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How Good Was Bayes' Response to Hume?

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Abstract. In this paper, we evaluate Bayes' posthumously-published "Essay Towards Solving a Problem in the Doctrine of Chances" understood as an attempt to answer two skeptical challenges raised in Hume's *Enquiry*. We argue that Hume should have rejected the claim that Bayes provided a non-skeptical solution to his challenges but that he could have accepted Bayes' result as a formal account of his own skeptical solution.

Keywords: David Hume, Thomas Bayes, problem of induction, chance, skepticism

How Good Was Bayes' Response to Hume?

In his 1989 paper on the rule of succession (especially Section 4), Sandy Zabell argues that the central results of Thomas Bayes' posthumously-published essay, "Towards Solving a Problem in the Doctrine of Chances," were developed in response to the inductive skepticism laid out in Hume's *Enquiry*.[1] The relationship between Hume and Bayes is still underappreciated by historians of philosophy. For example, Morris and Brown's (2014) entry on Hume in the Stanford Encyclopedia of Philosophy mentions neither Bayes nor his literary executor, Richard Price. Similarly, the Blackwell *Companion to Hume* (2008) has no index entries for Bayes or for Price and only one entry for "Bayesianism," which points to a discussion of Bayesian analyses of Hume's argument against miracles. Morris' chapter on "Hume's Epistemological Legacy," in the Blackwell volume discuses *seven* historically-important responses to Hume's inductive skepticism but never mentions Bayes or Price. And while there is a considerable literature on Bayesian responses to Hume's argument against miracles (for which, see Owen, 1987; Earman, 1993, 2000, 2002, 2013; Holder, 1998; Millican, 2003, 2013; and Vanderburgh 2005, 2019), to the best of our knowledge, no one has discussed whether or to what extent Bayes' argument satisfactorily answered the skeptical challenges raised by Hume.[2]

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[1] Zabell points to two pieces of evidence. First, in his 1749 *Observations on Man*, David Hartley mentions having had a solution to the inverse problem—the topic of Bayes' essay—communicated to him from "an ingenious Friend." Second, Dale (1986) discovered in one of Bayes' notebooks a proof of a rule that appears in his essay. Although the proof itself is undated, it occurs on pages between entries dated July 4, 1746, and December 31, 1749. Zabell dismisses Stigler's (1983) contention that Hartley's ingenious friend might have been Nicholas Saunderson and not Bayes. Zabell further suggests that the publication of Hume's *Enquiry* in 1748 motivated Bayes to solve the inverse problem, but as far as we know, there is no direct evidence that Hume's skeptical challenge led to Bayes' famous paper.

[2] Henderson's (2018) SEP entry on the problem of induction includes a very illuminating section (3.2.2) on Bayesian solutions, including some historical remarks on Bayes and Laplace. And Skyrms (2014) provides a very interesting history of work on the problem of induction, including some brief remarks (305-307) on

As far as we know, there is no evidence that Hume read Bayes' paper, which was published posthumously in the *Philosophical Transactions of the Royal Society* in 1763. However, Hume knew Price, who had submitted Bayes' paper to the Royal Society with some additions of his own and who published a demonstration of Bayes' "second rule" in 1764. In 1767, Price published his *Four Dissertations*, the last of which concerned historical and testimonial evidence, and especially, the evidence for Christianity. In a long footnote in his fourth dissertation, starting on page 396 of the 1767 edition, Price referred his reader to Bayes' essay. Price wrote that Bayes "proves, that so far is it from being true, that the understanding is not the faculty which teaches us to rely on experience, that it is capable of determining, *in all cases*, what conclusions ought to be drawn from it, and what *precise degree* of confidence should be placed in it" (398).[3] Hume was at least aware of Price's work. According to Price's *Memoirs* (Morgan 1815), "In [his fourth] dissertation, which was intended as an answer to Mr. Hume's arguments against the credibility of miracles, Mr. Price had, as he thought, expressed himself

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Bayes and Price, in terms of grades of skepticism. But neither Henderson nor Skyrms try to evaluate Bayes' arguments from a Humean perspective. Weintraub (2008) discusses a very general challenge leveled by Price against Hume's positive account of induction, but she does not discuss the adequacy of the Bayesian reply to the skeptical challenge or how Price's objection to Hume's account of induction might or might not fit within the Bayesian framework. Howson (2000) considers at length the prospects for Bayesianism to answer Hume's inductive skepticism, for which see especially Chapters 4, 7, 8, and 9. However, Howson does not consider Bayesianism as an historical response to Hume's challenge. Nor does he assess whether *Hume* would have or should have been convinced by the Bayesian argument. Similarly, Lange (2011) discusses Bayesian approaches to Hume's problem—but not in an historical setting. Millican (1996) also considers a range of replies to Hume's challenge, but like Howson and Lange, he does not address the adequacy of Bayes' argument to the historical Hume. Vanderburgh (2005, 2019) is explicitly concerned with Bayesian criticisms of Hume's argument for skepticism about miracles. However, if he is right when he claims that Hume's account of probability—and with it, any acceptable account of evidence—resists mathematical treatment (for which, see Chapter 4 of his 2019), then Hume would clearly reject Bayes' argument root and branch. We reject Vanderburgh's claim that Hume's account of probability cannot be formalized, but rather than attacking his argument, we think it is better to evaluate Bayes' assumptions directly from a Humean perspective, as we do in Section 3.

