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Of Miracles and Evidential Probability: Hume's "Abject Failure" Vindicated

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> It is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits.
> —Aristotle, *Nicomachean Ethics*, 1094b24–25

1. Introduction

In *Hume's Abject Failure: The Argument Against Miracles*,[1] the eminent philosopher of science John Earman applies his considerable philosophical, technical, and rhetorical skills to "Of Miracles," the famous tenth chapter of David Hume's *Enquiry concerning Human Understanding*, and he concludes that it is deeply flawed:

> "Of Miracles" is an abject failure. It is not simply that Hume's essay does not achieve its goals, but that his goals are ambiguous and confused. Most of Hume's considerations are unoriginal, warmed over versions of arguments that are found in the writings of his predecessors and contemporaries. And the parts of "Of Miracles" that set Hume apart do not stand up to scrutiny. Worse still, the essay reveals the weakness and the poverty of Hume's own account of induction and probabilistic reasoning. And to cap it all off, the essay represents the kind of overreaching that gives philosophy a bad name. (Earman, 3)

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This is strong stuff. It is also, I think, almost entirely mistaken.[2] In this paper I argue that Earman's critique fails to hit home because he both misunderstands the aims and principles of Hume's epistemology in general, and misconstrues the argument against miracles in particular. A key point against Earman, as Dorothy Coleman[3] shows, is that Hume's approach to evidential probability has an entirely different structure and basis than does the mathematical theory of probability Earman employs in his attack. Many other commentators on "Of Miracles"—both pro and con—have made a similar interpretive error.[4]

Since Earman and others get Hume's argument against miracles wrong by making errors about key points of Hume's epistemology, I begin by locating the argument against miracles within Hume's philosophy generally. Next I dissect some of Earman's errors, and finally I extend Coleman's idea that Hume holds a non-Pascalian theory of evidential probability by filling in some historical background and by providing the beginnings of a philosophical defense of that theory. The upshot is that although Hume's argument against miracles may well be flawed, it is certainly not an abject failure.

2. "Of Miracles": Its Context and Main Contentions

"Of Miracles" was a lightening rod for debate in Hume's own time and has never ceased to be one.[5] Some version of the essay seems to have existed while Hume was composing his *Treatise of Human Nature* (first published 1739–1740).[6] Probably for fear of upsetting religious authorities, it was not included in the *Treatise*.[7] Hume's argument against miracles thus makes its debut in the *Enquiry concerning Human Understanding* (first published 1748). Its target is the argument that miracles provide grounds for accepting the truth of religious hypotheses.

The thesis Hume defends in "Of Miracles" is that reports of miracles are never adequate grounds for belief that a miracle has actually occurred; such reports are, thus, never adequate grounds on which to infer the truth of any particular religious hypothesis. This result depends in part on what Hume means by "miracle." He gives two complementary definitions. The first is: "a miracle is a violation of the laws of nature" (EHU 10.12; SBN 114). In a footnote to the same paragraph, Hume offers his second definition: "A miracle may be accurately defined, *a transgression of a law of nature by a particular volition of the Deity, or by the interposition of some invisible agent*" (EHU 10.12, n. 13; SBN 115, n.1).[8]

Clearly, a violation of a law of nature will authenticate some specific religious message if and only if it really does come about through the will of the deity purportedly sponsoring the miracle. Whether or not the right sort of divine volition is present is a question fraught with its own skeptical difficulties, but it is a question that Hume can sidestep since according to his main argument we never have sufficient evidence for warranted belief that a law violation has occurred in the first place. This is why Hume's argument has little to do with the second definition of "miracle," and why critics who accuse Hume of leaning on the wrong definition have missed the point.

Hume is careful to distinguish "marvels" (the merely wonderful or unusual) from miracles (see EHU 10.8 and 10.11; SBN 113 and 114). Earman criticizes Hume's distinction because it is not one made by Hume's contemporaries: "what matters is not how Hume classified examples [of miracles versus marvels] but how the major participants in the eighteenth-century miracles debate classified them" (Earman, 11). But it is a common and well-accepted philosophical strategy to refine a definition in order to make it more precise, and then to draw philosophical conclusions from the redefinition. For Hume's purposes, only miracles conceived as law violations deserve the name, since only miracles in that sense could possibly provide a special kind of evidence for religious hypotheses. Marvels and other events that happen conformably to the order of nature (events that *can* happen, without there being a law violation, though they might rarely or never actually occur) could be invoked in design arguments—Hume's devastating critique of which occurs elsewhere.

Hume's epistemology of empirical facts leads him to characterize laws of nature in such a way that it follows directly from the definition of a miracle as a violation of a law of nature that no report of a miracle should be believed. Hume divides all possible knowledge claims into two categories, *matters of fact* and *relations of ideas* (EHU 4.1; SBN 25). Relations of ideas are known with certainty; matters of fact, in contrast, can be known only to higher and lower degrees of probability, depending on the kind and strength of the evidence available. (See EHU sections 2–6; SBN 17–59.) A law of nature is an empirical generalization formed on the basis of an observed constant conjunction of event types plus an expectation of the mind that future cases will resemble past cases. Note that since the activity of the human mind, in particular the role of customs or habits of thought is, Hume thinks, a crucial part of the story, for Hume laws of nature are epistemological rather than ontological categories.[9]

Even the best supported empirical generalizations are not known with certainty, but only with some degree of probability—they are constructed on the basis of inductive reasoning, which Hume famously shows to be non-demonstrative (see EHU 4, especially part 2; SBN 32–9). The appropriate degree of probability for a given empirical proposition is determined by the relevant available evidence.

> A wise man, therefore, proportions his belief to the evidence. In such conclusions as are founded on an infallible experience, he expects the event with the last degree of assurance, and regards his past experience as a full *proof* of the future existence of that event. In other cases, he proceeds with more caution: He weighs the opposite experiments: He considers

> which side is supported by the greater number of experiments: To that side he inclines, with doubt and hesitation; and when at last he fixes his judgement, the evidence exceeds not what we properly call *probability*. All probability, then, supposes an opposition of experiments and observations, where the one side is found to overbalance the other, and to produce a degree of evidence, proportioned to the superiority. . . . In all cases, we must balance the opposite experiments, where they are opposite, and deduct the smaller number from the greater, in order to know the exact force of the superior evidence. (EHU 10.4; SBN 110–11)

For Hume, the highest sort of knowledge we can have of empirical propositions is "moral certainty," a degree of assurance sufficient for action and belief but short of perfect certainty (EHU 4.18; SBN 35). Degrees of assurance with regard to empirical propositions vary along a scale of probabilities from "full proof" down through various lesser degrees of probability. (EHU 10.3–4; SBN 110–11 and EHU 6, n.10; SBN 56.)

