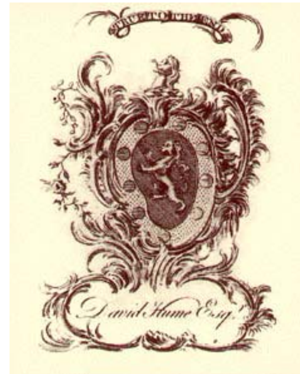

David Hume and the Probability of Miracles

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1. Introduction

Of late there have been published several discussions of David Hume's famous essay "Of Miracles" which attempt to make precise the reasoning it contains.[2] This, it turns out, requires the use of certain mathematical rules and theorems of the probability calculus which were unknown to Hume or, indeed, to anyone else when the essay was first published. It is suggested, in particular, that the claims he made can best be understood in the light of a theorem of the probability calculus which we now name after a celebrated eighteenth century probabilist, Thomas Bayes.

However, the so-called "Bayesian" interpretation of the argument against miracles misrepresents Hume's reasoning in that the manner in which he expressed his thinking does not fit such an interpretation. Moreover, this 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. By imputing them to Hume we overlook their contestability and so lose an opportunity to recapture an alternative way of understanding probabilistic reasoning.

As is well known, the argument against miracles, though not published until 1748, had been worked out some twelve or thirteen years earlier during Hume's stay at the Jesuit college of La Fléche, where he wrote most of *A Treatise of Human Nature*.[3] A chapter entitled "Reasonings concerning Miracles" had been written but, at the last moment, it was suppressed for fear that such an explicit attack on religion would give offence and thus deflect attention from the book's main philosophical aim which was to put an end to controversy.[4] On the other hand, Thomas Bayes's famous memoir on probability was not published until 1764, and it is unlikely that Hume knew of its existence until Richard Price mentioned it, though without identifying its author, in an elaborate mathematical footnote to a dissertation containing remarks on the probability of miracles, published in 1767. Had Hume taken the trouble to follow up this reference,[5] he would almost certainly have been either baffled by its mathematics or discouraged by its lack of practical advice. That, at least, seems to have been the reaction of Hume's contemporaries, for apart from Price none of them seems to have paid any attention to the memoir. Even today, when anyone with the slightest acquaintance with probability can recite a version of Bayes's theorem, it is hard to follow the reasoning of the memoir and to appreciate the significance of the ideas it contains. As an historian of statistics has recently said, "Bayes's contemporaries were stymied by the work, and modern readers unwilling to devote many hours to it are likely to gain only a superficial understanding of it."[6]

One paradoxical feature of attributing Bayesianism to Hume concerns the role of Richard Price. His dissertation, which had, it seems, been written some years before it was published — perhaps in the early 1750's — is sharply critical of Hume's argument against miracles.[7] Yet it was Price himself who was responsible for the posthumous publication of Bayes's memoir, and his introduction and commentary clearly express approval of its methods and conclusions. We are, then, faced with the suggestion that Hume, not having heard of Bayes, was applying Bayesian ideas about "inverse" inference to miraculous events, whereas Price, who was not only familiar with but also endorsed Bayes's ideas, preferred a quite different approach to reasoning about miracles and about the credibility of testimony generally. Can Hume really be described as a "better intuitive Bayesian" than Price?[8] Before embracing such a paradoxical conclusion we should consider whether it is really justified by what Hume said about the probability of miracles.

First, though, some attention needs to be paid to a way of thinking about probable arguments which does not conform to convention, and which implies a non-Bayesian interpretation of the question about miracles.

2. Jakob Bernoulli and the Combination of Arguments

In the seventeenth century, a new branch of mathematics, called in England "the doctrine of chances," had been created. In the main the doctrine was narrowly focused on games of chance. At any rate, various problems deriving from such games were used to illustrate the mathematical techniques. There were, though, some who had begun to consider other possible applications for the theory. Among them was Jakob Bernoulli.[9] In his *Ars Conjectandi*, published posthumously in 1713, he suggested that questions about how one probable argument can be "combined" with another might be resolved using mathematical methods. His basic idea was that a probable argument is analogous in certain respects to games of chance. Instead of favourable and unfavourable equipossible alternatives in games of chance, we consider equipossible "cases" of the argument, some of which "prove" its conclusion, others of which "disprove" its conclusion, and, if necessary, still others which do neither.