[3] We think it is worth remarking here that in offprints of Bayes' essay prepared for Price in 1764, the title was changed to "A Method of Calculating the Exact Probability of All Conclusions founded on Induction" (for which, see Stigler 2013).

improperly, by speaking of the *poor sophistry* of those arguments, and using other language of the same kind" (23-24). Price sent a copy of his *Dissertations* along with a note of apology for his uncharitable language (which was removed or altered in subsequent editions of the *Dissertations*), and in March 1767, he received a cordial reply from Hume, in which Hume thanked Price for addressing him "as a man mistaken, but capable of Reason and conviction" (Price et al. 1983, 46). Hume went on (46):

> I own to you, that the Light, in which you have put this Controversy, is new and plausible and ingenious, and perhaps solid. But I must have some more time to weigh it, before I can pronounce this Judgment with Satisfaction to myself. My present Occupations shall not deprive me of the Leizure requisite for that Purpose; as no Object can possibly have equal Importance. These Occupations, however, have bereav'd me of the satisfaction of waiting on you, and of thanking you in person for your Attention, which I should have thought my Duty, if I did not find my time so fully employ'd.

According to Price's *Memoirs* (16-17), Hume and Price had at least two conversations in person—one at a dinner party that Hume arranged for three of his cordial critics and one at Price's house. But despite what he said about the importance of the topic in his letter to Price and despite (according to Price's *Memoirs*) being convinced "that his arguments were inconclusive," Hume never discussed the arguments of Bayes or Price in any detail in print, so far as we know. He certainly did not make any changes to his *Enquiry* either agreeing or disagreeing with Bayes and Price on induction or testimony. The question that interests us, then, may be put somewhat as follows: What would Hume have said if, contrary to fact, he had carefully read Bayes' paper and offered a considered reply?[4]

[4] We can imagine more and less "distant" scenarios. For example, we might consider what would have happened if Hume had been prompted to read Bayes' paper by Price's *Dissertations* and had executed a serious study in 1767. Or we might consider what would have happened if Bayes had published his work in 1749 or 1750 as a direct response to Hume's *Enquiry* and then Hume had made a reply in print.

We could try to separate the descriptive question of what Hume *would* have said from the normative question of what he *should* have said given the constraints of his basic philosophical commitments, but for the purposes of this paper, we will treat those questions as identical. Hence, we are not aiming to evaluate Bayes' argument or Hume's position in any absolute sense; nor are we concerned to correct any mistakes made by Bayes or by Hume. Rather, we intend to give an account of what an idealized Hume might have said about Bayes' attempt to address the skeptical challenges of the *Enquiry*. We argue that Hume would have rejected the claim that Bayes had provided a non-skeptical solution to his challenges. However, we think that Hume would have embraced Bayes' results as a formal precisification of his own skeptical solution. Here is how we will proceed. In Section 1, we describe two skeptical challenges that Hume raised in the *Enquiry*, and we review Hume's skeptical solution. In Section 2, we discuss Bayes' solution to what has come to be called the "inverse" problem.[5] We evaluate whether Hume would have accepted each of three assumptions that Bayes makes in proving his main result, which is stated in Proposition 9 of his essay, namely: (1) the common assumption that geometrical reasoning is demonstrative, (2) the definition of the probability of an event as the ratio of the rational expectation of a gamble depending on the event to the value of the gamble on the occurrence of the event, and (3) the postulate that there is a square table "so made and levelled" that if any ball be rolled on it, "there shall be the same probability that it rests upon any one equal part of the plane as another, and that it must necessarily rest somewhere upon it" (302). We argue that Hume would have accepted the first two assumptions, though he would have had reservations about

[5] For an elaborate discussion of the history of the inverse problem, see Dale (1999).

applying Bayes' definition of probability in a non-skeptical way. We further argue that Hume would have accepted Bayes' postulate understood as a purely geometric object in the same way that he accepted other purely geometrical objects but that he would have had reservations about assuming that the postulate applies to any object of actual experience. In Section 3, we discuss Bayes' Scholium to Proposition 9, in which he gave an argument that we think was meant to satisfy Humean reservations regarding the application of his results to arbitrary cases where we know nothing about how often an event occurs "antecedently to any trials made concerning it." We argue that Hume would have accepted Bayes' reasoning as a formally-precise presentation of his own skeptical solution to his skeptical doubts, while rejecting Bayes' argument as a straightforward, *non*-skeptical solution.