> Mr. LOCKE divides all arguments into demonstrative and probable. In this view, we must say, that it is only probable all men must die, or that the sun will rise to-morrow. But to conform our language more to common use, we ought to divide arguments into *demonstrations*, *proofs*, and *probabilities*. By *proofs* meaning such arguments from experience as leave no room for doubt or opposition. (EHU 6, n.10; see the similar comments at THN 1.3.11.2; SBN 124)

It is vital to recognize that when Hume writes of evidence that amounts to a "full proof," he is not talking about *demonstration*. Demonstration is achievable only with regard to relations of ideas. Proof for Hume is a probabilistic category that applies only to matters of fact.[10] Some commentators, including Earman, mistakenly suppose that when Hume concludes that the exceptionless experience on which laws of nature are based amounts to a "proof" against miracles, he means that the probability of the occurrence of a miracle is "flatly zero" (see below). This is, however, to unjustly accuse Hume of committing a category mistake: whether or not a miracle has occurred is a matter of fact, so its non-occurrence cannot be logically necessary.[11]

Following the passage quoted above about proportioning belief to evidence, Hume makes the point that in ordinary life nothing is more important to judging the relative weight of evidence than evaluating testimony (EHU 10.5; SBN 111). Hume's epistemology of testimony resembles John Locke's. Locke[12] argues that the credibility of testimony depends on several factors, chief among which is *the conformity of the testimony with the rest of our experience*. To someone from England, testimony about a man walking on a frozen pond in December in England is in conformity with their own knowledge and experience, and the testimony is therefore to be judged credible by them. But (in the seventeenth century at least) to someone from the tropics, the same testimony is *and should be* much less credible: "[T]o a man . . . [who] has never heard of anything like it, the most untainted credit of a witness will scarce be able to find belief" (Locke, 4.15.5.2, 656). Thus, a proposition's degree of probability is determined in part by its epistemic context. Locke illustrates this with the story of the King of Siam's response to the Dutch ambassador's testimony that in northern countries water sometimes turns hard enough that an elephant could walk on it: "Hitherto I have believed the strange things you have told me, because I look upon you as a sober fair man, but now I am sure you lie" (Locke, 4.15.5.2, 657). Hume repeats this story (with slight modifications) to make a similar point (EHU 10.10; SBN 113–14).

For Locke there is a hierarchy of degrees of probability concerning matters of fact. Those cases in which we should have the highest degree of confidence are those where the testimony of fair witnesses is consonant with our own constant experience and the experience of every person in every age (so far as we can tell). In such cases we have what Locke calls *assurance*, and we *act as if* the thing were certain, even though no matter of fact is truly certain. (Locke's "assurance" corresponds to what Hume calls "full proof.") We have a degree of probability Locke calls *confidence* in those empirical propositions that, in our own experience and the experience of others, happen *for the most part* in a given way. Our assent is *unavoidable* with regard to events that may happen one way or another when there is no reason to doubt the witnesses. And so on. Hume concurs that "in our reasonings concerning matters of fact, there are all imaginable degrees of assurance, from the highest [moral] certainty to the lowest species of moral evidence" (EHU 10.3; SBN, 110). But as Locke says,

> The difficulty is, when testimonies contradict common experience, and the reports of history and witnesses clash with the ordinary course of nature, or with one another; there it is, where diligence, attention, and exactness is required, to form a right judgment, and to proportion the assent to the different evidence and probability of the thing: which rises and falls, according as those two foundations of credibility, *viz.* common observation in like cases, and particular testimonies in that particular instance, favour or contradict it. (Locke, 4.16.9, 663)

The connection between Locke's epistemology of testimony and Hume's argument against miracles is straightforward. A report of a miracle is a statement of the occurrence of a matter of fact of a special kind: the event, the miracle, is one that does not occur in the ordinary course of nature, nor (*ipso facto*) by natural causes. Hume's basic question is: *Can there ever be sufficient warrant for believing that a miracle in this sense has actually occurred?* His answer is that, *because of the nature of the case, there cannot ever be such warrant*.[13] No testimony is sufficient to warrant belief that a miracle has actually occurred (EHU 10.12–13; SBN 114–16) because the experience on which a given law of nature is founded is extensive and exceptionless, but the testimony to a miracle is singular and can be wrong through misperception, mistransmission, or deception. Thus, whenever a "wise" person weighs the evidence regarding any report of a miracle, she will judge the weight of evidence against the miracle to be (much) greater than the evidence for it.

The depth and breadth of the exceptionless regularity of past experience ("a firm and unalterable experience") gives the strongest kind of warrant possible to the belief that the law will continue to hold in the future. It is not that the evidence demonstrates with certainty that the law is true, just that no empirical claim can possibly have stronger evidence than the evidence we have for those things we call laws of nature. The evidence offered in opposition to the exceptionless regularity is testimony. Testimony is normally reliable but is also fallible; it is known to be problematic when unusual events are being reported; and it is especially suspect in cases of reports of miracles because of the likelihood of deception or misperception. Thus the weight of evidence derived from testimony about a purported exception to a law of nature in fact will never come close to the weight of evidence from experience that the law is indeed regular.

Hume says that humankind's experience of the laws of nature is "firm, unalterable and uniform." At least to many commentators, this seems to beg the question against experiences that observers take to involve exceptions to laws of nature. Fosl argues that "in fact [Hume's] argument works just as well with weaker, more guarded claims. Indeed, an argument which relies only upon weaker (that is, more limited) premises is a stronger argument":[14] it would be enough to say that the evidence for the laws of nature is the strongest, *most* uniform evidence available, and that it establishes "paradigmatically firm" regularities of nature—our experience need not be exceptionless. If the evidence for laws is the paradigmatically best evidence we have, the evidence for a miracle can at best equal it, in which case the competing evidence (for and against the uniformity of the law in question) will balance off, which "must properly only lead us to the *suspension* of judgment on the issue."[15] In defense of Hume's stronger claim, note that for the vast majority of laws of nature we *do* have exceptionless experience—the law of the solubility of sugar, for example, has never been violated.

The very exceptionlessness of regularities of past experience such as are embodied in the law of gravity is what prompts the overwhelmingly strong impulse to believe in the future applicability of the law. A subsequent singular occurrence, positive or negative, affects one's overall confidence very little. Contrary to what some commentators have claimed, however, it is neither a consequence nor an assumption of Hume's position that one can *never* have a rational belief in a truly singular event, though he does think it extremely unlikely in fact that sufficient evidence could be established. So long as the singular event in question does not appear to *contradict* any law of nature of which we are aware, one could potentially have a rational belief that it occurred. In such cases the degree of belief that results will depend for a "wise" person on the kind and strength of the available evidence. Also, some events that at first seem contrary to nature will be found upon further investigation to be in conformity with laws that were previously unknown; once these new laws become known, it becomes rational to believe that those events occurred. All this is as it should be: The issue with regard to miracles is not what sorts of events *are* actual or possible, but rather what events it is rational to *believe* to be actual.

Hume's imagined example of eight days of continual darkness about which there is agreeing testimony from learned observers and careful historians the world over (EHU 10.36; SBN 127–8) illustrates that Hume does not rule out the possibility of rational belief in the occurrence of singular events that appear to be contrary to the order of nature. Even there, though, Hume thinks that the correct approach will be to look for previously unknown laws or ordinary but unknown conditions that produced the event without there having been a violation of the laws of nature (a volcanic dust cloud, a rogue planet that passes through our solar system and temporarily blocks the Sun's light, etc.).[16] Hume is quite sure, moreover, that even if we had evidence sufficient for rational belief in the occurrence of a truly singular event, we would nevertheless never (in fact) have sufficient evidence for its supernatural origin. Thus, attempting to ground religious hypotheses in miracles will not succeed.

