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A Modest Proposal*

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What thesis is Hume trying to establish in his essay "On Miracles" (Section 10 of the *Enquiry Concerning Human Understanding*) and does he succeed? John Earman's answer to the latter question is clearly conveyed by the title of his new book. Earman uses a Bayesian representation of the problem to make his case. For Earman, this mode of analysis is both perspicuous and non-anachronistic, in that probability reasoning was central to the 18th century debate about miracles in particular and testimony in general. Indeed, one of Hume's most interesting antagonists, Richard Price, was the person to whom Thomas Bayes entrusted his now-famous essay for posthumous publication. For Earman, Price is the proper Bayesian, while Hume's essay provides only "rhetoric and schein geld" (p. 73). Earman's tone is consistently prosecutorial and sometimes snide; he says that his animus is not so much against Hume himself as against those who smugly invoke Hume's essay as definitively settling the matter. This tone should not deter potential readers who are convinced that Hume's essay contains something of value. Earman's book is interesting and provocative in multiple ways—it places Hume's essay in its historical setting, it offers an insightful close reading of the text, and it shows how the resources of Bayesianism can be powerfully put to work. Besides Earman's own essay (94 pages long), the volume also contains Hume's essay and relevant work by others, including Locke, Spinoza, Samuel Clarke, Price, Laplace, and Babbage. The book would be an excellent choice for an advanced undergraduate or graduate seminar.

Earman's argument that Hume's essay is an abject failure begins with the thought that Hume is trying to establish something very ambitious. Earman thinks Hume is attempting to provide an in-principle argument that settles how all reports of miracles should be evaluated. With such a "silver bullet" (p. 3) in hand, there would be no need to attend to the details of specific miracle reports. Hume's remark at the start of his essay perhaps suggests this interpretation. He says "I have discovered an argument …, which, if just, will

* A review essay on John Earman's, *Hume's Abject Failure—the Argument Against Miracles*, Oxford University Press, New York, 2000, xi+217 pages, ISBN 0-19-512738-2.

… be an everlasting check to all kinds of superstitious delusion, and consequently will be useful as long as the world endures." But here we must be careful to distinguish two senses in which Hume aims at something general. His goal may be to provide a *general criterion* by which all miracle reports should be evaluated, or he may be trying to establish the *general result* that all such reports should be discounted. Earman interprets Hume as trying to accomplish both tasks and as failing miserably in both.

What does Hume mean when he defines a miracle as a violation of the laws of nature? Earman (pp. 12-13) contends that Hume uses the term "law" to mean a *presumptive* law; U is a presumptive law for a person S (at a given time) if S then has lots of instantial evidence for U and knows of no counterexamples. A miracle (for S at time t) is a violation of what is, for S at t, a presumptive law. Earman concedes that this definition blurs Hume's distinction between miracles and marvels,[1] but argues that it is a good explication of Hume's idea that (presumptive) law statements are supported by "uniform experience." The definition also has the virtue of capturing Hume's claim that the occurrence of eight consecutive days of darkness starting on January 1, 1600 would count as a miracle, as would Queen Elizabeth's rising from the dead. Earman emphasizes that this definition does not entail that miracles are impossible and that the definition is entirely naturalistic—there is no mention of God. He points out that Hume has a footnote in which he offers a second, theologically loaded, definition of "miracle"; according to Earman, this second definition plays no role in the argument against miracles that Hume presents in part 1 of the essay.

How should we assign probabilities to presumptive laws? Hume says that "a wise man … proportions his belief to the evidence." He says that if 100 of the 101 As we have observed have been Bs, then we should be more confident that the next A will be B than if we had observed 100 out of 150.[2] And if *all* the many As we have observed have been B, we should have the highest "degree of assurance" that the next A will be B. In this instance, Hume uses the phrase "full proof," by which he seems to mean that we are entitled to be

1 For example, according to Earman's definition, the existence of black swans would have been a miracle for Europeans before Europeans reached Australia. However, strengthening the definition—so as to carve out a difference between black swans on the one hand and eight days of darkness and resurrections on the other—would not affect Earman's criticism of Hume's argument.

2 Hume's examples of 100/150 and 100/101 involve two different proportions and two different sample sizes. It would be better to separate these issues; his present point would be better served by comparing 100/150 with 149/150. The point about sample-size could then be made by comparing 100/150 with 2/3. Hume does discuss sample size, but not in the essay on miracles. In the *Dialogues Concerning Natural Religion*, Hume has Philo complain that we would be entitled to believe that our world was produced by intelligent design only if we had visited many worlds and seen that all or most of them had been so created. But how many other worlds have we visited and examined? Not even one. I discuss this argument in Sober (2003b).