1. Skeptical Doubts and Skeptical Solution

Scholars disagree about whether Hume should be read as issuing a descriptive challenge or as issuing a normative challenge with respect to our inductive practices.[6] Scholars also disagree about whether Hume took our ordinary inductive practices to be justified, and if so, how.[7] On the traditional interpretation, Hume argued that induction is never justified. On one plausible alternative interpretation, Hume accepted that induction *is* justified,

[6] Garrett (1997, 1998) defends a descriptive interpretation. Millican (1995, 1998) defends a normative interpretation. See Qu (2014) for more detail.

[7] For example, Stove (1965) argues that Hume offered a challenge only to "deductivists" and hence left the door open for ordinary inductive practices to be rationally justified. By contrast, Millican (1995) argues that Hume ruled out that our ordinary inductive practices are rationally justified without proposing any substitute source of justification. And Loeb (2006) argues that while Hume ruled out that our ordinary inductive practices are *internally* justified by reason, he maintained that they are nonetheless *externally* justified by custom and habit.

while wondering *how it comes to be so*.[8] Both points of disagreement are nicely illustrated by the following passage from Hume's first *Enquiry*:

> These two propositions are far from being the same, *I have found that such an object has always been attended with such an effect*, and *I foresee, that other objects, which are, in appearance, similar, will be attended with similar effects*. I shall allow, if you please, that the one proposition may justly be inferred from the other: I know, in fact, that it always is inferred. But if you insist that the inference is made by a chain of reasoning, I desire you to produce that reasoning. (E 4.2.29)

The first point of disagreement is bound up with how we are supposed to read Hume's request to see a chain of reasoning. If Hume was issuing a normative challenge, we should read him as asking for a chain of *good* or *valid* reasoning.[9] If he was issuing a descriptive challenge, we should read him as asking for some reasoning or other that we actually engage in, even if that reasoning is defective from a normative point of view. The second point of disagreement is bound up with how we read Hume's allowance that the one proposition may be justly inferred from the other. On the traditional interpretation, Hume was setting up a reductio. On the non-skeptical alternative, he should be read as sincerely willing to accept that induction is justified.

The first point of disagreement has to do with how we understand Hume's skeptical doubts. On the descriptive view, Hume was skeptical about a theory of human cognition according to which we are determined by reason to draw inductive inferences. On the normative view, Hume was skeptical about the power of reason to justify the inductive inferences we routinely make. The second point of disagreement has to do with how we understand Hume's skeptical solution. On the descriptive view, he offered a new

[8] See Loeb (2008, 108-110) for discussion of traditional and non-traditional readings of Hume on induction.

[9] Stove (1965, 172-173) opts for the strong view that for Hume, the phrase "any argument" means "any *valid* argument."

theory of human cognition according to which we are determined to draw inductive inferences by custom and habit, not reason. On the normative view, he proposed a new source of justification for the inductive inferences we routinely make. In the terms we have just set out, we think there are three live options. One may take both the challenge and the solution to be descriptive. One may take the challenge to be normative and the solution to be descriptive (though it is at best unclear whether "solution" is an appropriate label in this case). Or one may take both the challenge and the solution to be normative.

We think Bayes is most naturally read as proposing a non-skeptical, normative solution to a normative challenge—or really, as we will see, a pair of challenges. But that still leaves the *specific details* of the challenge unspecified. So we want to say what we think Bayes took Hume's skeptical doubts to be. Hume famously divided human reasonings into two kinds: those involving *relations of ideas* and those involving *matters of fact*. To know a proposition regarding relations of ideas does not rely, in any sense, on past experience. Rather, we know them either intuitively or demonstratively via the application of reason. To know a proposition about a matter of fact, by contrast, requires experience.

A traditional normative formulation of Hume's challenge appeals to his division of reasonings (sometimes called *Hume's Fork*) in order to argue that induction is not justified. On the one hand, no inductive inference is demonstrative, since the negation of the conclusion can always readily be imagined and hence, contains no contradiction. On the other hand, every inductive inference presupposes the Principle of the Uniformity of Nature, which says that "instances, of which we have had no experience, must resemble those, of which we have had experience, and that the course of nature continues always uniformly the same" (T 89). But the Principle of the Uniformity of Nature is itself not intuitively or demonstratively true, and it cannot be justified by way of experience without vicious circularity. Hence, induction is not justified.