Earman's example of the simultaneous cloud formations that spell out, "Believe in Emuh and you will have everlasting life," over every nation of the Earth in the language of that nation (Earman, 11), is supposed to be an example of an extraordinary event that would (Earman thinks) give grounds for rational belief in the religious hypothesis written in the clouds. Of course, this event need not be a miracle. It might happen that the deity has arranged the laws and initial conditions of wind and weather in such a way that on a certain day the cloud formations in question come about naturally. In that case, there might be grounds for a design argument. Whether the design inference is adequately warranted will depend on the evidence that the event was not simply a freak of nature (a product of coincidence rather than design), or that it was not produced as an elaborate hoax (perpetrated, perhaps, by frat boys from outer space).

Hume concludes that given what we know, including facts about the reliability of human perception and testimony, in all past cases and likely in all future cases as well, the balance of evidence has led and will lead wise people to infer that any purported violation of the laws of nature is really an instance of deception, misperception, or mistransmission. In order for testimony to justify belief that a miracle has actually occurred, the falsity of the testimony must be more unlikely than the occurrence of the event itself. As Hume puts it, “no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavours to establish” (EHU 10.13; SBN 115–16). Note that this is an outcome rather than an assumption of Hume’s analysis. Imagine a case, says Hume (EHU 10.37; SBN 128), where it is reported that Queen Elizabeth is dead, but then a month later she is found alive and resumes the throne. It would be *much* more likely that some misperception or deception had occurred than that Elizabeth had risen from the dead, even if the falsehood of the testimony to her death is unlikely because it would require a huge conspiracy. The whole history of humanity agrees that there is no cure for death. Moreover, there is good reason to think that it is not only possible but even fairly common for people to be misled or to mislead with regard to this kind of claim: “the knavery and folly of men are such common phaenomena, that I should rather believe the most extraordinary events to arise from their concurrence, than admit of so signal a violation of the laws of nature” (EHU 10.37; SBN 128).

All of this said, Hume would still grant the possibility that in some particular case an ancient witness had accurately reported the actual occurrence of a miracle and that the chain of transmission from them to us happened to be entirely uncorrupted. Hume can accommodate this because he is really arguing against believing in miracles rather than against the possibility of miracles occurring. Hume’s argument depends on the fact that in any given instance it cannot be known in advance that the testimony was good (that the event was accurately described and the transmission of the report uncorrupted). Whether or not the testimony is good is precisely what needs to be judged, and Hume says we judge it on the basis of the available evidence and our background knowledge, including our knowledge of the vagaries of human psychology and our common experience with other instances of testimony.

The idea that a miracle report is credible only when it would be more improbable for the testimony to be false than it would be for the law to be violated is something Hume calls a “general maxim.” A maxim is neither a necessary principle nor a certain truth, but rather a methodological rule to be followed in the absence of an adequate reason to think that it does not apply. Here, the maxim is a rule for reasoning about conflicting evidence claims: Barring adequate reasons to trust some particular testimony to a purported miracle, one should withhold assent from purported miracles.

This maxim has a second part: A miracle is credible just in case the falsehood of the testimony is more improbable than the event it describes, and “even in that case, there is a mutual destruction of arguments, and the superior only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior" (EHU 10.13; SBN 116). Hume's point here is that if one judges that the falsehood of the testimony is more unlikely than the falsehood of the event, even in that case one must still compare the probabilities on each side. The extent to which the falsehood of the testimony is probable is to be "deducted" from the probability that there was a law violation. The difference is the degree of assent to be attached to the miracle; since the probability of the non-violation of a law is extremely high, the probability of the truth of the testimony will have to be very high as well (in fact slightly higher), and the difference between the two, which establishes the degree of probability we should attach to the miracle's occurrence, will thus be very small. (Consider the analogous case of a criminal trial in which there is one highly reliable witness against three agreeing but individually less reliable witnesses. The jury must weigh the probabilities to come to a decision. The decision's degree of certainty will not be very high, because the competing testimonies partly cancel each other out.)

Earman calls this second part of the maxim Hume's "diminution principle," and it comes in for serious criticism on Bayesian grounds. In fact, the second part of Hume's maxim regarding miracles has long been a cause of concern, because it seems that Hume is double counting (cf. Earman, 43). The degree to which one should believe that the next roll of a die will come up six is not calculated by subtracting the probability that the event will not occur from the probability that it will occur: 5/6 – 1/6 = 4/6 against, or 2/6 in favor. Clearly the correct degree of belief is 1/6 in favor. Coleman offers a resolution of this sort of apparent difficulty. In effect, she argues that this simple mathematical example and Earman's more complex Bayesian ones are not appropriately analogous to Hume's reasoning about evidential probability. I say more below about the principles behind Coleman's defense of Hume's diminution principle. But first I turn to some of Earman's other mischaracterizations of Hume.

3. Earman's Misreadings of Hume

One of Earman's themes (see especially 9–26 and 29–32) is that Hume's theory of induction is flawed and is a source of error in Hume's argument against miracles. It is true that Hume's argument depends on his theory of the inductive generation of universal empirical beliefs. And Hume's theory of induction is admittedly imperfect—it is, if nothing else, based on an untenable theory of ideas. Nevertheless, I contend that Earman's attack on Hume fails, and that it fails because he misunderstands key elements of Hume's epistemology. There may well be problems with Hume's argument against miracles, but they are not the ones Earman identifies.

Earman claims that Hume's argument is an example of the kind of over-reaching that gives philosophy a bad name (Earman, 3). He compares Hume's maxim against belief in miracles to the Logical Positivists' "demarcation criterion" for distinguishing science from pseudo-science. The history of attempts to devise demarcation criteria is a history of failure too extensive to survey here. Suffice it to say that today philosophers of science agree that the best way to validate knowledge claims is on a case-by-case basis, rather than by trying to impose a single universal standard; Earman (3–4) concurs.

If Hume's maxim regarding miracles were a demarcation criterion, Earman would be right to be suspicious of it. But interpreting Hume's maxim as a demarcation criterion is a mistake. Earman supposes that Hume is offering an argument against the *possibility* of miracles: Earman takes it that for Hume the probability that a miracle has occurred is "flatly zero" (Earman, 13 and 23, *et passim*). In the framework of mathematical probability that Earman employs as the basis for his critique, $Pr(p) = 0$ means that p is a logically false proposition—and, thus, not-p is logically necessary. Above I mentioned that it is a mistake to read Hume as asserting that the non-occurrence of miracles is logically necessary: whether or not a miracle has occurred is a question regarding matters of fact. An even stronger reason to think that Earman is mistaken on this point is that the whole structure of Hume's argument against miracles is *a posteriori* (and hence cannot arrive at logically necessary propositions). Hume argues that *as a matter of fact*, and *given what we know* about human psychology and the facts of history—especially what we know about bogus miracle claims, the credulity of religious believers, and how humans come to know laws of nature—there has never been and very probably never will be an instance in which the probability that a miracle has occurred is greater than the probability that the reporter is mistaken, has been deceived or is a deceiver.

Hume's argument against miracles does not depend on, nor does it result in, a demarcation criterion. Rather, Hume is making a general *a posteriori* judgment about the facts of experience that are relevant to specific judgments about purported singular occurrences. These facts are so well-established that we need not perform a detailed analysis in every case. This is consistent with common sense and good epistemic practice.[17] Given this, a great many of Earman's criticisms are moot, based as they are on thinking that for Hume the probability that a miracle has actually occurred is always flatly zero.

