Review of COLIN HOWSON. *Hume's Problem: Induction and the Justification of Belief.*

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COLIN HOWSON. *Hume's Problem: Induction and the Justification of Belief.* Oxford: Clarendon Press, 2000. Pp. 261. ISBN 0-19-825037-1, cloth, $35.

Hume's Problem comprises two main projects: (a) defending Hume's argument about induction against a dozen or so purported answers, and (b) laying out a logic of induction that incorporates Hume's great insight in a formal theory. In this review, I will look at several instances of Howson's defense of Hume; then I will sketch the broad outlines of Howson's own "answer," the details of which are myriad and sometimes technical.

I

"There is no good reason to suppose that inductive practice should have been successful at all." Such is the upshot of Hume's iron logic regarding the "problem of induction." How, then, do we account for the "undoubted fact that induction not only seemed to work but to work surpassingly well" (10)? Just as mathematicians have practically been able to ignore Gödel's proofs about the limits of completeness and consistency, so scientists have been oblivious of Hume's "problem" with induction. Astronomers' predictions of solar and lunar eclipses are *really and truly* more reliable than the vaticinations in the daily horoscope. The situation is neatly encapsulated in C. D. Broad's characterization of induction as "the glory of science and the scandal of philosophy."

Before we decide that Hume is indeed wrong about induction, we should make sure that we have his argument straight. Here is Hume himself:

> 'Tis evident, that *Adam* with all his science, would never have been able to *demonstrate*, that the course of nature must continue uniformly the same, and that the future must be conformable to the past. . . . Nay, I will go farther, and assert, that he could not so much as prove by any *probable* arguments, that the future must be conformable to the past. All probable arguments are built on the supposition, that there is this conformity betwixt the future and the past, and therefore can never prove it. (T Abstract, 14; SBN, 651)

Most of Hume's readers (then and now) would grant that there is no *deductive* link between statements about the past and statements about the future, but many bridle at the sundering of even a *probable* link between the two. Can Hume be right about this? Right about *what*? He never says that we are *wrong* to believe in induction, that the future will *not* resemble the past. He says only that we cannot make sound inductive inferences on the basis of *observation alone*. On pain of patently begging the very question at issue, we may not use *any* inductively established premises—premises that give pride of place to some hypotheses over others that are also compatible with the observational data.

Howson thinks that we tend to underestimate the *force* of Hume's argument and exaggerate the *devastation* it is supposed to wreak. Although Hume's argument is unassailable (an irresistible force, so to speak), it nevertheless leaves intact something of inestimable value; namely, "the logic of science itself" (119). Here, then, by way of anticipation, is Howson's (halfway) positive solution to Hume's problem: There are "demonstrably sound inductive inferences"; but they do not—and must not be claimed to—*justify* induction.

In canvassing the putative solutions to Hume's problem, Howson examines the major 'isms' of twentieth-century philosophy of science: reliabilism, realism, falsificationism, naturalism, and Bayesianism—not to mention the Anthropic Principle, significance tests, and miracles. He also considers eight "quick responses" to Hume's problem, some of which do not fit comfortably under any of the 'isms' just listed. I will briefly sample a few quickies.

1. D. C. Stove and J. L. Mackie contend that Hume equates "probable reasoning" with "reasoning concerning matters of fact and existence." Here is Mackie's verdict: "Reasonable but probabilistic inferences, then, have not been excluded by Hume's argument, for the simple reason that Hume did not consider this possibility" (cited on 13). Howson contends—correctly, I think—that the historical and textual evidence against Mackie, Stove, *et al.* is decisive. (*Treatise* 1.3.11.8; SBN 127, is especially clear and to the point.) Hume's argument is simplicity itself, and yet it exhibits in high degree what Georg Kreisel calls "informal rigour." Howson neatly captures the strength of Hume's challenge: "The argument is so effective just *because* it makes no assumption about what exactly constitutes valid reasoning—deductive, probabilistic, or whatever" (14; emphasis is Howson's).

2. P. F. Strawson dismisses the question of justifying induction as meaningless or improper and, consequently, as neither requiring nor admitting of an answer. To ask whether it is reasonable to place our trust in inductive procedures is like asking whether it is reasonable to proportion the strength of our convictions to the strength of the evidence. That is what "being reasonable" *means* in such a context. But this "answer" is beside the point:

Hume's question is about the way the world is, not about the "grammar" of *reasonable*. Nelson Goodman's appeal to *entrenched* or *projectable* predicates is a more sophisticated version of the same argument.[1]

3. A curious latter-day descendant of Kant's unsuccessful transcendental argument against Hume is the claim supposedly generated by Darwinian theory, to the effect that our cognitive habits are likely to be successful because they have grown in response to evolutionary pressures. As an *explanation* of why we think in certain ways, this one is perhaps not implausible; but as a *justification*, it is egregiously circular.

So much for the "quick responses."[2] Now I will consider a couple of the more sophisticated answers.