However, we think Bayes was concerned to answer a slightly different pair of challenges set out in Hume's *Enquiry*. The first challenge, which we call *the challenge of extension*, is how to justify inferences from past experiences of a certain type to future cases. Specifically, Hume suggested that we sometimes experience the conjunction of some superficial, sensible qualities of objects with some secret, natural powers, and he asked why we should expect future instances of those superficial qualities to be conjoined with those same secret powers.[10] The superficial qualities of objects, but not their secret powers, may be known by way of direct sensation. As Hume put it (E 4.2.29, 33), "Our senses inform us of the colour, weight, and consistence of bread; but neither sense nor reason can ever inform us of those qualities which fit it for the nourishment and support

[10] One might balk at the claim that Hume suggested we observe any conjunctions between superficial qualities and secret powers. After all, in writing about the idea of necessary connexion, he said (E 7.2.58, 74), "As we can have no idea of any thing which never appeared to our outward sense or inward sentiment, the necessary conclusion *seems* to be that we have no idea of connexion or power at all, and that these words are absolutely without any meaning, when employed either in philosophical reasonings or common life." He went on to locate the origin of our ideas of power and necessary connection in our *feeling* that two events are connected—a feeling we acquire by custom and habit upon observing a constant conjunction between those events. Acknowledging this much, we want to make three observations. First, we have been closely following Hume's own statement of his arguments in Section 4 of the *Enquiry*. Hume made liberal use of the term "power" in setting out his skeptical doubts, and we are merely setting out the arguments in the same way. Second, there is a scholarly debate as to Hume's considered view regarding the ideas of power, causation, and necessary connection. For example, Strawson (1989) defends a skeptical realist interpretation according to which "it never really occurs to [Hume] to question the existence of causal power … even in his most extravagantly sceptical (or Pyrrhonian) mode" (2). Whereas, Beebee (2006) defends a projectivist interpretation according to which "in speaking and thinking causally, we express our habits of inference and project them on to the world" (143). See Garrett (2009) for an overview of the debate. We intend to be agnostic with respect to that debate. Third, we do not need to attribute to Hume an all-things-considered commitment to the existence of powers or to our cognitive ability to access them. Rather, we *could* read Hume as making an argument that grants to his non-skeptical opponents that we can observe secret powers indirectly when they act, just as we may observe the wind by way of its action on the leaves of a tree. We think Hume could grant that much to his opponents, so long as he does not grant that powers may be observed directly, in the way that superficial qualities may be observed, or inferred demonstratively from the presence of some superficial qualities without the aid of further experience.

of a human body." We always *presume* that objects having like sensible qualities have like secret powers, and hence, we expect that objects with like sensible qualities will produce like effects (E 4.2.29, 33). Hume asked "why this experience should be extended to future times, and to other objects, which for aught we know, may be only in appearance similar," and he emphasized that the question of why we ought to extend past experience to future cases is *the main question* (4.2.29, 33-34). Reading the "should" in "why this experience should be extended" in a normative way yields the first challenge. Here is a restatement of the first challenge. We have experienced a certain number of objects that had some sensible qualities and some secret powers. We observe in a new object the same sensible qualities as before. May we justly infer that the new object also has the same secret powers as before, and if so, on what basis?

Hume reported that he could not find any chain of reasoning by which we might justly draw an inference from the proposition that an object having *these* sensible qualities has always been attended with *this* effect to the proposition that a new object having similar sensible qualities will have a similar effect. After giving his negative argument, Hume then gave an explicit positive argument based on his division of the branches of human knowledge into those concerning relations of ideas and those concerning matters of fact.[11] The second challenge, which we call *the challenge of repetition*, arose in the course of making his positive argument. Hume observed that an inference from a single case is much different than an inference from a large number of similar cases. But it seems evident that an inference made *by reason* would be "as perfect

[11] Hume wrote that his negative argument "must certainly, in process of time, become altogether convincing, if many penetrating and able philosophers shall turn their enquiries this way and no one be ever able to discover any connecting proposition or intermediate step, which supports the understanding in this conclusion" (E 4.2.30, 34).

at first, and upon one instance, as after ever so long a course of experience." Hume went on (4.2.31, 36):

> It is only after a long course of uniform experiments in any kind, that we attain a firm reliance and security with regard to a particular event. Now where is that process of reasoning which, from one instance, draws a conclusion, so different from that which it infers from a hundred instances that are nowise different from that single one? This question I propose as much for the sake of information, as with an intention of raising difficulties. I cannot find, I cannot imagine any such reasoning. But I keep my mind still open to instruction, if any one will vouchsafe to bestow it on me.

We now restate the problem. We observe some number of objects in which some sensible qualities are (or are presumed to be) conjoined with some secret powers. We infer that a new object having the same sensible qualities will have the same secret powers. But the strength of our expectation that the new object will have the same secret powers depends on the number of objects that we have observed. If we have observed *many* objects in which these sensible qualities have been conjoined with those secret powers, then we have a strong expectation that we will find the same secret powers in a new object having those sensible qualities. But how and why does the number of observations matter?

