qua* scientist. Such a scientist is, of course, an abstraction, a convenient but unfortunately misleading device for talking about the logic of the scientific enterprise. One cannot be a scientist *qua* scientist without also being a *person*. A person should be ethically responsible, and, of course, the person who is a scientist studying lakes

should, if possible given moral obligations and resources, provide the policy-maker more information. But this is consistent with the scientist *qua* scientist being motivated wholly by a desire to pursue a purely scientific quest for truth. One would hope, moreover, that the figures the policy-maker gets from the scientist are based on just such a quest. It would be most unfortunate if the only scientific assessments of hypotheses the former could get were filtered through the scientist's moral judgments.

III

So far, the view that scientists as such do *not* accept hypotheses has not been discussed. As Levi has contended, adopting this view is not necessary to defending the thesis that scientific acceptance does not require making moral judgments. But why should Levi have said that adopting it is like fleeing from Charybdis into the hands of Scylla? Granted, scientists often accept and sometimes even argue vehemently for hypotheses. But this could be regarded as extra-scientific; even the scientific search for truth could be accounted for by saying that scientists seek to articulate the best confirmed hypotheses and theories they can discover in the relevant scientific domain (perhaps allowing simplicity to figure as a subsidiary ideal). If, as human beings, they cannot help accepting certain apparently true hypotheses or theories, this only shows that they operate in two roles: the scientific and the pragmatic. To be sure, on this view scientists would still tentatively accept judgments of the degree of confirmation of various hypotheses, or at least comparative judgments to the effect that one hypothesis is better confirmed than its rivals. But they need not accept any actual scientific hypothesis.

This view of science is not indefensible, but it seems preferable to conceive the scientific enterprise more broadly. If it is supposed that scientists as such accept hypotheses, then the view expressed by Levi and others, that the value-neutrality thesis—and scientific objectivity in general—does allow those value judgments which are implicit in the canons of scientific inference is accepted. These are plausibly considered purely epistemic, and it is reasonable to suppose that they could be made by a purely epistemic agent, in the sense of one whose only aims are to believe (certain sorts of) truths and avoid erroneous beliefs. Similarly, scientific objectivity allows scientists to make what Nagel (1961) called "characterizing value judgments," which are, roughly, judgments of the value of a thing as a means to something else. These too may be plausibly argued to be neither in any sense moral nor necessarily open to bias by subjective influences. The question of when a hypothesis is acceptable relative to the evidence is one on which rational persons may disagree. But enough has been said to suggest why it is not a moral question. The values it involves are epistemic: acceptability here is analogous to the notion of a good (deductive or inductive) *argument*, not to

that of a right act. Rudner was thus mistaken in claiming that determining the degree of confirmation of a hypothesis is itself accepting a hypothesis whose assessment involves value considerations of a moral or at least non-epistemic kind. A good argument itself may not be morally neutral, but a *prima facie* cogent case for that position has apparently not been made.

This way of replying to Rudner's claim contrasts with the view taken by Levi, at least in his initial response to Rudner. Levi there suggested that the claim can be refuted only by establishing a positive answer to the question whether "the canons of scientific inference dictate the assignment of minimum probabilities in such a way as to permit no differences in the assignments made by different investigators to the same . . . hypotheses." This is an important question whose answer is not clear. No attempt will be made to answer it here. However, defense of the moral neutrality of scientific acceptance does not require a positive answer.

It is essential, in discussing the notion of acceptability, to bear in mind an adequate distinction between the vagueness of concepts or claims and their subjectivity. Consider the claim that something is blue. It is vague. Is it also subjective? Granted, vagueness often allows subjective judgments to generate disagreements. The vagueness of "intelligent," e.g., may lead to disagreements about someone's intelligence, based on subjective judgments of "brightness." But notice that it is possible to be clearly right or clearly wrong about colors (or intelligence), in a way in which it does not seem possible to be clearly right or clearly wrong in many matters of taste (those plausibly considered subjective). Moreover, disagreement over whether something is blue can often be resolved by attention to terminology. These and related points show that vagueness does not entail subjectivity, and surely that applies to "acceptability" as well as to many other terms.

Notice also that most people will agree that certain specimens are paradigms of blue, and these can be appealed to in resolving some disagreements about non-paradigm cases. A similar point holds for scientific hypotheses in respect to acceptability; and, just as the epistemically cautious will not apply "blue" where their less strict colleagues do—without this implying either subjectivity or any (non-epistemic) value judgment—scientists may differ in the application of "acceptable" without this implying either subjectivity or any (non-epistemic) value judgment.