“morally certain” or that we should regard the prediction as beyond reasonable doubt. Earman goes further. He thinks Hume uses what Reichenbach (1938) calls “the straight rule” of induction—if we observe n As (where n is large) and m of them have been B, then we should assign to Pr(B| A) (“the probability of B, given A”) a value of m/n.[3] If m=n, the straight rules tells us to assign to Pr(B | A) a value of unity. According to Earman, Hume says we should assign to presumptive laws a probability of one. From this it follows that no further evidence (whether it is testimonial or takes some other form) can lower the probability of the presumptive law, and so none can increase the probability of M, where M entails that the presumptive law has a counterexample. The reason derives from Bayes’ theorem, which says:

$$\Pr[M \mid t(M)] = \frac{\Pr[t(M) \mid M]\Pr(M)}{\Pr[t(M)]} .$$

Here M means that a miracle contravening some presumptive law has occurred, and t(M) means that someone has reported that M is true. If the prior probability Pr(M) = 0, then the posterior probability Pr[M | evidence] = 0 as well, regardless of what the evidence might be. In probability theory, the values 0 and 1 are *sticky*—once a proposition is assigned one of those values, it is stuck with that assignment.[4]

Earman correctly observes that the straight rule “is both descriptively inadequate to actual scientific practice, and … stultifying to scientific inquiry” (p. 51). The fact that no counterexample to “All As are B” has yet been encountered hardly allows one to be absolutely certain that all As are B, or that the next A will be B. Scientists are often open to the possibility that future observations will not resemble those made in the past. Earman might have added, but did not, that Hume himself was very much alive to this possibility—it is central to his discussion of the justification of induction. Did Hume betray his own epistemology in his attack on miracles?

There is a more charitable interpretation of Hume’s recommendations about how belief should be apportioned. If m/n As have been B (where n is large), we should be more confident that the next A we observe will be B the closer m is to n.[5] Therefore, if *all* the many dead people we have examined have failed to return to life, we should be maximally skeptical that the next dead person we examine (or hear about) will come back to life. This more

3 Reichenbach’s rule addresses the task of assigning a probability to the next A’s being B, not to the generalization that all As are B. These two problems can have different solutions, as Price understood (pp. 28-30).

4 This is why Sobel (1987) chooses to represent Hume’s argument as assigning a tiny positive probability to the proposition that a miracle has occurred.

5 Of course, even this more modest principle is vulnerable to Goodman’s (1965) grue problem.

charitable interpretation is also more modest, in that no numerical values for probabilities are assigned. Modern Bayesians often resist assigning 0's and 1's to hypotheses other than truth-functional tautologies; their caution is consistent with the m/n principle just stated. My charitable interpretation also has the virtue of making sense of Hume's admission in part 2 of the essay that the occurrence of eight consecutive days of darkness beginning January 1, 1600 would be a miracle, but that he would nonetheless accept the occurrence of this extraordinary event if he received multiple independent reports from otherwise credible witnesses. Apparently Hume did not claim to provide the knock-out blow that Earman claims he sought.[6]

What, then, is Hume's insight about the epistemological status of testimony that a miracle has occurred? The first part of Hume's "general maxim" is that "no testimony is sufficient to establish a miracle, unless … its falsehood would be more miraculous than the fact, which it endeavors to establish." Earman thinks this an "unhelpful tautology" (p. 41); *of course* the question is whether $Pr[M \mid t(M) \& B] > Pr[notM \mid t(M) \& B]$, where B represents our background knowledge. Earman also is unimpressed by the point that the more certain we are beforehand that M is false, the stronger the testimonial evidence must be to overcome this skepticism. According to Earman, this commonplace in modern discussions of evidence had the same status in the 18^{th} century debate.

