Robert Feleppa

Epistemic Utility and Theory-Choice in Science: Comments on Hempel

Robert Feleppa
Wichita State University, robert.feleppa@wichita.edu

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EPISTEMIC UTILITY AND THEORY ACCEPTANCE: COMMENTS ON HEMPEL

Professor Hempel has sketched a number of turns in the problem of induction, showing us in the process that the traditional problem of justifying inductive inference is inextricably bound up with problems concerning rational criteria of hypothesis and theory acceptance. In taking us through these various turns he surveys, and provides us valuable insights into, several of the guiding trends of a vast and often highly technical literature. In the interests of highlighting this valuable feature of his paper and to provide some focus for our subsequent discussion, I shall briefly review some of these turns, placing emphasis on what Hempel notes as the central relevance of certain questions raised by Richard Rudner concerning the character of scientific criteria of hypothesis acceptance. There are a number of, to my mind unsettled, issues concerning the task of the scientist qua scientist – particularly, the range of considerations that figure in the acceptance of hypotheses, and, indeed, how such "acceptance" is to be construed – and I shall direct the latter part of my commentary to these issues.

In his opening remarks, Hempel correctly points out that adequate solutions to the problem of justifying the method of inductive acceptance, or MIA, depend on a clear characterization of the proposed method and the rules of inference that comprise it. Unlike the relatively unproblematic cases of deductive validity, when we apply an inductive rule to a premise such as 'All examined instances of *A* have been *B*', it is not clear what we mean when we say that 'All *A* are *B*' inductively follows from it. We cannot, of course, say this conclusion follows invariably, nor can we make the initially more plausible-sounding claim that when this sort of premise is true it is rational to accept the conclusion, or to act as if it were true: for adherence to the rule, thus construed, can lead us to the *reductio* predicament of "rationally" accepting logically incompatible conclusions that are supported by the same body of evidence.

One way to avoid this problem, and thus bring us a step closer to

Synthese **46** (1981) 413–420. 0039–7857/81/0463–0413 $00.80.

solving the twofold problem of characterizing, and then justifying rules of inductive inference, is to reconstrue the original problem as one of determining criteria for making precise probability assignments to the conclusions of inductive arguments. However, rather than reconstrue the problem this way, Hempel argues that it is better to see this as half of yet another twofold division of labor: we need to concern ourselves with the problem of characterizing and justifying rules of probability assignment *and* rules of acceptance of these conclusions, based in part on these determinations of probability. The problem of probability assignment demands, and has received, rigorous mathematical treatment; and it is certainly not entirely settled. However, the core of the original problem seems to be in the rational acceptability of these conclusions, and Hempel sets the probability problem aside to focus on this latter question. (Although he subsequently considers grounds for questioning whether dealing with this problem of acceptance is indeed a proper part of our philosophical inquiry into the bases of scientific validation.) And here too the intuitively most direct acceptance rule, i.e., accepting an hypothesis on the basis of its exceeding some stipulated probability runs, via the lottery paradox, into another *reductio* consequence similar to that just noted.

So the current focal point of the problem of induction does not thus yield so easily to solution, but, rather, involves problems over and above those that concern the assignment of probabilities. In fact, this turn in the problem of induction connects it to a wider context of questions of hypothesis- and theory-acceptance, and to the turn that will be my main concern here: the degree to which value-judgments function in the validation of theory. That is, if the problem of induction is in part a problem of delineating rational criteria of hypothesis acceptance, then resolution of the controversy surrounding Richard Rudner's 'The scientist qua scientist makes value judgments' is a necessary task. For the thesis in this article is that considerations of social cost, governed in part by moral standards, figure in how high a probability will be demanded by a scientist of an hypothesis before accepting it.

Hempel sets up Richard Jeffrey's 'Valuation and acceptance of scientific hypotheses' as an antithesis to Rudner's article. Here Jeffrey argues that Rudner has conflated the scientist qua scientist's type of acceptance with the practical application of an hypothesis – i.e., its "acceptance" in the sense of making commitments to practical courses of action. Indeed, Jeffrey concedes that acceptance in this latter sense involves moral value judgments, but that this sort of acceptance is no proper part of the task of the scientist qua scientist, whose objective, though we typically *call* it hypothesis acceptance, is not really to accept hypotheses at all, but only to assign them degrees of probability. These assignments are in turn taken into consideration, along with considerations of social cost, by those who would *apply* the scientist-qua-scientist's results to particular problems: but while scientists themselves may make such applications, or may have them on their mind when engaged in theory-validation, these considerations are outside the purview of "pure science".