The point of resolving a probable argument into component parts in this way is to enable us to combine two or more such arguments in as determinate a way as possible. Consider, for example, one of Bernoulli's own illustrations, which involves combining two probable arguments.[10] Suppose we know that p people fit the description we have of the perpetrator of a crime. One of them, Gracchus, turns pale when accused. Is he guilty? There are, said Bernoulli, two probable arguments proving the guilt of Gracchus. First we have an argument based on the description, for the fact that Gracchus fits it gives some grounds for the conclusion that he is guilty. However, the description also gives grounds for concluding that each of the other $p - 1$ suspects is guilty, and if any of these other conclusions are true then, of course, Gracchus is innocent. So there are, as it were, p cases in all; each is as probable as any other; $p - 1$ prove Gracchus's innocence; just one proves his guilt; none leave the matter undecided. Secondly, we have an argument for Gracchus's guilt deriving from his pallor when accused. This, too, is a probable argument in the sense that Gracchus's guilt does not follow of logical necessity from his pallor. In one of an appropriate number, q, of equipossible cases, Gracchus's pallor proves his guilt because in this case the guilt is the cause of the pallor; but in the $q - 1$ other cases his pallor does not prove his guilt because then it has some other cause. There are, as it were, q cases in all; each is as probable as any other; $q - 1$ leave the matter of Gracchus's guilt or innocence undecided; just one case proves his guilt; none of the cases prove that he is not guilty.

We can now use the first of the two probable arguments to divide the $q - 1$ undecided cases into those that prove Gracchus's guilt and those that prove his innocence. Clearly, a proportion $1/p$ of these $q - 1$ undecided cases will prove his guilt, and $(p - 1)/p$ of them will prove his innocence. So, taking both arguments together, we have $1 + (q - 1)/p$ out of a total of q cases which prove Gracchus's guilt. We can therefore represent the combined probability for Gracchus's guilt as $[1 + (q - 1)/p]/q$, or $(p + q - 1)/pq$. If, for example, there were just four suspects who fitted the description, and we judged on the basis of experience that guilt can be correctly inferred from pallor one out of every ten times, then the combined arguments yield a probability of 13/40 in favour of Gracchus's guilt.

So far as I know, neither Bernoulli nor anyone else ever applied such a technique to the question about miracles. However, that question certainly can be understood as one about how arguments are to be combined. Moreover, it is not difficult to see that it would yield a probabilistic but non-Bayesian argument; and the fact that such a technique could be contemplated by Hume's contemporaries[11] is enough to show that alternatives are being obscured by the imposition of a twentieth century framework, with its language of conditional probabilities, upon an eighteenth century debate.

The occurrence of miracles is supported by an argument based on the testimony of reliable witnesses, and is undermined by an argument based on observation of unbroken regularities in the natural world. How can these two arguments — the testimony argument and the observation argument — be combined? Clearly, the testimony argument itself, though a probable argument, gives no grounds for drawing the conclusion that miracles never occur. It allows us to doubt, but not to deny, the non-occurrence of miracles. Bernoulli's technique, then, would suggest that we divide this argument into a number of supposedly equiprobable cases, r of which prove that miracles occur, and s of which prove nothing for or against their occurrence. The values r and s will depend on the nature of our evidence about the reliability of witnesses. So $r/(r+s)$ is that proportion of the cases in this argument which are favourable to the conclusion that miracles occur, and $s/(r+s)$ is that proportion where no conclusion can as yet be drawn. With the aid of the other probable argument — the observation argument — we now divide the s undecided cases into those that conclude against miracles, those that conclude in their favour, and those that still leave the matter undecided. The observation argument itself clearly gives no grounds for believing that miracles occur. So with this argument, too, we can doubt, but not deny, its conclusion. There are, at it were $t + u$ equipossible cases of this argument, t of which disprove miracles and u of which prove nothing. Again the values of t and u are determined by the nature of our evidence concerning observed regularities. So, with regard to the s cases which were left undecided by the first argument, we can say a proportion $t/(t+u)$ of them disprove miracles and a proportion $u/(t+u)$ leave the matter still undecided. Combining both arguments we have, then, r out of $r + s$ which prove miracles, and $ts/(t+u)$ out of $r + s$ which disprove them. The probability for miracles is $r/(r+s)$, and the probability against miracles is $ts/(t+u)(r+s)$.