It is true that Hume did less than he might have done to establish that *every* case of a miracle report involves mistakes or deceptions, but it is enough for Hume's purposes that reasonable people will be able to furnish particular evidence from their own experiences to make the general claim plausible for themselves. In the same way that no reasonable person today puts enough stock in claims about UFOs to demand an extremely detailed assessment of the evidence in every individual case, reports of miracles can be dismissed without detailed individual analyses. It is a moral certainty, given what we know, that these types of events do not occur.

This kind of judgment could be mistaken—moral certainty is not certainty, after all—but the burden of proof is on those who wish to establish that a miracle (or even a marvel) has occurred. Given the nature and extent of the evidence for the laws of nature, this is a heavy burden indeed.

Earman misses Hume's drift, too, when he gives a long discussion of results in Bayesian probability theory to show that testimony or other evidence could potentially supply warrant adequate for rational belief in the occurrence of a miracle. Earman relies here on assumptions Hume would grant hypothetically but would utterly reject in the analysis of actual cases. For example, Earman's proof that the testimony of multiple witnesses could raise the probability of the occurrence of a miracle above the threshold for reasonable belief (Earman, 53–9) depends on the assumption that all the witnesses are honest and have correctly perceived the event in question! It is possible, Hume would say, that this condition is sometimes satisfied. But given our background knowledge it is a practical certainty that we will never have adequate grounds to believe that this condition is satisfied in any particular case.

Earman's attack on Hume depends on taking the threshold for reasonable belief in some proposition to be that there is a greater than 50 percent chance that it is true (Earman, 41, *et passim*). Since Hume speaks constantly of probabilities but never in numerical terms, it is difficult to say what Hume would take as the numerical threshold reasonable belief. As discussed below, this is something Hume likely would not be willing to specify at all. But of one thing I am quite sure, namely, that the threshold for reasonable belief would depend on the knowledge claim in question: the more extraordinary the claim, the more extraordinary the evidence required to make believing it reasonable. And even *ordinary* beliefs Hume would not think of as adequately warranted if there were a 49 percent chance of them being false. This is in the range where a good skeptic should suspend judgment, since both the assertion and its denial are nearly equally likely.

Earman attributes the naïve "straight rule" of induction to Hume: "If n As have been examined, all of which are found to be Bs, then if n is sufficiently large, the probability that all As are Bs is 1" (Earman, 23). The textual evidence Earman cites to support the claim that Hume advocates the straight rule of induction is slight at best—he quotes four passages from the *Enquiry*,[18] none of which imply the straight rule when Hume's epistemology is properly understood.

The text Earman cites that most strongly suggests the straight rule is this: "[I]t seems evident, that, when we transfer the past to the future, in order to determine the effect, which will result from any cause, we transfer all the different events, in the same proportion as they have appeared in the past" (EHU 6.4; SBN 58; quoted in Earman, 81). But what Hume means by this is just that, through the habits of thought involved in constructing laws, we form an expectation that the proportion of future effects resulting from present causes will resemble the proportion of those effects that followed similar causes in the past. The strength of this expectation depends on our past experience: the more extensive and the more regular it is, the stronger the belief that the resemblance between past and future events will hold up. But even in the case of a very extensive and perfectly exceptionless regularity in past experience, Hume does not think the probability is 1 that the future will resemble the past. He admits, for example, that it is possible that the sun will not rise tomorrow (EHU 4.2; SBN 25–6), and that bread will not be nourishing the next time we eat it (EHU 4.16; SBN 34). For Hume no amount of past experience could make it that "the probability that all As are Bs is 1."[19]

The mistaken view that Hume adopts the straight rule leads Earman (23) to claim that

> Hume is saying that when experience is uniform—when sufficiently many As have been examined and all have been found to be Bs—then we have a "proof" that all As are Bs. . . . Proofs are defined as "such arguments from experience that leave no room for doubt or opposition." In the probabilistic language I will adopt . . . this seems to imply that when experience provides a proof, the conditional probability of the conclusion, given the evidence of experience, is 1.

This gets Hume wrong in a fundamental sense: matters of fact can never be certain; moreover, since induction is fallible, Hume would never have intended to suggest that even a very large number of uniform observations produces a probability of 1. This is true even for the exceptionless regularities upon which laws are founded. When Hume says "no room for doubt" here, he means that we have a strong psychological tendency to expect that the contrary will not occur. There are no *grounds* for doubt (an epistemic claim), but this is not the same as saying that the contrary is impossible (a logical claim).

Earman's misinterpretation of Hume on this point leads to other problems. One example is Earman's summary of the structure of Hume's argument regarding miracles:

> So here in a nutshell is Hume's first argument against miracles. A (Hume) miracle is a violation of a presumptive law of nature. By Hume's straight rule of induction, experience confers a probability of 1 on a presumptive law. Hence, the probability of a miracle is flatly zero. Very simple. And very crude. (Earman, 23)

This is indeed simple and crude. And that is a good reason, I take it, for thinking that someone of Hume's acumen and sophistication would offer no such argument. In large measure, it seems to me, Earman's mischaracterizations of Hume are based on not understanding that "proof" is for Hume a probabilistic category. There are also some deep differences between Hume and Earman on the nature of probability itself, to which I now turn.

4. Coleman: Hume as Non-Pascalian.

Dorothy Coleman[20] defends Hume's diminution principle, the idea that even in the case of reliable testimony regarding a miracle, when the testimony is compared to the experience of an otherwise exceptionless law of nature, there is "a mutual destruction of arguments, and the superior only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior" (EHU 10.13; SBN 116). While conceding that the diminution principle is inconsistent with mathematical probability theory (as Earman, 49–53, shows), Coleman argues that Hume can evade Earman's critique because Hume's theory of evidential probability has an entirely different structure and basis than that of mathematical probability. Following L. Jonathan Cohen,[21] Coleman categorizes Hume as a "Baconian" rather than a "Pascalian" about evidential probability:

> [W]hat all conceptions of probability have in common is that they provide different criteria for grading degrees of *provability*, and that degrees of provability allow for two kinds of scales. Pascalian scales take the lower extreme of probability to be disprovability or logical impossibility; the Baconian scale takes the lower extreme to be only non-provability or lack of proof.[22]

This understanding of Hume is certainly consistent with his remarks on evidential probability. Often, Hume is concerned with probability when he is evaluating the bases for various empirical beliefs; in those contexts a scale running from "not proved" through "proved" is exactly what is needed, especially for a skeptic like Hume.

The mathematical theory of probability originated with Blaise Pascal, who in 1654 exchanged a series of letters with Fermat addressing the question, "If one agrees to throw a certain number on a die in a given number of throws, does one have the advantage?" Cohen claims that "We must apparently look to David Hume . . . for the first explicit recognition by one of Bacon's admirers that there is an important kind of probability which does not fit into the framework afforded by the calculus of chance."[23] He argues that there is "a long line of philosophical or methodological reflections about such a probability, stretching at least from the seventeenth into the nineteenth century."[24] This approach is present, for example, in the philosophies of science of J. S. Mill and J. W. Herschel, and in the legal theory of James Glassford, a nineteenth century Scot.