The synthesis that Hempel offers involves conceding to Rudner that the pure scientist makes value judgments but in maintaining, on the other hand, in essence the fact-value dichotomy that Jeffrey favors by circumscribing the range of pertinent values to purely epistemic varieties. That is, while the assignment of utilities to the success or failure of hypotheses should figure in the justification of their acceptance (and in our general consideration of the justifiability of the MIA), it is only the epistemic utilities that need be included and not the wider range of practical utilities that figure in the Rudner account of rational acceptability. If we take the increase of scientific knowledge as our rational objective, then relative to that we may ascertain the epistemic utility of truth, high information content, simplicity, explanatory power, etc. Thus considerations of epistemic utility will figure in the determination of inductive acceptance rules (although success will still not come easy), without breaching the intuitive divide between science and practical or moral concerns.

The subsequent turns that Hempel discusses extend the original problem of induction into still wider contexts of problems raised by Thomas Kuhn concerning theory-adoption and change and particularly to what Hempel calls the "historic pragmatist" strategy of justification of scientific acceptability – i.e., by reference to the intuitive judgments of scientific experts – that Kuhn offers. There are two lines of development from here: (1) Insofar as the generally accepted desiderata of scientific theories *cannot* be given precise specification or ordering, they cannot be rationally justified at all (although Kuhn contends otherwise) – that is, for a wide range of important cases we never escape the problem that has been with us from the outset, namely, of not having a clear idea of what is being justified. (2) The historic pragmatist account that Kuhn gives is significantly analogous to Nelson Goodman's account of the justification of deductive and inductive rules, and this account does avail us of a justification of inductive practice for the range of cases in which clear specification of the rules can be given. What enables genuine rational justification here, but not in Kuhn's account, is a rational vindication of inductive procedure. According to Professor Goodman's account (which, by the way, has close affinities with the epistemological views of W. V. Quine and Pierre Duhem, and which has been significantly extended to the context of ethical justification by Morton White and John Rawls), the principles governing inductive practice exist, if I may adapt Rawls's terminology, in "reflective equilibrium" without strong, presystematic intuitive judgments regarding the inductive validity of certain kinds of arguments. Although the principles generally "govern" our judgments on particular cases, the principles may indeed be modified if they conflict with our strong intuitive judgments. The justification of inductive rules comes in seeing them as arising from the *codification* of inductive practice, a codification which achieves certain systematic advantages over the uncodified practice in promoting the growth of knowledge – and here is the important vindicatory step, I believe Hempel sees as missing from Kuhn's account. In the spirit of Quine and Duhem, we are not seeking to provide some transcendant basis for inductive rules: their grounding comes in part from their conformance to the practices and presystematic judgments they serve to codify and in part from the systematic advantages codification provides.

Thus we have, in I hope fairly clear outline, the survey Hempel has given us, and I think it is evident that various questions of hypothesis acceptance turn up in important places throughout the evolution of the problem of induction. However, as I indicated at the outset, I have some difficulty in getting an adequate understanding of what hypothesis acceptance is, given the significantly different construals of this notion that various of the philosophers discussed here seem to give it. And, although the questions I shall raise here directly concern the scientist-qua-scientist dispute, I wonder if the conceptual difficulties here (if they are not entirely mine) may not also affect the subsequent turns Hempel discusses.

As you will recall, Hempel was in substantial agreement with

Jeffrey's point that Rudner's examples are insufficient to make the point that the pure scientist must make moral value judgments. This seems to follow, they argue, only if we conflate concerns with the practical application of hypotheses with the purely scientific objective of improving our body of scientific knowledge. Where Hempel disagrees with Jeffrey, and agrees, in part, with Rudner, is in claiming that the scientist does, indeed, accept hypotheses (but, qua pure scientist, only with respect to the epistemic utilities). However, it is not clear to me that Rudner's point that moral value judgments figure in scientific acceptance has been fully refuted. The general applicability of his examples is, I agree, made questionable by the objections Jeffrey raises to the effect that the scientist, on this view, would be unable to assign any *one* level of acceptance to most hypotheses. Withholding acceptance of hypotheses with their potential high-cost applications in mind might well be detrimental to other potential areas of application where the social cost factor is lower. With regard to these sorts of cases there seems to be good reason to modify Rudner's claim – perhaps in the way Hempel suggests. In these cases it does not seem to be the proper business of the scientist qua scientist to take the non-epistemic utilities into account, and, indeed, it seems that in these sorts of cases scientists don't in fact do so.