These Bernoullian results do not coincide with those produced by an application of Bayes's theorem. Indeed, they imply a non-standard semantics for probability in that the probabilities for and against miracles do not sum to one.[12] Evidently, then, Bernoulli's concept of probability is not that required for Bayes's theorem. Moreover, it is a significant feature of the Bernoullian result that the probability in favour of miracles yielded by the testimony argument, $r/(r+s)$, is not affected when this argument is combined with the observation argument. The probability **against** miracles does indeed change; it increases, but does so in the only way it can, namely at the expense of those cases left undecided by the testimony argument.

This puts a common objection made by Hume's contemporaries in a different and revealing light. For, as the Bernoullian analysis shows, it is possible to understand probability in such a way that however strong the argument **against** miracles may be, it has no effect on the strength of the argument **for** miracles. Is it, though, an understanding which can be attributed to Hume? Is his thinking about probability to be interpreted in Bernoullian terms, or is it as so many have claimed Bayesian? For an answer we must attend to what Hume said.

3. Hume on Chance and Probability

Hume knew something about the doctrine of chances; he knew, for example, that chances could be "calculated." But it is unlikely that he was aware of Bernoulli's work on combining arguments; his knowledge of mathematics was not extensive and it is doubtful whether he would have coupled the algebraic manipulations of the doctrine with questions about how probable arguments can be combined. Hume's pioneering efforts were directed elsewhere.[13]

Nevertheless, there are some significant similarities between Hume's observations on the concept of chance and the idea of equipossible cases of a probable argument. So even if Bernoulli's work was closed to Hume, the concepts presupposed by it were available. To this extent, if no other, Bernoulli-like thoughts were expressed in the *Treatise* chapters on chance and probability which formed the original setting for the argument against miracles. Hume's training as a lawyer may well have influenced his attitude to probabilistic reasoning, and it is recognised that legal probabilities are in certain respects non-standard.

There are, Hume claimed, two important and distinct contexts in which our expectations are uncertain. One of these contexts is that of chance, and Hume elucidated a "species" of probability appropriate to chance events. His view was that although chances are by their very nature equal, they can "concur" in their influence on the mind so as to induce beliefs which are probable. Consider, he suggested, a conventional cubical die which is *no longer supported*. We *cannot without violence regard it as suspended in the air*, so we naturally think of it as having fallen on the table and we *view it as turning up one of its sides*.[14] That is to say, the mind is naturally constrained to envisage a particular effect when presented with a cause; but because in the case of the die the particular effect of the cause *is determin'd entirely by chance* (T 128), the mind is obliged to divide the constraining force of custom and habit into six equal parts. So, *the original impulse, and consequently the vivacity of thought, arising from the cause, is divided* *and split in pieces* (T 129). These equal components are what we call "chances," and in certain circumstances they can unite to produce a superior impulse which will enhance the vivacity of the idea of a particular outcome and thus induce the belief that it, rather than any other outcome, will occur. Suppose, for example, that the die has four sides marked with a circle and two marked with a cross. Then, said Hume, *the impulses of the former are ... superior to those of the latter ... and the inferior destroys the superior, as far as its strength goes* (T 130).[15] Evidently Hume's idea was that it is the **difference between**, rather than the **ratio of**, the number of chances favourable to an outcome and the number of chances unfavourable to it, that measures the strength of our belief for or against its occurrence. Clearly, then, 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.