Hume's idea that testimony must be evaluated by considering both the reliability of the witness and the plausibility the proposition has independent of the witness's testimony can be given a Bayesian representation, though it is not one that Hume himself supplied. If we want to know whether $Pr[M \mid t(M)] > Pr[notM \mid t(M)]$—i.e., whether the ratio $Pr[M \mid t(M)] / Pr[not\ M \mid t(M)] > 1$—then a double application of Bayes's theorem reveals that there are two other ratios that matter:

$$(*) \qquad \frac{Pr[M \mid t(M)]}{Pr[notM \mid t(M)]} = \frac{Pr[t(M) \mid M]}{Pr[t(M) \mid notM]} \times \frac{Pr(M)}{Pr(notM)} .$$

If the second ratio on the right-hand side is tiny (say it equals $1/10^{100}$), then the first ratio on the right must be bigger than the tiny number's reciprocal (i.e, it must exceed 10^{100}) if the occurrence of the miracle, given the testimony, is to be more probable than not. This formulation brings out the relevance of the two quantities $Pr[t(M) \mid M]$ and $Pr[t(M) \mid notM]$, which modern probabilists call the "likelihoods" of M and notM; these should not be con-

6 Fogelin (forthcoming) also criticizes Earman's attribution of the straight rule to Hume on these two grounds—that it conflicts with Hume's general views on induction and also with Hume's discussion of the eight days of darkness example. Becker (2003) gives different reasons for rejecting this interpretation.

fused with the posterior probabilities Pr[M | t(M)] and Pr[notM | t(M)]). Hume speaks of a clash between prior experience and current testimony and describes them as opposing forces whose resultant is ascertained by subtracting one from the other. According to (*), it would be better to talk of multiplication, not subtraction, with the ratio of priors "conflicting" with the ratio of likelihoods.[7]

Proposition (*) shows that a single witness must be *enormously* reliable if his testimony is to render more probable than not a proposition that has a very low prior probability. However, multiple witnesses need not live up to this demanding standard, as Babbage demonstrated in his Bridgewater treatise, in which he took Hume to task (p. 54). If there are n witnesses each of whom is at least minimally reliable, in the sense that "$Pr[t_i(M)| M] > Pr[t_i(M)| notM]$" holds for each witness (i = 1, 2, …, n), if their reports are independent of each other (conditional on M and on notM), and if they all agree, then a tiny ratio of priors can be transformed into a ratio of posterior probabilities that is as large as you please by making n large enough. What one imperfect witness cannot do, a number of such witnesses can easily achieve.[8]

Although Earman does not discuss (*) in exactly this form, he makes a very good observation about the character of the problem it represents. He remarks that we must be careful about the order of quantifiers. Hume is right that

> (AE) For every witness, no matter how reliable, there is a proposition that is sufficiently improbable that we should decline to think the proposition true even if the witness says that it is,

if reliability is measured by the likelihood ratio in (*) and this ratio is finite. However, the following statement, which reverses the order of the quantifiers given in (AE), does not follow from (AE) and it is false:

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7 Owen (1987) interprets Hume as characterizing the reliability of witnesses in terms of the likelihoods Pr[t(M)| M) and Pr[t(M)| notM] and Price as characterizing them in terms of the probabilities Pr[M | t(M)] and Pr[notM | t(M)]. According to this interpretation, Hume and Price were talking past each other, and both were right in their own terms. Hume was right that priors are relevant in addition to information about the "reliability" of witnesses, whereas Price was right that information about priors is irrelevant if one already knows how "reliable" the witnesses are. In terms of ordinary usage, detectors (e.g., thermometers, tuberculosis test kits, and human witnesses) are "reliable" when what they say has a high probability of being correct. A better term for Pr[t(M) | M] is the detector's "sensitivity." However, usage of these terms in discussions of probability has unfortunately drifted away from these ordinary meanings.

8 This point about multiple witnesses raises a question about Hume's differential treatment of the report of eight days of darkness and the report that Queen Elizabeth rose from the dead. He grants that multiple independent witnesses could justify believing that a miracle had occurred in the former case, but not in the latter. Maybe his thought is that the latter event is much less a priori probable than the former. If so, Babbage's point about enlarging the number of witnesses applies.

(EA) There is a proposition that is sufficiently improbable that no witness, no matter how reliable, can make it reasonable for us to believe the proposition is true by reporting that it is.

The distinction between (AE) and (EA) is relevant to Hume's discussion of the Roman saying "I should not believe such a story were it told to me by Cato."