Hume's own skeptical solution appeals to custom and habit to explain our tendency to infer similar powers in objects having similar superficial qualities and to explain how the strength of our expectation depends on the number of cases we have observed. As we remarked at the beginning of this section, it is a controversial matter whether the appeal to custom and habit also *justifies* our ordinary inductive practices. Scholars who favor a normative reading think Hume's solution is skeptical with respect to the power of reason but not with respect to justification simplicater. Scholars who favor a descriptive reading think Hume's solution either answers to a descriptive challenge or else changes the subject from justification to explanation.

2. Three Bayesian Assumptions

We think Bayes is most naturally read as proposing a non-skeptical solution to a normative reading of Hume's challenges. On our view, Bayes set out to provide a rational justification for induction: to show that, as Price (1767, 398) put it, "[the understanding] is capable of determining, *in all cases*, what conclusions ought to be drawn from [experience], and what *precise degree* of confidence should be placed in it." Bayes (1763/1958) made his case by first providing a geometrical solution to the following "inverse" problem (298):

> *Given* the number of times in which an unknown event has happened and failed: *Required* the chance that the probability of its happening in a single trial lies somewhere between any two degrees of probability that can be named.

Bayes explicitly defined "chance" to have the same meaning as "probability." So, the inverse problem is to find the (second-order) probability that the (first-order) probability of an event is in a given interval. Earman (1990) argues that Bayes used two conceptions of probability in his essay—one in terms of propensity or some other *aleatory* idea and one in terms of rational degree of belief or some other *doxastic* idea. But we think Bayes used a single doxastic notion of probability, which he defined in terms of expectations. We will return to Bayes' conception of probability and to how it figures in his answer to Hume's challenges later in this section and again in Section 3.

Before discussing the details of Bayes' argument, we want to indicate how solving the inverse problem could be relevant to Hume's extension and repetition challenges. In the first place, recall that the main question for Hume was how our ordinary inferences from past observations of the conjunction of some sensible qualities and some secret powers to the conclusion that there will be a conjunction between those same sensible qualities and those same secret powers in a new case could be justified. In the inverse problem, the observed conjunctions are occasions when the event has happened.[12] If successful, Bayes would have shown that when there is “a long course of uniform experiments” in which a secret power is conjoined to some superficial qualities, a demonstrative argument may be given for inferring their conjunction in a new case. At the same time, Bayes answered Hume’s repetition challenge by displaying a chain of reasoning showing how and why the number of observations matters, even when each event observed is entirely similar to every other.

Bayes solved the inverse problem by way of a clever geometrical argument from a postulate and a definition. The main mathematical result in Bayes’ paper is Proposition 9, which provides a rule whereby “without knowing anything more concerning [the event described in the proposition], one may give a guess whereabouts it’s [sic] probability is, and … see the chance that the guess is right” (305). But an essential part of his answer to Hume’s challenges was left for the Scholium to Proposition 9, where he argued that “the same rule is the proper one to be used in the case of an event concerning the probability of which we absolutely know nothing antecedently to any trials made concerning it” (305). In the rest of this section, we discuss what we take to be the three philosophical assumptions that Bayes needed for his solution to the inverse problem to be poised to underwrite a non-skeptical solution to Hume’s skeptical doubts.

The first assumption that Bayes required is that geometrical reasoning is demonstrative. Of course, Bayes did not make the assumption explicit in his argument,

[12] The way Hume puts the problem in the passages we have quoted is a special case of the inverse problem that Bayes solves. Specifically, Hume’s problem is the special case where the number of times the unknown event fails to occur is zero.

but for his argument to serve as an answer to Hume, it must be the case that geometrical reasoning is demonstrative. For if geometrical reasoning were merely probable, then using it to provide an answer to Hume's challenge would be viciously circular. One might be tempted to say that the assumption that geometrical reasoning is demonstrative is trivial, but Hume adopted radically different positions on the status of geometry in his earlier and later works. In the *Treatise*, Hume maintained that geometry "falls short of that perfect precision and certainty, which are peculiar to arithmetic and algebra … because its original and fundamental principles are deriv'd merely from appearances" (T 1.3.1.71). However, in the *Enquiry*, Hume asserted that geometry is a science that is intuitively or demonstratively certain—a science of the relations of ideas. According to the later Hume, "the truths demonstrated by Euclid would for ever retain their certainty and evidence" even if there were no circles or triangles in nature (E 4.1.20).[13] We take the *Enquiry* to represent Hume's mature philosophy, so when the two are in conflict, we take the *Enquiry* to be the official Humean doctrine. Hence, we assume that Hume would accept Bayes' geometrical reasoning as demonstrative. What Hume would have said about the *application* of Bayes' geometrical reasoning is another matter, which we will take up again shortly.[14]

The second assumption that Bayes required is stated in his definition of the probability of an event (298):

> The *probability of any event* is the ratio between the value at which an expectation depending on the happening of the event ought to be computed, and the value of the thing expected upon it's [sic] happening.