The other of the two contexts for uncertain beliefs was that of cause, where Hume was concerned to say something about those causal inferences in which the effect is neither a matter of chance nor an invariable consequence of the cause. Thus, if a certain type of effect is observed to follow a certain type of cause in most, but not all, cases, then the argument from cause to effect is a probable argument in the sense to be considered. This *species* of probability arises, Hume said, *where there is a* ***contrariety*** *in our experience and observation* (T 131). To understand this probability we must investigate *how we extract a single judgment from a contrariety of past events* (T 134). In such reasoning our natural inclination to expect the future to be like the past *presents us with no steady object, but offers us a number of disagreeing images in a certain order and proportion*. Consequently, what Hume called the *impulse of the imagination* resulting from this natural inclination, *is here broke into pieces, and diffuses itself over all those images, of which each partakes an equal share of that force and vivacity, that is deriv'd from the impulse* (T 134). Suppose, for example, a cause of type C to have been observed on just ten occasions and on nine of them it has been followed by an effect of type E. C appears for the eleventh time and the psychological force of habit and custom obliges us to anticipate its effect; but because C and E are not invariably conjoined in experience, this psychological force is divided into ten equal components, nine of which favour the occurrence of E and one of which disfavours its occurrence. So, in order to account for the probability of causes, Hume proposed that we treat observations as analogous to chances. *Every past experiment*, he said, *may be consider'd as a kind of chance; it being uncertain to us, whether the object will exist conformable to one experiment or another* (T 135). Just as, in games of chance, some chances are favourable, and some unfavourable, to a predicted outcome, so also some observations of an irregular causal sequence are favourable, and some unfavourable, to a predicted effect. Just as it is in the nature of chances to be equal, so it is in the nature of observations to be equal also. *When we transfer the past to the future,* he said, *every past experiment has the same weight, and ... 'tis only a superior number of them, which can throw the ballance on any side* (T 136). Finally, in reaching a conclusion about what should be expected, *the mind is determin'd to the superior only with that force, which remains after substracting the inferior* (T 138). As in the case of chances, it is the **difference between**, and not the **ratio of**, the numbers of favourable and unfavourable observations, which measures the probability of the argument for that conclusion.

The cogency of these ideas is, no doubt, questionable; but however misguided we may judge Hume to have been, we should notice that in some of his key ideas he had the support of an expert. For Hume's suggestion of an analogy between experiments and chances is not unlike Bernoulli's idea of a probable argument being resolved into a number of equiprobable cases. A simple example will illustrate the similarities. Usually, opium has a soporific effect but occasionally, through the operation of "contrary causes," it fails. So the argument from the fact that a person has taken opium to the conclusion that he will fall asleep, is a probable argument. Bernoulli's view was that such an argument could be resolved into an appropriate number of cases, most but not all of which would favour that conclusion. How, though, did Bernoulli suppose that this number of appropriate cases should be determined? Presumably by considering the observations which are relevant to the argument. If there should be just ten observations of opium taking, in nine of which the taker fell asleep, then Bernoulli would have said that there are ten cases of the argument, nine of which favour the conclusion. So in this version of the example, Bernoulli's equiprobable cases are nothing other than observations, and his account coincides with that which Hume would give. In general, that part of Hume's explanation which invites us to think of experiments as analogous to chances, is compatible with Bernoulli's views. If, for example, there should be one hundred observations of opium taking, ninety of which result in sleep, then although Bernoulli would still have said that there are just ten cases of the argument, nine of which favour the conclusion, he could quite consistently have adopted a Hume-like explanation and supposed that there are one hundred cases, ninety of which favour the conclusion that the opium taker will fall asleep. This is because Bernoulli, like us, thought of probabilities as ratios and for him, therefore, the absolute number of cases was unimportant; but Hume, as we have seen, wanted to subtract the "inferior" number of observations from the "superior" number in order to arrive at a probability, so he could not treat the ten-observation and the one-hundred-observation versions of the example as the same. Nor, it might be said on Hume's behalf, **should** they be treated the same, for surely the absolute number of observations counts for something when a person "proportions his belief to the evidence," and ought not to be so entirely neglected as it is in a Bernoulli-like account.[16]

The discussions of chance and probability in the *Treatise*, then, do not encourage the attribution of Bayesianism to Hume. Not only is there no sign of his thinking in terms of conditional probabilities, but also he quite evidently evaluates probable arguments in a way which cannot be reconciled with the pre-suppositions of Bayes's theorem. For example, an argument of zero probability is, for Hume, one where the favourable cases equal the unfavourable cases; and an argument where there were only favourable, or only unfavourable, cases, would not be a probable argument at all. We think that an event with small probability is unlikely to occur; Hume thought that such an event is more likely to occur than not.