What hinders my acceptance of Hempel's position, however, are a number of the cases that initially lend credence to Rudner's view – e.g., that of the Manhattan Project which is not discussed by either Hempel or Jeffrey. Granted that there are problems in generalizing from this example (although there is also Rudner's adaptation of Quine's critique of Carnap's distinction between internal and external questions to be taken into account), but this example resists, I think, an adequate accounting on the epistemic utility view. No doubt the social cost of failure figured in the determination of the acceptance level of hypotheses assigned by the Manhattan Project physicists; yet it seems, on the present view, that we must say that insofar as they took such non-epistemic utilities into account in accepting these hypotheses, they were not accepting them qua scientists, but rather qua practical appliers of theory. Yet, these hypotheses did, during the course of the Manhattan Project research, get accepted into physical theory; and, one would think, they were accepted just at the point at which enough evidence was in to assign a probability level suitably high enough to offset the social cost of failure. However, it seems that on the present view there was *another* sort of acceptance involved – one that was not governed by this non-epistemic utility and which was, undeniably, a full acceptance of the hypotheses in question into physical theory. What I cannot get clear is what this latter kind of acceptance was like and at what point in the historical development of the Manhattan Project research it occurred. It does not seem that it occurred at the point at which the scientists *said* the hypotheses were accepted: surely they withheld such pronouncement until the social cost factor was fully taken into account. If anything, one would think this non-epistemic utility would drive the acceptance level upward, leading one to think that if epistemic utility were all that figured in hypothesis acceptance, this latter act of acceptance would have occurred earlier, when less evidence was in. But then, for some time during the course of the development of that project the hypotheses were accepted parts of scientific theory, yet were not accepted in the fuller sense: and this fuller sense would involve, I think, their announcing their full and unmitigated acceptance to their superiors. Am I to understand the purely scientific act of acceptance as a private or secret one? I would expect that if I polled the Manhattan Project scientists on this, they would recollect no such private decision. And even if they did, couldn't we still quite justifiably ask if they, or anyone, can be said to *accept* hypothesis if they refuse to *admit* this because they aren't yet sure enough of the likelihood of its truth. In any construal of a notion of acceptance wouldn't we want to preserve the truth value of the claim 'Jones does not accept *p* if he refuses to say that he accepts *p* because he isn't quite sure'?

If a scientist develops a theory about atomic fission and applies more rigorous standards of acceptance because of the immense social cost of one sort of hypothesis failure (a small, but annoyingly open possibility of an atmospheric chain reaction), I do not see how it can be claimed that this contemplation of social cost can be said to be figuring only in some context of application of theory and not, inextricably, in the process of adding hypotheses to theory. Now, again, I agree that it is difficult to generalize from this sort of case to all other types of hypothesis acceptance. The scientist needn't always make moral value judgments. But I cannot yet see how we can say that such judgments *never* figure in the validation of theory if we do indeed admit that hypothesis *acceptance* is a proper part of the pure scientist's task.

Of course Jeffrey needn't be concerned with this problem because he also insists that the scientist qua scientist doesn't really *accept* hypotheses at all. On his account I think one can clearly delineate those aspects of the Manhattan Project research that belonged to pure science. The research scientists were acting qua scientists in assigning probabilities to hypotheses on certain evidential bases and qua theory-appliers when deciding whether or not to announce acceptance of them. However, this view has other problems which Hempel has noted here and in the earlier paper of his which he cites, and I will not recount them here: except to point out that these objections turn on Jeffrey's overly stretching the notion of acceptance in the opposite direction – that is, in asking us to refrain from regarding as hypothesis-acceptance a wide variety of uses of and dispositions towards hypotheses (such as the evidence statements which are used in the very determinations of probability).

Perhaps there is an easy enough solution of these problems in an adequate explication of 'hypothesis acceptance'. But aside from this, one must keep in mind the Quinean-Duhemian background of Rudner's thesis which presents general considerations that run counter to a number of the sharp delineations that Hempel is making. (And, indeed, as Hempel notes in his earlier article, the Duhemian view of theories also conflicts with the implication of Jeffrey's view that the analysis of the experimental testing and validation of hypotheses need take into account only the isolated hypothesis and its confirming evidence and not the other theoretical hypotheses that are assumed – and, thus, accepted – in the process.) For in extending Quine's critique of Carnap's delineation of internal and external questions (the former, on Carnap's view, being the factual ones that it is the business of genuinely scientific hypotheses to answer, and the latter being the practical ones answered in a prior act of scheme adoption), Rudner challenges the proposed fact-value distinction as well. If scientific hypotheses, as Quine argues, serve to answer these practical, "external" questions, and if practical questions involve making value judgments in a wide sense, then it is not clear that we free the scientist qua scientist from the task of making moral, as well as epistemic, value judgments in validating hypotheses. As Professor Leach notes in a penetrating commentary on this dispute,

as it might be argued in Quinean fashion both that no observational statements are quite free from theory, and that no theoretical statements are quite free of experimental considerations, so it might also be argued in extension that no hypotheses are quite free of practical consequences. ('Explanation and Value Neutrality')

Wichita State University