4. According to a *reliabilist* response (Howson cites Frank Ramsey, James van Cleve, and Alvin Goldman in his discussion), we have factual evidence for induction. Suppose (as Hume and the rest of us actually do) that we inhabit a world in which inductive arguments are, as a matter of fact, probable (i.e., a world in which true premises usually lead to true conclusions). This means that persons may acquire justified beliefs by using inductive arguments, even if they do not have a justified belief in the soundness of the inference rule being invoked. The condition of justification is *external* to the arguers. That is, the argument is (straightforwardly but innocuously) *rule*-circular, but need not be (viciously and cripplingly) *premise*-circular—or so we are told.

It is hard to see how this response constitutes an answer to Hume's problem. *Given* the rule being assumed, I may prove that believing the conclusion of an inductive argument is justified; but it is the soundness of the *rule itself* that is at issue. I must know not only that belief in the premises is justified (which is, often enough, no big problem), but also have good reason to believe that *the rule of induction itself* is sound (or "probable" in van Cleve's sense). Hume's problem is, after all, about what we can legitimately infer from the knowledge that is available to us. The *rule-circular/premise-circular* distinction is quite useless in helping us to determine that we are on the right track, even if we are in fact on the right track.

5. Here is a robust statement of the nub of the *no-miracles* argument against Hume:

> It would be a miracle, a coincidence on a near-cosmic scale, if a theory made as many correct empirical predictions as, say, the general theory of relativity or the photon theory of light without what the theory says about the fundamental structure of the universe being correct

> or 'essentially' or 'basically' correct (John Worrall, quoted by Howson, 37).

Less esoteric examples are abundant—e.g., the invariably correct predictions of solar and lunar eclipses. The close "fit" between theory and evidence seems to rule out mere fortuitous correlation. Given the total body of experimental evidence that we have, the odds of merely chance agreement are too small to be taken seriously.

Though intuitively compelling, the no-miracles argument is fallacious in its usual form. We can construct a "sounder" version by adding an appropriate inductive premise (more about this later).

The great statistician R. A. Fisher tells the story of a (wholly fictitious) lady who can infallibly tell, merely by tasting a cup of milky tea, whether the tea or the milk was poured into the cup first—and this in spite of the best efforts of the experimenters to confound her (randomizing the sequences of the cups involved, etc.). *How* does she do it? The natural hypothesis—that she has extraordinary powers of gustatory discrimination—accounts for the observational data, which would otherwise remain completely baffling. The *null hypothesis*—that she has *no* special powers of sensory discrimination—comprises all the alternative explanations of her success; e.g., that she knows which part of the table of random numbers is being used to determine the positions of the milk-first cups, that a confederate is signalling the correct answers, that she is a seer, that she has made a pact with the devil, etc., etc., etc. *ad infinitum* (alas, literally *ad infinitum*). If the null hypothesis is true, then the probability of a never-fail success rate in the set of experiments is equal to the total probability of all the alternative explanations. If the truth of the null hypothesis would render the observed facts (in this case, the lady's unerring discriminations) practically impossible, we may properly reject it in favor of the obvious one—i.e., the lady really does have astounding powers of gustatory discrimination. Or so we are inclined to think. (The null hypothesis in the *no-miracles* argument is that current science is not even approximately true.)

The catch—and it is insuperable with the assumptions available to us at this stage—is that we have no idea whatsoever how to calculate the probability of the infinitely numerous alternative explanations. *Any* hypothesis will be radically and irredeemably underdetermined. But are not most of these other explanations too altogether *outré* to be taken seriously? Only if we beg the question the exercise is supposed to answer and assume a rich set of background conditions, or (functionally) *a priori* principles. To refute Hume's argument about induction, we must abjure *any* inductively founded assumptions—e.g., using terms like *miraculous, outré, astonishing, expected, normal*, etc. in describing explananda or explanantia.

Hume knows, of course, that we assume inductive principles willy-nilly, even if we realize that they lack rational foundation. We would reject out of hand any hypothesis whose truth depended on the existence of flying pigs, but only because we believe, *on inductive grounds*, that pigs cannot fly. To compare the probabilities of competing hypotheses, we must have some *prior probabilities* in place before the fight begins (so to speak). Only as we are armed with the appropriate "priors" may we use observational data to reinforce some hypotheses and weaken others. This is the only way to get a "sounder" no-miracles argument. Here is Howson's peroration:

> We would like to think that an unbroken sequence of viewings of green emeralds reinforces the hypothesis that all emeralds are green. Unfortunately, it can equally be regarded as reinforcing the hypothesis that all emeralds are grue, which is inconsistent with the favoured hypothesis, unless we prevent it doing so by assigning appropriate prior weights. Without our assistance, the evidence cannot tell us that the course of Nature may not change, or for that matter remain the same in emeralds' continuing grueness. Nothing can. Hume was right. (240)

II

Howson's own positive solution to the problem of induction is an elaboration of ideas borrowed mainly from Hume and F. P. Ramsey (with a nod to Leibniz for his recognition of the new theory of formal probability as "a new species of logic"). From Hume he takes the thesis that inductive conclusions may be soundly inferred only from inductive premises. Inductive soundness—no less than deductive soundness (or validity)—is a property of inferences, not their conclusions. (Howson repeatedly calls attention to the parallels between inductive and deductive logic—a strategy that helps to sandbag critics who complain about certain features of inductive logic [Bayesianism in particular] while saying nothing about the same features in deductive logic.) From Frank Ramsey he takes the claim that reasoning from evidence is probabilistic reasoning and, accordingly, is "nothing but the application of logical principles of consistency" (4).