[13] For further discussion of Hume's change of view on geometry, see Batitsky (1998).
[14] We think it plausible that Bayes, too, took the *Enquiry* to be the official Humean view. In this, he would have been in good company. Mary Shepherd (1824), for example, felt compelled to justify her use of the *Treatise* in responding to Hume.

Bayes was not explicit about the meaning he gave to the term "ought" in his definition of probability. We think it is best understood as appealing to an objective but agent-relative epistemic norm.[15] Roughly, the value at which an expectation ought to be computed by an agent is the value that an epistemically rational agent would compute given the ordinary agent's evidence. Hence, we take Bayes to have defined the probability of an event as the ratio of [1] the rational expected utility of a gamble that depends on whether the event occurs to [2] the utility of the payout expected from the gamble if the event occurs. We understand Bayes' definition to say that if the probability of some event is r, then a rational bettor would take the fair odds to be $\frac{r}{(1-r)}$ in favor of the event happening. Given our understanding of Bayes' definition, the inverse problem may be stated as follows. Suppose some event *E* is known to have happened *p* times and to have not-happened *q* times in ($p + q$) total occasions for its happening. What are the fair odds one ought to give for the event that the rational fair odds to give for *E* happening are within some specific interval? Bayes purported to answer that question.

Would Hume have accepted Bayes' definition of probability? Hume himself did not define probability in terms of beliefs or expectations, whether rational or not. Rather, he seems to have thought of the probability of an event as some kind of count with respect to basic or elemental chances (or causes). But he did maintain in both the *Treatise* and the *Enquiry* that probabilities and degrees of belief are intimately related. For example, in the brief section on probability in the *Enquiry* (E 1.6.46, 56), Hume maintained:

[15] Or maybe better to say that it is relative to the informational or evidential state of an agent. Hence, we understand Bayes' "ought" to be similar to what is sometimes misleadingly called the "subjective ought" today. See Schroeder (2009), Carr (2015), and Olsen (2017) for examples.

> There is certainly a probability, which arises from a superiority of chances on any side; and according as this superiority encreases, and surpasses the opposite chances, the probability receives a proportionable encrease, and begets still a higher degree of belief or assent to that side, in which we discover the superiority. If a dye were marked with one figure or number of spots on four sides, and with another figure or number of spots on the two remaining sides, it would be more probable, that the former would turn up than the latter; though, if it had a thousand sides marked in the same manner, and only one side different, the probability would be much higher, and our belief or expectation of the event more steady and secure.[16]

The precise details of Hume's theory of probability have been and continue to be a matter of controversy.[17] However, we think that this much is obvious: Hume took probability to be reducible somehow to a count of elementary possible outcomes, each of which is of equal weight or value; and he took probabilities to generate *proportionate* degrees of belief.

We think Hume would have found Bayes' definition unobjectionable in itself. After all, as he said in the *Treatise* (1.3.11, 124), everyone is free to use their terms as they like. Moreover, we think that Hume would have endorsed the spirit of Bayes' definition, which cuts right to the crucial matter of *belief* or *expectation*. The only reservation we think Hume might rightly have had is with how to understand the crucial

[16] Hume wrote very similar things about essentially the same example in the *Treatise* (T 1.3.11, 125-127).
[17] Attempts to make sense of Hume's account of probability, especially in the context of what he says about testimony and miracles, go back at least to Peirce's discussions (1878, 1901) of the method of "balancing reasons." More recently, scholars have disputed as to both Hume's metaphysics and his logic of probability. For example, Murdoch (2002) claims that Hume's account of probability is straightforwardly classical. Owen (1987) argues that Hume's account was Bayesian or proto-Bayesian. Gower (1991) argues that Hume's account was not Bayesian and perhaps even *anti*-Bayesian. Mura (1998) argues that Hume's account of probability was a sophisticated, albeit informal, variety of Bayesianism and that Hume endorsed several principles developed formally in Carnap's programmatic writings on probability and confirmation. Coleman (2001) and Vanderburgh (2005, 2019) argue that Hume's account of probability is not even the familiar "Pascalian" variety but rather a "Baconian" account that descends from an ancient Roman legal tradition. Similar disagreements plague the literature on Hume's argument against miracles, with some authors giving Bayesian reconstructions favorable to Hume, others giving Bayesian reconstructions unfavorable to Hume, and still others arguing that Bayesian reconstructions are completely inappropriate. We think that Hume is ultimately responsible for most of the disagreement, since his writings on probability are, to put it charitably, not a model of clarity and precision.

"ought" in Bayes' definition. Specifically, we think that Hume might have doubted the applicability of Bayes' definition to actual human reasoning. However, doubts about the application would not jeopardize Bayes' mathematical results any more than the theorems of Euclidean geometry would be jeopardized if there were no circles in nature.