On the other hand, some of Bernoulli's ideas are congruent with Hume's, at least in general terms. Both men resolved probable arguments into equiprobable "cases," and both believed that the probability of a conclusion and the probability of its negation could be zero at one and the same time. What has yet to be seen, though, is evidence that Hume combined probable arguments in a manner like that suggested by Bernoulli. An appropriate place to look for such evidence is that chapter in the *Enquiry* entitled "Of Miracles."

4. "Of Miracles"

Hume divided his essay "Of Miracles" into two parts. In the second and longer part we find a number of sociological, psychological and historical observations of variable value which he used to support his principle point which was that there are insufficient reasons for believing in the occurrence of miracles. His probabilistic argument for this insufficiency is explained in the first part of the essay. Because of the way it is expressed, this argument should be seen, I believe, as Bernoullian rather than Bayesian even though, when it is so seen, its weaknesses become plain.

To begin with, Hume reminded his readers of certain general principles to do with reasoning about matters of fact. In particular, we need to distinguish between those circumstances where the reasoning concerns events which are constantly conjoined, and those where it concerns events which are irregularly conjoined. It is misleading, Hume thought, to describe all such reasoning as probable, for where *conclusions ... are founded on an infallible experience*, a person rightly *regards his past experience as full* ***proof*** (E 110). Where, on the other hand, observed conjunctions are not constant, reasoning may properly be described as probable. For example, since it is only for the most part that there has been *better weather in any week in June than in one in December* (E 110), we have only a probable argument for projecting this claim onto the future. Of these probable arguments, Hume remarked that their strength is best judged by considering *which side is supported by the greater number of experiments* (E 111). It is clear, then, that these preliminary remarks are entirely in accord with what had been proposed in the section on "species" of probability in the *Treatise* and briefly summarised in a preceding chapter of the *Enquiry*. These proposals, I have argued, indicate that Hume thought of probable arguments in Bernoullian rather than Bayesian terms.

To apply these general ideas to the question about miracles we need to consider what they imply about reasoning from the testimony of witnesses. Since there is no necessary connection between any testimony and the occurrence of any event, reasoning from testimony must be reasoning concerning matters of fact. Sometimes testimony, though inconclusive, is unambiguous and unopposed, in which case it is natural and legitimate for us to believe in its truth and even, perhaps, to think that we have a proof of its truth. Nevertheless, as Hume rightly pointed out, *we frequently hesitate concerning the reports of others* (E 112), because the arguments for believing those reports are probable arguments. As we have seen, this must mean that there is evidence both for and against the truth of the reports. When, for example, the testimony of some witnesses is contradicted by that of others, any conclusion we draw will be probable to the extent that the testimony for it outweighs the testimony against it. In such a case, it might[17] be appropriate to estimate the confidence we have in the conclusion by subtracting the number of witnesses testifying against the conclusion from the number testifying in its favour. Other examples of probabilistic reasoning from testimony occur when witnesses are of *doubtful character* or *when they have an interest in what they affirm* or *when they deliver their testimony with hesitation, or ... with too violent asseverations* (E 112-13). In each of these cases, there are, Hume implied, reasons for denying the truth of the testimony and we must try, as best we can, to estimate the extent to which those reasons *diminish or destroy the force of any argument, derived from human testimony* (E 113).

The case which is of more direct importance to miraculous events is when the witnesses's reports are of an event which *partakes of the extraordinary and the marvellous* (E 113). In using this phrase, Hume meant to refer to events, like heat waves in winter and snow storms in summer, which although they may never have occurred, do not violate any laws of nature and are neither *contrary* nor *conformable* to experience. Were it not for reports of extraordinary events, our experience would furnish us with proofs, in Hume's sense, that such events never occur. Given the reports and thus the need to combine a testimony argument with an observation argument, these proofs become probabilities because we have *a contest of two opposite experiences; of which the one destroys the other, as far as its force goes, and the superior can only operate on the mind by the force, which remains* (E 113). Here, it is clear, Hume was using his idea that experiences or observations are analogous to chances and so can be counted, added and subtracted. In effect, a probable argument results from combining two proofs. One proof allows us to doubt, but not to deny, the occurrence of an extraordinary event; the other proof allows us to doubt, but not to deny, its non-occurrence. If, when combined, these proofs yield a probable argument which entitles us to conclude that the extraordinary event did occur, then this means that the force of the proof based on testimony was sufficient to overcome the force of the proof based on common experience.