Cohen's basic claim seems right, but there is something odd about the details. Hume was, after all, largely echoing Locke's view of probability, so Hume is not the first writer after Bacon to hold that theory of probability. And if Hume was the first to *explicitly* distinguish the two conceptions of probability, he did so in a way that has been missed by most of his readers. In fact, as I show below, the "Baconian" approach to probability predates Bacon by centuries.

Coleman's defense of Hume's diminution principle is a good one unless there are definitive arguments against the non-Pascalian conception of degrees of proof. This is an issue I cannot conclusively resolve here, but I claim that the non-Pascalian approach is at the very least plausible for the kinds of cases Hume is considering. In what follows I support this claim in two ways. In section 5, I show that Hume's approach is consistent with a long tradition of thinking about evidential probability, a tradition that is plausible in its own right. In section 6, I argue that the non-Pascalian approach is not ruled out by the "Dutch book" argument, which various authors use to show that one *must* reason about probability in Pascalian terms.

5. The History of the Non-Pascalian Approach to Evidential Probability.

Franklin discusses evidential probability from its roots in ancient Greek and especially Roman thought, through the medieval period and up to the seventeenth century. Franklin begins with a standard distinction between two categories of probability. The first is "factual," "stochastic," or "aleatory" probability; it has to do with chance set-ups (such as rolling dice) that produce characteristic random sequences. The second is "logical," "epistemic," or "evidential" probability, the theory of the relation of partial support between propositions (a.k.a. non-deductive logic).[25] Franklin shows that the two kinds of probability have been treated separately throughout almost the whole of the history of thought. Moreover,

> while the probability of outcomes with dice throws is essentially numerical, and advances in understanding are measured by the ability to calculate the right answers, it is otherwise with logical probability. Even now, the degree to which evidence supports hypotheses in law or science is not usually quantified, and it is debated whether it is quantifiable even in principle. (Franklin, xi)

Bayesianism assumes that both factual and epistemic probabilities are to be treated in the same way: numerically. Earman's attack therefore turns, in effect, on accusing Hume of not treating the evidence for miracles in the same terms as dice throws are treated. Given that the debate over the nature of probability is not settled, and that history sides with Hume in treating factual and epistemic probabilities differently, Earman's contention that "Of Miracles" is an *abject* failure is clearly far too strong.

Ancient and medieval thinking about probability is mostly in the context of the law where, naturally, there is a concern about what kinds of evidence establish what kinds of facts with what degree of assurance. The Talmud and Roman law are similar with respect to rules of proof, especially on the question of witnesses and in the fact that a high standard of proof is required. This latter point is related to the fact that it is morally worse to convict the innocent than to acquit the guilty. Because of this, in turn, "The fundamental rule in Roman law, and since, [is] that 'proof is incumbent on the party who affirms a fact, not on him who denies it'" (Franklin, 7, quoting the Roman *Corpus of Civil Law*).

Members of the first generation of medieval commentators on the *Digest* of ancient Roman law (re-discovered in Pavia about 1050 C.E.) are known as "Glossators"; modern law descends from the Glossators in an unbroken tradition (Franklin, 16). The Glossators initiated the development of thinking in terms of (non-numerical) *grades* of probability when they invented the concept of "half proof." In ancient and medieval law, two witnesses or a notarized document were normally required for "full proof," that is, proof sufficient for conviction. The Glossators' innovation was to come up with a way to deal with the fact that a single witness, or a private document, is not entirely worthless even though it is still less than full proof. The Glossators proposed that two half proofs added together would be sufficient for conviction although one alone would not be. Franklin (12) writes,

> The resulting theory is a coherent one. It is not numerical, and there is no reason to think it would have been improved if it had been numerical. On the contrary, since modern (English) law has a similar theory, and insists on keeping it non-numerical, there is every reason to believe the medievals were correct in avoiding numbers.[26]

According to Franklin, during the Renaissance there was almost no development in legal theory, but there was widespread familiarity with medieval concepts of evidence and probability, including non-numerical grades of proof. These concepts were promulgated by two standard texts on legal evidence, by Menochio and Mascardi respectively.[27] Menochio held that innovations in law, particularly attempts to invent new *kinds* of evidence, are generally a sign of fraud (Franklin, 45). It is worth noting that the treatment of presumptions in Scots law is based on Menochio (Franklin, 44): Hume was no doubt familiar with this, and it would be a short step to the position that claims about violations of laws of nature are also likely to be frauds, even independent of the fact that fraud is known to be common with regard to purported miracles. At the very least, Hume's treatment of miracles is consistent with a long and careful tradition regarding evidential probability, one still current in Hume's time. If that tradition is incorrect, it is certainly misguided to lay the blame at Hume's feet.

Whatever we should say about its direct influence on Hume specifically, it is true that legal theory had a huge impact on thinking about evidential probability generally. Franklin (350) points out that in the medieval and early modern periods, the law was

> a model for reasoning in all those areas [including medicine, philosophy, business, politics, and so on] in which there is necessarily a balancing of opinions. This helps explain why the originators of mathematical probability were all either professional lawyers (Fermat, Huygens, de Witt, Leibniz) or at least the sons of lawyers (Cardano, Pascal).

And, since Bacon was a lawyer, the fact that his account of evidential probability in science mirrors the treatment of evidence in the law is no surprise once the connections have been laid out. Note that even today legal concepts such as "proof beyond reasonable doubt" (first appearing in English law around 1770) are not treated numerically (Franklin 366).[28]

6. Hume and the Bayesians.

This much shows that Hume's non-mathematical approach to evidential probability is consistent with a long and laudable tradition. But history aside, Hume's approach to evidential probability may even be correct. Certainly, the non-mathematical approach is still commonly agreed to be appropriate in many kinds of situations. As Franklin (131) emphasizes, it is very unusual in law, science, and ordinary life to treat probability numerically: "The big bang theory of the universe is much more probable, on present evidence, than the steady-state theory. But it is a rare scientist who can be found to say exactly how much more probable—or even approximately how much." Part of Franklin's thesis is that the historical scarcity of numerical treatments of probability is not a sign of the underdevelopment of probability theory before Pascal, nor even a sign of the difficulty of applying numerical probability in practical situations. Rather, it is due to the fact that in many situations the numerical approach is simply inappropriate.

Bayesians often appeal to the Dutch book argument to establish that their approach to probability is the only correct one. The Dutch book argument shows that someone whose reasoning about probabilities is not in conformity with the axioms of mathematical probability is susceptible to willingly accepting a "Dutch book"—that is, a finite series of bets each of which the bettor takes to be fair but over the course of which the bettor is guaranteed to lose money, no matter what the outcomes. Since Bayes's Rule for updating degree of belief in light of new evidence is a simple consequence of the axioms of mathematical probability, the Dutch book argument is taken to imply that it is necessary to be a Bayesian about evidential probability. Bayesians often put the point by saying that Bayes's Rule is a *condition for rationality*.[29]

A Bayesian might hope to find here a demonstration that the non-Pascalian approach is doomed to failure. But the Dutch book argument *assumes* the Pascalian scale of probability, it does not prove it. Without the assumption that degrees of belief are numerical and run on a continuous scale from 0 to 1, the basic arithmetic of the Dutch book argument would be impossible. So, what the Dutch book argument actually shows is merely that *if* there are numerical degrees of belief ranging continuously from 0 to 1, *then* one must, on pain of incoherence, reason about degree of belief in conformity with the axioms of mathematical probability (and hence with Bayes's Rule). A hypothetical proposition of that sort, even if true, does not prove that one must be a Pascalian in the first place.