The third assumption that Bayes required is the postulate that there is a square table "so made and levelled" that if any ball be rolled on it, "there shall be the same probability that it rests upon any one equal part of the plane as another, and that it must necessarily rest somewhere upon it" (302). Whether Hume would have accepted Bayes' postulate is a surprisingly delicate question. In the rest of this section, we consider two features of the postulate that we think would have been especially relevant to Hume and then we describe some reservations that we think Hume would have had.

At least at first blush, Bayes' postulate makes a matter of fact assertion about the actual world. It says that there exists a table such that whenever a ball is rolled on it, the ball necessarily stops somewhere on the table, and whenever a ball is rolled on it, the probability that the ball stops on any given area of the table's surface is the same as the probability that it stops on any other area of equal size. Now, there is a sense in which Bayes' postulate understood as a factual assertion was a very weak one. Bayes only required the existence of such a table. He didn't require that every table was like the one in his postulate or that most of them were. Moreover, he didn't require that we be able to *identify* any actual table satisfying the postulate. But even though the postulate is very weak when understood as a claim about an actual table, Hume would not have accepted it. After all, it isn't a *logical* truth that when we roll a ball on a table, the ball comes to rest somewhere on the table (cf. Murdoch 2002, 188-189). Among other things, Bayes' postulate implicitly asserts that the ball and the table continue to exist throughout the experiment, that the ball steadily loses speed until it comes to rest, that it does not turn into a dove and fly away, and so on. As Hume emphasized in the *Treatise*, “there must always be a mixture of causes among the chances, in order to be the foundation of any reasoning” (T 1.3.11, 126).

Moreover, we think Hume would have had reservations about the geometrical properties ascribed to the table. As we have noted already, in the *Enquiry*, Hume accepted that geometrical reasoning was demonstrative, but he did not concede that the *objects* of geometry actually existed. We suspect that Hume would have denied that we could ever have sufficient reason to think that an actual table has an infinitely-divisible surface or to think that ordinary people have infinitely-divisible degrees of belief. But whereas the theorems of geometry would not be thrown into doubt by the fact that the objects of geometry do not exist in the actual world, the *applicability* of the theorems of geometry to the actual world would be. Vanderburgh (2019, 119) suggests a related reservation, writing:

> Bayes … 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.

Following Vanderburgh’s suggestion, one might argue that even if Hume had accepted that *some* actual table had the geometrical properties required by Bayes’ postulate, he would have denied that the rule Bayes derived in Proposition 9 was widely applicable. We think Bayes anticipated these reservations. On our reading, Bayes intended his postulate to be understood as describing a *mathematical* object, not an actual table.

Hence, the table was a literary conceit—an aid to thinking. As we read him, in Proposition 9, Bayes produced a mathematical *model* for inductive reasoning, and he argued for the *applicability* of that model in the Scholium to Proposition 9.

Hence, we want to consider whether Hume would have accepted Bayes' postulate on the understanding that it describes a mathematical object, rather than an actual table. Understood in abstract geometrical terms, the postulate says that we can describe a square on whose surface we may choose a point such that every pair of equal-sized regions that might be drawn on the square have the same ratio between [1] the rational expected utility of a gamble that depends on a point being chosen in that region and [2] the utility of the payout expected from the gamble if the point chosen is not in that region. Holding the utility of the payout fixed, Bayes' postulate amounts to saying that when a point is chosen haphazard from the points on a square, all regions of equal size on the square are equally good bets to the rational mind. Now that the postulate is understood explicitly as asserting the existence of a geometrical object, one might think that it has the same status as Euclid's third postulate, which says that we can describe a circle of arbitrary size. But Bayes' postulate has a hybrid character, since it also includes a claim about rational expectation. Is the value at which we *ought* to compute an expectation a matter of fact, a relation of ideas, or something else?

Hume made some remarks that seem to render Bayes' postulate intuitively or demonstratively true. For example, in the *Treatise*, Hume wrote (T 1.3.11, 129):

> The very nature and essence of chance is a negation of causes, and the leaving the mind in a perfect indifference among those events, which are suppos'd contingent. When therefore the thought is determin'd by the causes to consider the dye as falling and turning up one of its sides, the chances present all these sides as equal, and make us consider every one of them, one after another, as alike probable and possible. The imagination passes from the cause, viz. the throwing

> of the dye, to the effect, viz. the turning up one of the six sides; ... But as all these six sides are incompatible, and the dye cannot turn up above one at once, this principle directs us not to consider all of them at once as lying uppermost; which we look upon as impossible: Neither does it direct us with its entire force to any particular side; for in that case this side wou'd be consider'd as certain and inevitable; but it directs us to the whole six sides after such a manner as to divide its force equally among them. We conclude in general, that some one of them must result from the throw: We run all of them over in our minds: The determination of the thought is common to all; but no more of its force falls to the share of any one, than what is suitable to its proportion with the rest.