However, miracles, in Hume's view, are not just extraordinary events; they are events which are contrary to laws of nature. If a miracles, M, occurs, then some supposed law of nature, L, is false, and if L is true, then M cannot occur. There is an argument for believing that M occurs (and consequently that L is false), and also an argument for believing that L is true (and hence that M cannot occur). It is only by combining these arguments in a satisfactory manner that we can reach any conclusion about the occurrence of M and thus about the truth of L.

Now Hume was prepared to grant that *considered apart and in itself* the argument for believing that M occurs, *amounts to an entire proof* (E 114). He was willing, that is, to set aside any suggestion that the testimony for miracles was ambiguous or in any other way faulty, and to allow that the witnesses themselves were unanimous, unbiased and of undoubted character. For, as had already been indicated, if either the witnesses or their testimony could be complained of, there would no longer be a proof, but merely a probability that M occurred. Moreover, it was clearly in Hume's own interest that these concessions were made, for if the testimony favouring M were questioned then it would be hard to resist the claim that the evidence used to discount it should also be questioned.

The proof that M occurred is confronted by a proof that it did not occur. *Unalterable experience has established* (E 114), Hume said, the law which M's occurrence would violate. There is, he said, *a direct and full proof ... against the existence of any miracle* (E 115). The two arguments, then, considered apart and in themselves, yield proofs of contradictory conclusions. It is, as Hume acknowledged, a matter of *proof against proof*, both of which count as *entire* (E 114).

Given that Hume's evident intention was to examine the question of miracles using the ideas he had developed about chance and probability, we expect him to combine these two proofs so as to give a probable argument for, or against, the occurrence of miracles. If, for example, the observations supporting the truth of L outnumber the observations supporting the occurrence of M, then Hume could have used his idea that observations, like chances, are equal, in order to justify the claim that the proof against M is *superior* to the proof for M. Indeed, in the light of remarks made earlier in the chapter, we expect the strength of the resulting probable argument to be estimated by *deducting* the number of observations yielding the lesser proof from the number of observations yielding the greater proof. We expect, certainly, that whatever conclusion is reached it will be supported by a **probable** argument.

However, none of these expectations are satisfied. Hume made no attempt to combine the arguments; he simply invited his readers to adopt, without qualification, the proof against miracles and to reject, without reservation, the proof for miracles. *No testimony is sufficient to establish a miracle*, he said, unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavours to establish (E 115-16). So instead of combining two *entire* proofs, Hume invited his readers to choose the lesser of two miracles. We could choose either to believe that M occurred, or to believe that reports of M's occurrence were made by deceived or deceiving witnesses. Those who choose, as Hume did, to disbelieve reports of M are, in effect, choosing to **ignore** the proof of M's occurrence, for in that case the premises of the proof are false. The testimony argument is not even treated as a probable argument, and reports of M are taken to be not just doubtful but mistaken.

According to Hume, the only circumstance in which we would need to combine arguments would be that in which, for some reason, we chose the belief that M occurred as the lesser miracle. In that case, he said, we would still have to take into account the proof that L is true. There would be *a mutual destruction of arguments, and the superior* — presumably the proof of L — *only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior* (E 116). However, Hume does not say why the observation argument **always** counts, whichever choice of miracle we make, whereas the testimony argument only counts when we choose M as the lesser miracle.

Hume's artificial *choice of miracles* dilemma is, then, specious; only those who have already conceded that miracles do not occur, that is, those who **ignore** the proof from testimony, will draw the conclusion