Earman (25) chastises Hume for being unaware of Bayesianism and of mathematical probability generally. This is unfair on two counts. First, Bayes's work on probability was not widely known in 1748 when Hume published the first edition of the *Enquiry*. Richard Price arranged the posthumous publication of Bayes's essay only in 1763, and it remained obscure even after its publication; Price's paper applying Bayesian methods to the evidence for miracles appeared in 1767.[30] We know Hume read and admired that paper,[31] but he neither addressed Bayesian arguments nor revised his account of miracles for the 1768 and 1777 editions of the *Enquiry*. This suggests that Hume ultimately did not view Bayes's work as relevant to the argument against miracles. Second, Hume's discussion of the probability of chances (see for example THN 1.3.11 and 1.3.12; SBN 124–42, and EHU 6; SBN 56–9) "shows without controversy that he was familiar with the basic concepts of probability based on the calculus of chances."[32] Given Hume's familiarity with Pascalian probability in general, and his acquaintance (through Price) with Bayesian ideas, his non-numerical treatment of the evidential probability of miracles must be seen as a deliberate philosophical position, not as a result of negligence or ignorance.

Is there anything wrong with Bayesian analyses of evidential probability? One problem can be teased out this way. Bayes and Price (and, by extension, other Bayesians such as Earman) are forced to make assumptions about the distribution of chances across contrary possible outcomes, assumptions which are rarely if ever justified outside of highly constrained and artificial experimental situations. Bayes, for example, develops his argument in terms of the equal chances of a perfectly round ball coming to rest at any given place on a perfectly flat table. His conclusions do indeed follow for such idealized cases. But, to speak metaphorically, we usually do not have round balls and flat tables, or at least we cannot be sure that we do. The assumption of the equipossibility of contrary outcomes is therefore usually not justified in actual cases. Barry Gower writes, "Hume had, in effect, noticed the role of an assumption of this kind in any attempt to counter inductive skepticism when he pointed out that 'if there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless and can give rise to no inference or conclusion.'"[33] It may be that, methodologically speaking, when assessing probabilities numerically there is no *better* principle to apply than the principle of indifference (which says that contrary possibilities are to be treated as equally probable in the absence of any reason to think otherwise), but that does not mean that it is a *good* principle to apply in assessing the evidential probability of empirical hypotheses in the sense that it reliably leads to correct conclusions in all types of cases.

Is it sensible to treat all probabilities numerically? Franklin (327) notes that

> Factual probability is essentially numerical, certainly. . . . But Keynes, in his classic *Treatise of Probability*, argued at length that not all logical probabilities should have numbers. Even if they do have numbers in principle, no convincing way has been discovered of actually assigning a number to, for example, the probability of the steady-state theory of the universe on present evidence—or even such a simple case as the probability that the next ball chosen from an urn will be black, given that all of the twenty balls already chosen have been black. . . . This should caution us against supposing that, because the concept of probability before Pascal was mostly nonnumerical, it was therefore primitive or in some way inadequate.

Having a non-numerical theory of evidential probability does not mean that we must give up all hope of precision and rigor in our assessments of the probability of empirical hypotheses. There are other ways to achieve this: In discussions of temperature, for example, classificatory concepts (hot, warm, very cold, etc.) and comparative concepts (hotter than, etc.) are possible without any numerical scale. Similarly the non-numerical tradition of evidential probability has given us perfectly serviceable classificatory concepts (improbable, probable, highly probable) and comparative concepts (more probable than, etc.). (See Franklin 328.) If this seems insufficient, we should ask whether quantitative theories of probability can really do better: "Has the quantification of probability helped in the evaluation of uncertain evidence in science?" Franklin (369) answers, "In the restricted cases in which statistical tests apply, it has, but for more general theory evaluation, it seems not." The desire for increased precision is laudable but, as the quotation from Aristotle at the head of this article points out, it can be taken too far.

There have been several attempts over the years to analyze Hume's argument against miracles in Bayesian terms.[34] Interestingly, about as many claim to support Hume's conclusion as claim to refute it; the fact that contradictory results have been achieved is possibly a symptom of the wrong-headedness of Bayesian reconstructions of Hume's argument against miracles.[35] The fact that there are so many incompatible Bayesian accounts of miracles also suggests that the correct Bayesian account is even now neither clear nor settled—yet another reason not to blame Hume for giving a non-Bayesian analysis. Given the diversity of Bayesian opinions, it is perhaps no surprise that some of them are consistent with what Hume says about miracles. (In the present context I hesitate to calculate the probability of such a coincidence.)

Barry Gower thinks that the

> "Bayesian" interpretation of the argument against miracles misrepresents Hume's reasoning. . . . [T]he error needs to be rectified, not just because it involves a mistaken view about the past but, more importantly, because it obscures the legacy of a different mode of thinking evident in Hume's writing about probabilistic inference which deserves to be recovered. Our thinking is impoverished if certain presumptions about probability become so entrenched that we have great difficulty in seeing them as anything other than obvious.[36]

The "different mode of thinking" Gower has in mind is a mathematical (but non-Bayesian) approach to evidential probability found in Jakob Bernoulli's (1713) *Ars Conjectandi*.[37] Gower suggests that Hume's argument against miracles is an application of Bernoulli's approach. I agree with almost every point in the quotation above, but I see two difficulties with the Bernoullian hypothesis. First, a Bayesian could respond that belief coherence demands that mathematical probabilities conform to the probability calculus: if Bernoulli-style reasoning leads to a conclusion different from that produced by Bayes's Rule, it can be rejected. That would imply that either Hume's argument must be equivalent to the Bayesian assessment of the probability arising from the testimony for miracles or it must be wrong—a conclusion we should not be too quick to embrace. The second difficulty is that the Bernoullian analysis already assumes that evidential probabilities are numerical. If we deny this, as I think Hume would, then we also avoid the first difficulty. Rather than looking to Bernoulli in order to understand Hume's theory of evidential probability, we should look to the medieval legal tradition—a tradition whose mutual influence on Pascal, Bernoulli, and other developers of mathematical probability explains the similarity between their approaches and Hume's.

In other respects Gower's position is very similar to the one advocated here. He thinks, for example, that "Hume's probabilities are not structured in accordance with any conventional theory of chances where they are represented by fractional numbers between zero and one."[38] He notes, too, that Hume's probabilities are non-additive, and remarks that "Hume's training as a lawyer may well have influenced his attitude to probabilistic reasoning, and it is recognized that legal probabilities" do not conform to the axioms of the probability calculus.[39] Gower gets things exactly right when he writes that Hume

> quite evidently evaluates probable arguments in a way that cannot be reconciled with the pre-suppositions of Bayes's theorem. For example, an argument [with no] probability is, for Hume, one where favourable cases equal the unfavourable cases; and an argument where there were only favourable, or unfavourable, cases, would not be a probable argument at all [but a proof in Hume's sense]. We think that an event with a small probability is unlikely to occur; Hume thought that such an event is more likely to occur than not.[40]

7. Conclusion

Let me conclude by commenting on one final passage from Earman (25):

> A number of Hume's contemporaries, such as Price, understood Hume's claims as being about quantifiable degrees of belief or credibility, the quantification being subject to the constraints of the probability calculus. I have no doubt that Hume would have agreed to this much, and I have little doubt that . . . he would have agreed that the probabilistic form of analysis is wholly appropriate when discussing the credibility of testimony. Naysayers will have a hard time explaining away [the letter from Hume to Price[41]], where Hume is implicitly accepting the probabilistic form into which Price cast Hume's argument.