Applying this to questions of the confirmation and acceptability of hypotheses, suppose for the sake of argument that a precise method cannot be found, which commands the assent of all rational persons who understand it, for assigning inductive probabilities to hypotheses given the scientific evidence for them, and suppose that even if it were found, scientists would disagree on the minimum probability required for

acceptability. If there is high intersubjective agreement among scientists on (a) whether purported evidence confirms a hypothesis and (b) which of two competing hypotheses, if either, is better confirmed by the purported scientific evidence, the notions of confirmation and acceptability might still be sufficiently objective to sustain, in the assessment of hypotheses, the moral neutrality thesis and scientific objectivity. Such intersubjective agreement may not exist among scientists; but it has not been shown to be an unrealistic ideal, and it is certainly more readily defended than the quantitative ideal to which some people apparently want to tie scientific objectivity.

IV

We may conclude, then, that neither Rudner's argument nor similar ones show that the scientific acceptance of hypotheses requires making moral or other non-epistemic judgments. It may be true that even if scientists *qua* scientists do not accept scientific hypotheses they do accept propositions about degrees of confirmation which might be called hypotheses; and there certainly appear to be alternative rational sets of criteria of acceptance and of confirmation. Perhaps selecting of one or another set of either kind can be shown to require moral considerations; but this does not appear to have been shown. Moreover, if it turns out that, on any plausible criteria, "degree of confirmation" and "acceptability" remain vague, it may not be inferred either that they are not objective or that differences in their application must be attributed to moral or other non-epistemic normative judgments. When Rudner's (1953) arguments—and the many similar ones proposed since—are rightly understood in the light of these points and the distinctions developed above, they cease to appear to undermine the thesis that scientific acceptance is, in principle, morally neutral.

There remain two important and often neglected problems which have not been broached. First, if *simplicity* is a properly scientific criterion of acceptability, is there an objective way of judging it? Second, even if the inductive probability of a hypothesis given the scientific evidence for it can be determined precisely, how can it be determined objectively when there is enough evidence—and enough warrant for believing the evidence statements—to make even a high inductive probability justify actually accepting the hypothesis?

If these problems cannot be resolved, then the defense of the moral neutrality thesis, and of scientific objectivity in general, may at least need to move closer to the view that scientists *qua* scientists do not accept or reject hypotheses. The problems can perhaps be resolved, and reasons given here suggest that the relevant issues are epistemic and *prima facie* capable of objective resolution. In any case, supposing the defense of the moral neutrality of science does require adopting the view

that scientists *qua* scientists do not accept or reject scientific hypotheses, this narrower view of scientific practice is unlikely to prevent adequate treatment of the central questions in the philosophy of science. At present, however, we may apparently suppose that scientists *qua* scientists do accept hypotheses and that scientific acceptance is, in principle, morally neutral.

REFERENCES

- Burks, A. W. 1977. *Cause, chance, reason*. Chicago, University of Chicago Press: 694p.
- Carnap, R. 1950. *Logical foundations of probability*. Chicago, University of Chicago Press: 613p.
- Gaa, J. C. 1977. Moral autonomy and the rationality of science. *Philosophy of Science*, 44:513–541.
- Hempel, C. G. 1949. Review of C. W. Churchman's *Theory of experimental inference*. *Journal of Philosophy*, 46:557–561.
- Jeffrey, R. C. 1956. Valuation and acceptance of scientific hypotheses. *Philosophy of Science*, 23:237–246.
- Kaplan, M. 1981. A Bayesian theory of rational acceptance. *Journal of Philosophy*, 78:305–330.
- Levi, I. 1960. Must the scientist make value judgments? *Journal of Philosophy*, 57:345–357.
- . 1967. *Gambling with truth*. New York, Random House: 246p.
- Nagel, E. 1961. *The structure of science*. New York, Harcourt Brace: 618p.
- Rudner, R. 1953. The scientist *qua* scientist makes value judgments. *Philosophy of Science*, 20:1–6.
- Swinburne, R. G. 1980. Comment on Teller. In J. Cohen and M. Hesse (eds.), *Applications of inductive logic*. Oxford, Oxford University Press: 63.
- Teller, P. 1980. Zealous acceptance. In J. Cohen and M. Hesse (eds.), *Applications of inductive logic*. Oxford, Oxford University Press: 28–53.