Howson contends that the Hume-Ramsey amalgam he has fashioned is the best possible solution to the problem of induction: "there is a genuine logic of induction which exhibits inductive reasoning as logically quite sound given suitable premisses, but does not justify those premisses" (4). Many philosophers—even some of Howson's fellow Bayesians—resist the suggestion that we can have a logic of induction that does not justify induction. If Howson is right, we already have such a logic—Bayesian confirmation theory (named after Hume's contemporary Thomas Bayes [1702–61]). As Howson construes that theory, Hume's skepticism is reconciled with the inexpugnable conviction that some inductive arguments are sound. As with sound deductive arguments, you have to put in synthetic premises to get a synthetic conclusion. Such premises can serve as *evidence*—i.e., strengthen or weaken hypotheses—only if we have anointed some hypotheses and rejected others beforehand. Defending and elaborating that claim is a major part of *Hume's Problem.*

Bayesianism is a many-splintered thing (John Earman says that there may be more varieties of Bayesianism than there are Bayesians); but even if it were not, a book review is not the place to say much about the nuts and bolts of the underlying theorem(s)[3]. The philosophically interesting questions are almost never about the formal theory *per se,* but about how we can, and cannot, apply the theory. Thus, although there is virtually no dispute about the formal details of Bayesian probability theory (a derivation is valid or it is not), there are long-standing disputes about its strengths and weaknesses as a model of scientific method. In the course of *Hume's Problem*, Howson defends the *subjective* or *personal* form of Bayesian probability against several objections.

The "sharpest and most persistent" (and, I think, the most philosophically complex and interesting) objection to the Bayesian approach is that it is infected with a "far-reaching subjectivism" (Wesley Salmon's phrase). Critics also allege that Bayesian reasoners would have to be logically omniscient in order to satisfy the laws of probability; and further that Bayesian reasoners cannot account for the obvious probative value of *old evidence* (i.e., evidence that was known at the time a hypothesis was proposed but was not known to support the hypothesis). Unlike philosophers who accept Bayesianism with serious reservations and misgivings (Earman describes himself as schizoid on the issue), Howson is unmoved and unimpressed by these and other attacks. He is a true believer (and a very able one): *Voi sapete quel che fa.* Whether Howson's replies to the criticisms will satisfy the critics is, of course, a different matter. Given the nature of philosophical disputes (and this one in particular), I would not expect the critics to throw in the towel. (Now *there's* a safe inductive inference.)

Hume's Problem is closely reasoned and demanding, but rich in sharp and unexpected insights. A great virtue of the book is that it ties familiar Humean doctrines to features of a formal theory, and does so naturally, without Procrustean stretching and lopping. In a word, the book is excellent. I started to say that it is excellent *in suo genere*, but that might be taken as faint praise (the opposite of what I intend) and might discourage some prospective readers. To be sure, some readers will find Howson's preoccupation with technical matters uncongenial; but the book will repay careful reading (and re-reading). If you know a bit of probability theory (or are willing to be instructed or reminded, in chapter 4), you should be able to navigate most of the book without undue trouble. Whether you will consistently find the payoff—which is sometimes a small technical point in the service of the larger project—*worth* the effort is a different question. But even if you find the formal parts less than enthralling, you will see Hume's problem of induction stated, examined, and defended with a clarity that is almost painful.

NOTES

1. I assume that just about every English-speaking philosopher of the last half-century has made the acquaintance of Goodman's portmanteau word *grue*.

2. Hans Reichenbach (not mentioned by Howson) offers a "vindication" of induction, while conceding that no direct answer to Hume's problem is possible. The gist of the argument is that induction will work *if* anything will; i.e., the eventual success of induction is a necessary condition of the success of *any* method (ornithomancy, tarots, whatever). But this begs the question. To suppose that we can use our experience in the past to sort out the different kinds of evidence and their relevance to various methods is to assume precisely what we are required to prove; namely, that the past is, in some fashion or other, a reliable guide to the future.

3. Howson's own book (written jointly with Peter Urbach) *Scientific Reasoning: The Bayesian Approach,* 2nd ed. (La Salle: Open Court, 1993), covers a wide range of Bayesiana, from the elementary to the more challenging. For a book that bristles with mean-looking formulae, John Earman's *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory* (Cambridge: MIT Press, 1992), is more helpful to anglophone readers than one would think. There are numerous brief accounts of

Bayes's Theorem(s) that focus on applying the theorem(s) to simple problems—e.g., *Choice and Chance: An Introduction to Inductive Logic,* by Brian Skyrms (Belmont: Wadsworth, 1999), and *Choices: An Introduction to Decision Theory*, by Michael Resnik (Minneapolis: University of Minnesota Press, 1987).

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