I have argued here, following Coleman and Cohen and relying on Franklin's history of evidential probability, that the propositions Earman has no doubt Hume would accept are ones Hume would in fact deny. As a "naysayer" I have an easy explanation of the letter to Price. Hume's remarks are really rather non-committal: all he says is that Price's argument is "new and plausible and ingenious, and perhaps solid" and that he needs more time to judge it. Hume was always cordial to critics who treated him cordially (see Coleman, 196–7): politeness is thus a sufficient explanation of Hume's letter to Price. It would be perfectly consistent for Hume to have judged that numbers are inappropriate in the assessment of evidential probabilities.

My overall case here against Earman is twofold. First, that to whatever extent his critique of Hume's argument against the credibility of miracles succeeds, it does so only by shifting the ground. Earman in effect attacks a straw Hume. His claim that Hume's argument against miracles is an "abject" failure has therefore certainly not been proven; Earman's arguments in fact do not establish that it is a failure at all. Second, in order to reach a definitive answer regarding whether or not Hume is correct, it would need to be decided whose starting assumptions—Hume's non-Pascalian assumptions or Earman's Pascalian ones—properly apply to the analysis of the evidence for miracles. This issue has not been decided here. But several factors—the history of thinking about evidential probability, current normal practice in assessing the probability of empirical hypotheses, and a lack of proof of the necessity of the Pascalian approach—all coincide, and together strongly suggest that Hume's non-Pascalian foundation, and his resulting analysis of the evidence for miracles, is plausible. The burden of proof is on Pascalians to justify the basic principles which Earman assumes in his attack on Hume. Only once that burden has been satisfied could a Pascalian go on to try to ascertain whether or not Hume's actual position on miracles holds up.

NOTES

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1 John Earman, *Hume's Abject Failure: The Argument Against Miracles* (Oxford: Oxford University Press, 2000). References to this work will be cited in the text by page number.

2 In this I am not alone: see for example Michael P. Levine, "Review of John Earman, *Hume's Abject Failure: The Argument Against Miracles*," *Hume Studies* 28 (2002): 161–7, and Robert J. Fogelin, *A Defense of Hume on Miracles* (Princeton: Princeton University Press, 2003). I submitted a version of this paper to *Hume Studies* before having seen Fogelin's book. Although Fogelin and I share similar positions on Hume and agree that Earman is dead wrong, there are many differences of detail and emphasis between us.

3 Dorothy Coleman, "Baconian Probability and Hume's Theory of Testimony," *Hume Studies* 27 (2001): 195–226.

4 In fact, although L. Jonathan Cohen, *The Probable and the Provable* (Oxford: Clarendon Press, 1977) and "Some Historical Remarks on the Baconian Conception of Probability," *Journal of the History of Ideas* 41 (1990): 219–31, defends the "non-Pascalian" account of probability and argues for its influence from the sixteenth through the eighteenth centuries, generally neither historians of philosophy nor philosophers of science have given it sufficient attention. This is especially problematic since, as James Franklin shows in *The Science of Conjecture: Evidence and Probability before Pascal* (Baltimore: The Johns Hopkins University Press, 2000), that view dominated thinking about probability from Roman times to the present.

5 Don Garrett, "Hume on Testimony concerning Miracles," in *Reading Hume on Human Understanding*, ed. Peter Millican (Oxford: Oxford University Press, 2002), especially 301–4 and associated footnotes, summarizes the major analyses of Hume's essay on miracles. Earman canvasses the eighteenth-century debate (especially the parts relevant to the development of mathematical probability theory), and the second half of his book is an anthology of important sources for, and responses to, Hume's argument.

6 A note on references to Hume's works. *A Treatise of Human Nature*, ed. David Fate Norton and Mary J. Norton (Oxford: Oxford University Press; 2000, reprinted with corrections 2002), is cited as "THN book.part.section.paragraph." *An Enquiry concerning Human Understanding*, edited with introduction and notes by Tom. L. Beauchamp (Oxford: Oxford University Press, 1999), is cited as "EHU chapter.paragraph." For both, references are also given to "SBN" by page number, that is, to the edition of the respective work edited by L. A. Selby-Bigge and revised by P. H. Nidditch: *A Treatise of Human Nature*, 2nd ed. (Oxford: Clarendon Press, 1978), and *Enquiries concerning Human Understanding and concerning the Principles of Morals*, 3rd. ed. (Oxford: Clarendon Press, 1975). *The Letters of David Hume*,ed. J. Y. T. Grieg, 2 vols. (Oxford: Clarendon Press, 1983 [1932]), are cited as "*Letters* volume, page."

7 See E. C. Mossner, *The Life of David Hume*, 2nd ed. (Oxford: Clarendon Press, 1980), 112, and Hume's letter to Henry Home, *Letters* 1, 24–5.

8 Beginning with George Campbell's 1763 *Dissertation of Miracles*, various commentators have read Hume as defining miracles so narrowly as to have begged the question. (For references and discussion, see Peter S. Fosl, "Hume, Skepticism, and Early American Deism," *Hume Studies* 25 (1999): 171–92, especially 180 and note 45.) I think it as a serious mistake to read Hume's argument as circular or based purely on a definition. It is true that with definitions different than Hume's it will be possible to evade the letter (though perhaps not the spirit) of Hume's argument, but Hume has good reasons for defining miracles as he does, as I explain in the text.

9 Critics of "Of Miracles" have complained that Hume's conception of laws is vague or otherwise inadequate. But Fosl argues that Hume's conception of laws "conform[s] to the principal features of laws of nature developed by more recent philosophers of science": the things Hume discusses as laws or as grounded in laws are "true, non-analytic, universal generalizations, whose subject terms are unrestricted, that sustain counterfactual conditionals, and that may be used to formulate explanations and predictions" (Fosl, 180–1).

10 Many critics of the argument against miracles have missed this point. The Flew/Fogelin debate in these pages, for example, largely turned on this very issue.

See: Robert J. Fogelin, "What Hume Actually Said About Miracles," *Hume Studies* 16 (1990): 81–7; Antony Flew, "Fogelin on Hume on Miracles," *Hume Studies* 16 (1990): 141–5; and Kenneth G. Ferguson, "An Intervention into the Flew/Fogelin Debate," *Hume Studies* 18 (1992): 105–12. Fogelin, *A Defense* (91, note 8) admits having formerly misunderstood "proof" in Hume.

11 The word "proof" is today often used to refer to demonstration, as in a proof in mathematics. This is not, however, its only use: "proof beyond a reasonable doubt" in criminal law, for example, clearly implies the possibility of lesser and greater degrees of proof, and also implies that what has been proven is possibly (though not probably) false.

12 John Locke, *An Essay concerning Human Understanding*, edited with an Introduction by P. H. Nidditch (Oxford: Oxford University Press, 1975 [1690]). Further references in text, cited as "Locke, book.chapter.section(.paragraph), page."