Hume saw that he had to be careful not to overstate the implications of his general position. His basic idea is that the testimony that M is true must somehow be combined with the background evidence that M is false if we are to form an overall assessment of whether M is correct, and that the stronger the background evidence is against M, the more difficult it will be for testimony to reverse our prior judgment. This general point, by itself, does not settle whether testimony that a miracle has occurred has ever sufficed or will ever suffice to reverse what we antecedently believe. That will depend on the details of each case. In early editions of the *Enquiry*, Hume concludes in part 2, after debunking a few reported miracles, that "no testimony for any kind of miracle can ever possibly amount to a probability, much less a proof," but in the 1767 edition he revises this sentence to read "no testimony for any kind of miracle has ever amounted to…" I suspect that Hume made this change because he realized that he had to keep his conclusion in line with the lesson he extracted from the example of eight days of darkness. Yet, Hume let stand his claim at the end of the same paragraph that "… no human testimony can have such force as to prove a miracle, and make it a just foundation for any such system of religion." Although miracles as violations of presumptive laws of nature *can*, in principle, be established by testimony, testimony *cannot* establish the occurrence of a miracle in the sense of a violation of a presumptive law that is due to the intervention of a deity. "Weak miracles," if they occur, are epistemically accessible, but "strong miracles" are beyond our ken.

Why does Hume say this, and is he right? He says that every religion has its supposed miracles, and if one miracle supports one religion, it disconfirms the others. They thus destroy each other's credibility. Earman makes the good point that Hume needs premisses additional to the ones he offers and that there can be no in-principle argument that the set of all alleged miracles, when properly analyzed, must result in a stand-off among all religions. Earman adds to this a Bayesian representation, and a provocative comment about the similarity between religion and science (p. 66). Suppose we know, on the basis of adequate testimony, that Jesus rose from the dead (J). This incrementally confirms Christianity (C) just in case $Pr(C \mid J) > Pr(C)$, which is true precisely when $Pr(J \mid C) > Pr(J)$. The prior probability of Jesus's rising, $Pr(J)$, is the average probability the proposition has under all possible religions (R_1, R_2 , etc.), weighted by the probability that those religions are true—$\Pr(J) = \sum_i \Pr(J \mid R_i)\Pr(R_i)$. Earman acknowledges that there is no objective way to assign prior probabilities to religions—these priors are just a reflection of one's subjective degrees of belief. However, lest this be taken to cast religion in a bad light, Earman adds that the same is true of scientific theories. One might have thought that Eddington's observation of the bending of light during an eclipse (L) was objective confirmation of general relativity (G), but for this to be true one must evaluate the prior probability of L, which is its average probability under all possible physical theories, weighted by the prior probability that those theories are true. There are no objective prior probabilities for these theories, so confirmation in science also has its subjective element. Earman's book assumes a Bayesian framework, rather than arguing for it.[9] Is this point about confirmation a reason to reconsider Bayesian epistemology?[10]

In summary, I don't think Hume should be interpreted as using the straight rule. However, I agree with Earman that Hume's general insight does not extend much beyond the thought that very strong evidence (testimonial or otherwise) is needed to render a proposition probable that we antecedently think is incredible. Whether Hume's modest point was a point worth making in the context of the 18th century debate on miracles I leave to others to decide. That it is a point worth making to the students we now teach seems pretty clear. Earman's book is critical of Hume, but much of what he says can be seen as an insightful elaboration of Hume's conception of the problem. Hume's argument, modestly construed, is not an abject failure, but neither is Earman's book—far from it. It beautifully succeeds in raising a host of important questions and constructing answers to them with great logical acuity.[11]

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9 Earman (1992) provides a measured assessment of Bayesian confirmation theory.

10 One way to avoid the subjectivity of priors is to focus exclusively on specific hypotheses and their likelihoods. Instead of asking whether an observation raises the probability of a hypothesis, we could ask whether an observation favors one specific hypothesis over another. The Bayesian criterion "$\Pr(H \mid O) > \Pr(H)$" is equivalent to the likelihood inequality "$\Pr(O \mid H) > \Pr(O \mid \text{not}H)$," so the proposal to focus on likelihoods, by itself, does not involve a departure from Bayesianism. The proposal becomes nonBayesian if we decline to evaluate the likelihoods of composite hypotheses; the negation of a specific hypothesis is typically composite. For example, even if we know how probable L is according to G, we have no objective means to evaluate the probability of L according to notG. What we can do is compare $\Pr(L|G)$ with $\Pr(L|G')$, where G' is a specific (non-composite) alternative to G. See Sober (2003a) for discussion.

11 My thanks to Lon Becker, Robert Fogelin, Alan Hájek, and David Owen for useful comments.

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