Hume indicated. It is, of course, possible that reports of M, that is, reports of a falsifying instance of L, are mistaken. Moreover, despite the way the dilemma is expressed, there would be nothing at all miraculous in the realisation of this possibility; but since no question is raised at this stage as to the reliability of the witnesses, the only reason we have for believing, as Hume evidently did, that this possibility is in fact realised derives from the proof of L. Why, though, should defenders of M take this proof into account if rejectors of M are not willing to take the proof of M into account. After all, there is an analogous dilemma which invites the opposite conclusion that it is the argument against M's occurrence which should be rejected. For it might be urged that "no evidence is sufficient to establish a law of nature unless the evidence be of such a kind that its falsehood would be more miraculous than the fact which it endeavours to establish"; and, it could be said, our surprise on learning that the conclusion of an observation argument is true ought to be greater than our surprise on learning that the evidence it uses is faulty. Moreover, unless we have already drawn the conclusion that miracles do not occur and that, *a fortiori*, testimony favouring their occurrence is mistaken, we must allow that there are reasons, based on this testimony, for believing that L is false.

So, despite the studied rehearsal of the principles of probabilistic reasoning as applied to the question about miracles, Hume relied on rhetoric rather than those principles for his conclusion. Everything that he said in the final paragraph of Part I of the essay about the credibility of testimony for miracles can be said about evidence for laws, and it becomes apparent that he ignored miracle testimony rather than answered it. There is, it is true, a decent argument against miracles implicit in the remarks about how probabilistic ideas are relevant to reasoning from testimony, but it was never stated by Hume.

5. Conclusion

Readers of Hume's argument against miracles are, I believe, faced with a difficulty. On the one hand, he introduced his argument as a probabilistic one, and our attempts to understand it are inevitably influenced by our knowledge of twentieth century conceptions of probability and by our relative ignorance of their eighteenth century counterparts. More particularly, we are tempted to read a modern Bayesianism into Hume's argument, for not only does such an interpretation allow us to endorse a persuasive argument, but also it must be acknowledged that there are a few undeveloped remarks in the chapter on miracles which positively invite Bayesian thoughts. On the other hand, the probabilistic principles that Hume invoked are not given a significant role in the final form of the argument, so there is a question about their relevance. Moreover, in the miracles chapter as well as elsewhere, many of Hume's claims about probabilities and their assessment are not consistent with Bayesianism.

There is no simple or clear response to this difficulty. We do well to remember, though, that although Hume was by no means alone in providing an argument concerned with miracles with a probabilistic context, the principles of probable reasoning were by no means settled. New concepts of evidence were being developed, not least by Hume himself, and it would be surprising if our Bayesianism with its rigid presuppositions could be made to fit his miracles argument. If there is novelty in the argument, as Hume claimed, then the suggestion that it consists in his Bayesianism cannot be supported. A more helpful interpretation, despite the questions that it raises about the cogency of the argument, is to be found by looking beyond Hume to a style of probable reasoning which I have associated with Jakob Bernoulli. The cogency of question should not, I think, oblige us to resist this interpretation, for it seems clear that Hume could have constructed a respectable probabilistic argument yielding the conclusion that miracles are incredible. Perhaps it was because others among his contemporaries were using just such an argument, that Hume saw, and tried to satisfy, the need for something more powerful.

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1. A version of this paper was read at the 16th Hume Conference, Lancaster, England, in August 1989. I am grateful to David Owen for his helpful comments on that occasion. Correspondence with Howard Sobell and careful scrutiny by Martin Hughes have also been very useful in clarifying the issues. None of these helpers would, I believe, wish to be associated with the conclusion I draw.
2. B. Langtry, "Miracles and Principles of Relative Likelihood," *International Journal for the Philosophy of Religion*, 18 (1985): 123-31; J. H. Sobel, "On the Evidence of Testimony for Miracles: a Bayesian Interpretation of David Hume's Analysis," *Philosophical Quarterly* 37 (1987): 166-86; D. Owen, "Hume versus Price on Miracles and Prior Probabilities: Testimony and the Bayesian Calculation," *Philosophical Quarterly* 37 (1987): 187-202; G. Schlesinger, "Miracles and Probabilities," *Nous* 21 (1987): 219-32. A Bayesian interpretation of Hume's *Dialogues Concerning Natural Religion* is given in W. Salmon, "Religion and Science: a New Look at Hume's *Dialogues*," *Philosophical Studies* 33 (1978): 143-76; cf. D. Raynor, "Hume's Knowledge of Bayes' Theorem," *Philosophical Studies* 38 (1980): 105-6.