13 Flew, 141, incorrectly reads "from the nature of the fact" to mean "from the very concept of a miracle." Hume clearly intends to be making the empirical claim that belief in miracles is evidentially unwarranted, not that it is conceptually impossible.

14 Fosl, 181.

15 Ibid., 182.

16 Hume seems to believe that the apparently stochastic character of some causes is an epistemological artifact rather than an ontological fact: probably, causes that appear to act irregularly do so because interfering causal factors—perfectly regular ones—exist, but are unknown to us. (EHU 8.13; SBN 86–7; EHU 6.1–4; SBN 56–9, and THN 1.3.12.5; SBN 132)

17 As Hume says to Hugh Blair, "Does a man of sense run after every silly tale of witches or hobgoblins or fairies, and canvass particularly the evidence?" (*Letters* 1, 350).

18 The passages Earman cites are: EHU 10.4 (SBN 111); EHU 10.6 (SBN 112); EHU 10.12 (SBN 114–15); and EHU 10.12 (SBN 115).

19 Fogelin, *A Defense*, 43–53 and 58, gives an extended argument showing why Earman is wrong to attribute the straight rule of induction to Hume.

20 See note 3 above.

21 See note 4 above.

22 Coleman, 198.

23 Cohen, "Some Historical Remarks," 225.

24 Ibid., 219.

25 See Franklin, x. Further references will be given in text by page number.

26 As Franklin notes, there *were* a few attempts to discuss finer grades of legal proof in a numerical fashion, but they were dead ends. For example, a passage from the *False Decretals*, a hodge-podge of real and forged documents assembled about 850 C.E., asserts that seventy-two witnesses are required to convict a bishop, forty-four to convict a priest, and so on down to the doorkeeper, who needs seven. This "is the world's first quantita-

tive theory of probability. Which shows why being quantitative about probability is not necessarily a good thing" (Franklin, 13–14). For the most part, numerical and even quasi-numerical accounts of evidential probability did not appear until after Pascal.

27 Josephi Mascardi, *On Proofs* (*Conclusiones probationum omnium, or, De probationibus*), 3 vols. (Venice, 1584–8), and Giacomo Menochio, *On Presumptions, Conjectures, Signs, and Indications* (*De praesumptionibus, conjecturis, signis et indicitis*), 2 vols. (Venice, 1587–90).

28 Reviews of Franklin suggest that there have been some attempts to apply mathematical probability to legal questions. I do not know whether such attempts have survived the test of case law; for present purposes not much hangs on this since it is still true that most uses of probability in the law are deliberately non-numerical.

29 Earman (26) gives a précis of the Dutch book argument, and later (30) also appeals to it as providing a persuasive argument that the axioms of probability are conditions of rationality.

30 Thomas Bayes, "An Essay towards Solving a Problem in the Doctrine of Chances," *Philosophical Transactions of the Royal Society* (1763) 53: 370–418; reprinted in *Biometrika* 45 (1958): 296–315. Richard Price, "On the Importance of Christianity and the Nature of Historical Evidence, and Miracles," in *Four Dissertations*, 2nd ed. (London: A. Millar and T. Cadell, 1768 [1767]); reprinted in Earman, 157–76.

31 *New Letters of David Hume*, ed. R. Klibansky and E. C. Mossner (Oxford: Clarendon Press, 1954), 233–4; quoted in Earman, 24.

32 Coleman, 201.

33 Barry Gower, *Scientific Method: An Historical and Philosophical Introduction* (London: Routledge, 1997), 103, quoting EHU 4.21 (SBN 37–8).

34 Unlike Price and Earman, David Owen, in "Hume versus Price on Miracles and Prior Probabilities: Testimony and the Bayesian Calculation," *Philosophical Quarterly* 37 (1987): 335–48, argues that a Bayesian analysis shows that Hume's position against the credibility of miracle reports is correct. Fogelin (*A Defense*, 47) thinks that Hume's position, "though mathematically naïve, is broadly Bayesian in character." (He goes so far as to say that Hume is a better proto-Bayesian than Price!) Jordan Sobel, ("On the Evidence of Testimony for Miracles: A Bayesian Interpretation of David Hume's Analysis," *Philosophical Quarterly* 37 (1987): 166–86; and "Hume's Theorem on Testimony Sufficient to Establish a Miracle," *Philosophical Quarterly* 41 (1991): 229–37) is one who misreads what Hume means by "proof," claiming that Hume thinks the probability of a miracle occurring is zero. Making the probability of miracles zero is equivalent to saying that miracles are logically impossible. This has the undesirable consequence that Bayes's rule is then unable to increase the degree of belief in the occurrence of a given miracle *no matter what the evidence*. Sobel "corrects" Hume here by treating the probability of a miracle occurring as infinitesimally close to zero. (Other considerations aside, this seems a very un-Humean move.) Peter Millican, "'Hume's Theorem' Concerning Miracles," *Philosophical Quarterly* 43 (1993): 489–95, proposes a Bayesian alternative to Sobel and criticizes various of its details and implications. Phillip Dawid and Donald Gillies, "A Bayesian Analysis of Hume's Argument Concerning Miracles," *Philosophical Quarterly* 39 (1989): 57–65, offer a Bayesian analysis of Hume's argument which they think is

simpler than the analyses of Owen and of Sobel, and which they think makes clearer the differences between Hume and Price. A detailed response to these works is beyond the scope of this paper; it will have to suffice to note that if Hume is a non-Pascalian, even pro-Hume Bayesian reconstructions of "Of Miracles" are misguided. The endnotes in Dorothy Coleman, "Hume, Miracles and Lotteries," *Hume Studies* 14 (1988): 195–226, mention several other works that address Hume's account of probability in terms of the mathematical theory of chances.

35 Similarly, Sally Ferguson, "Bayesianism, Analogy, and Hume's *Dialogues concerning Natural Religion*," *Hume Studies* 28 (2002): 113–30, argues that "the attempt to apply Bayesian reasoning to the argument as presented in the *Dialogues* is not well supported as a reconstruction of Hume's own approach" (113); "there are good reasons for not treating Hume's reasoning [in the *Dialogues*] as even proto-Bayesian" (114). Ferguson shows that "the benefits that have been claimed for [the Bayesian] approach, in terms of exposing both the subtleties of the argument and Hume's reasoning about it, can equally well be derived from a careful analysis of the argument under a model of analogical reasoning, without the need of Bayes's theorem" (114). Ferguson (114–18) gives an excellent survey of attempts to interpret Hume through Bayes.

36 Barry Gower, "David Hume and the Probability of Miracles," *Hume Studies* 16 (1990): 17–32, 17.

37 Fred Wilson, "The Logic of Probabilities in Hume's Argument against Miracles," *Hume Studies* 15 (1989): 255–75, proposes another mathematical but non-Bayesian interpretation of Hume which he thinks is consistent with what Hume says about probabilities in the *Treatise*, answers some of Price's objections, and improves on Sobel, Owen, and others. If I am right, Wilson's proposal does not accurately reflect Hume's position either.

38 Gower, 22.

39 Ibid., 21

40 Ibid., 24. I have inserted "[with no]" for Gower's "of zero": it is clear in the context that Gower does not intend to attribute a numerical conception of evidential probability to Hume.

41 *New Letters*, 233–4.