3. See Hume's letter to G. Campbell, first published in the latter's *A Dissertation on Miracles*, 2nd ed. (Edinburgh, 1812).
4. See E. C. Mossner, *The Life of David Hume*, 2nd ed. (Oxford, 1980), 113.
5. In a letter to Richard Price, dated 18 March 1767, Hume conceded that *the light, in which you have put this Controversy, is new and plausible and ingenious, and perhaps solid*, and he expressed an intention to find *time to weigh it*, but there is no evidence that the intention was carried out. See R. Klibansky and E. C. Mossner, eds., *New Letters of David Hume* (Oxford, 1954), 234.
6. S. Stigler, "Thomas Bayes's Bayesian Inference," *Journal of the Royal Statistical Society*, ser. A, 145 (1982): 250. It has been suggested, with some plausibility, that the reason Bayes himself did not arrange for the publication of his own memoir was that he was fully conscious of the shortcomings it had in the eyes of fellow mathematicians; see S. Stigler, *The History of Statistics* (Cambridge, Mass., 1986), 129-30, for comments on this issue.
7. R. Price, *Four Dissertations* (London, 1767). Part of the final dissertation is concerned with Hume's argument. On p. 383 Price refers to Campbell's 1762 reply (above, n. 3) as having been published "some time after this dissertation had been composed." The footnote referring to Bayes's memoir, on pp. 395ff., was evidently an afterthought.
8. See Sobel (above, n. 2), 181, and Owen (above, n. 2), 199. Sobel thinks that Price was no Bayesian. That may be so, but at least he was not anti-Bayesian.
9. Other early applications were explored by John Arbuthnot in "An Argument for Divine Providence, Taken from the Constant Regularity Observ'd in the Births of Both Sexes," *Phil. Trans.* 27 (1710): 186-90; and by Daniel Bernoulli in "Recherches physiques et astronomiques sur le probleme ... Quelle est la cause physique de l'inclination des plans des orbites des planetes," *Receuil des pieces qui ont remporte le prix de l'Academie Royale des Sciences* 3 (1735): 93-122 (French version), 123-44 (Latin version). For a discussion of Daniel Bernoulli's application of probability to astronomy, see Barry Gower, "Planets and Probability: Daniel Bernoulli on the Inclinations of the Planetary Orbits," *Studies in the History and Philosophy of Science* 18 (1987): 441-54.
10. Jacobi Bernoulli, *Ars Conjectandi* (Basileae, 1713). Reprinted in *Die Werke von Jakob Bernoulli*, hrsg. von der Naturforschenden Gesellschaft in Basel, Bd. 3 (Basel, 1975), 218-20. This reprint preserves the original pagination. For aid with Bernoulli's reasoning, see G. Shafer's very helpful "Non-additive Probabilities

in the Work of Bernoulli and Lambert," *Archive for History of the Exact Sciences* 19 (1978): 309-70, esp. 328-37.

11. Bernoulli's mathematical treatment of the combination of arguments was developed and amended by J. H. Lambert (1728-77). For details, see Shafer (above, n. 10), 349-63.
12. This is made clear in Shafer (above, n. 10).
13. See I. Hacking, "Hume's Species of Probability," *Philosophical Studies* 33 (1978): 21-37.
14. David Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge, 2nd ed., rev. ed. P. H. Nidditch (Oxford, 1978), 128. Further references ("T") will be given in parentheses within the body of the text.
15. The same point is made in the short chapter on probability in the *Enquiry*; see David Hume, *Enquiries Concerning the Human Understanding and Concerning the Principles of Morals*, ed. L. A. Selby-Bigge, 3rd ed. (Oxford, 1975), 56. Further references ("E") will be given in parentheses within the body of the text. E. J. Lowe in his "Miracles and the Laws of Nature," *Religious Studies* 23 (1987): 263-78, has drawn attention to Hume's understanding of probability in terms of differences rather than ratios.
16. For a discussion of what is called the "weight" of arguments, see J. M. Keynes, *A Treatise of Probability* (London, 1921), chap. 6. Also, L. J. Cohen, "Twelve Questions about Keynes's Concept of Weight," *British Journal for the Philosophy of Science* 37 (1986): 263-78.
17. It would be appropriate only if the witnesses report independently of each other.