Hume used the same dice example again in Section VI of the *Enquiry*, where he wrote that "when the mind looks forward to discover the event… it considers [each possible outcome] as alike probable; and this is the very nature of chance, to render all occurrences… entirely equal" (E 1.6.46, 57).

Hume's claim that it is the very nature of chance to render all occurrences entirely equal suggests an argument for Bayes' postulate running along the following lines. It is in the nature of chance that all possible outcomes of a chance experiment are equal. Like the faces on Hume's die, every point on the table is a distinct possible outcome of the imagined experiment of rolling a ball haphazard on it, and the ball cannot rest on more than one point in a single roll. Hence, the imagination "divides its force" equally over all of the points so that they are all equally likely, and any two regions of equal area, being composed of an equal number of points must be equally likely to one another. Moreover, a person believes that a given outcome will occur "tho' still with hesitation and doubt, in proportion to the number of chances, which are contrary" (T 1.3.11, 127). So, as Bayes' postulate requires, if two regions have equal areas, then the value we ought to compute for a gamble that pays out if a ball comes to rest on one of them must be the same as the value we ought to compute for a gamble that pays out if a ball comes to rest on the other.

But the contemplated line of argument faces two very serious challenges. First, Hume's die is different from Bayes' table in that the number of distinct outcomes is finite in the case of the die but infinite in the case of the table. Consequently, counting elementary chances makes sense in the case of the die but not in the case of the table.[18] Second, we do not think Hume would accept the final step from the fact that a person believes in proportion to the number (or area) of the chances to the postulate that they ought to compute the value of a gamble in the same way. That step of the argument seems to run afoul of Hume's complaint that the word *ought* expresses a different relation than does the word *is*, such that it "seems altogether inconceivable" that the former may be deduced from the latter (T 3.1.1, 469-470). But Hume never said that the degrees of belief that naturally arise from the probability of chances (or causes) was *rational*. In fact, he maintained rather that the ordinary tendency to proportion belief according to the number of the chances arises "by an inexplicable contrivance of nature" (E 1.6.46, 57), and earlier, he had maintained that chances operate upon the mind and produce belief "neither by arguments deriv'd from demonstration, nor from probability" (T 1.3.11, 127). Perhaps Hume would have taken Bayes' postulate to follow from some epistemic or cognitive norm, such as norms of consistency, clarity, and evidence (for which, see Broughton 2003; Garrett 2007; and Greenberg 2008). But we think it is more in line with Hume's system of philosophy to suppose that Hume would have taken the tendency to proportion one's degree of belief to the number of the chances to be a description of what people customarily and habitually do.

[18] We think Hume would have recognized a *problem* here, but we do not think that either Bayes or Hume would have been able to provide a satisfactory solution.

In the *Enquiry*, Hume's semblant endorsement of Bayes' postulate appeared in Section VI, only *after* Hume had outlined his skeptical doubts and his skeptical solution. But after the introduction of his skeptical philosophy, it is not safe to read Hume as saying that our ordinary inferential behaviors are rationally justified. The placement of the section on probability suggests that what Hume said in that section should be read in a skeptical way, consistent with what he said about custom and habit in Section V. But read in a skeptical way, Hume maintained only that we do, in fact, assign equal probabilities to all the possible outcomes, not that we *rationally ought* to do so. The upshot is that Hume would have accepted Bayes' postulate only in a skeptical way, as describing what people actually do, but not in a non-skeptical, normative way, as describing what they ought to do. Hence, we think Hume would have seen Bayes' argument as a formal representation of his skeptical solution, as opposed to being a non-skeptical response to a normative challenge. There is no *reason* to assign equal probability to each side of a die or to equal-sized regions of Bayes' table; we simply do so by custom and habit.

3. Hume and Bayes' Scholium

At this point, we have a geometrical theorem that has a conditional form: *If* probability is defined in terms of rational expectation and *if* one ought to treat equal-sized areas of Bayes' table as equally-probable, *then* one ought to update one's rational expectations according to the rule that Bayes derived in his Proposition 9. We have argued that Hume would have accepted the geometrical reasoning and that he would have accepted Bayes' definition of "probability" in substance, at least. However, what Hume would have said about Bayes' postulate is less clear. We have argued that Hume would have accepted

Bayes' postulate in a skeptical way but not in a way that would provide a non-skeptical solution to a normative problem. Moreover, we have suggested that there are two challenges to Bayes' argument regardless of whether it is understood in a skeptical or non-skeptical way. First, the infinite divisibility of Bayes' table would have led to a measure paradox from Hume's perspective. Second, we have seen no reason (so far) to think that Bayes' mathematical model applies to anything other than very specially arranged tables—and perhaps not even to those.

However, we think Bayes anticipated the challenges we've pointed to or something very much like them and set out to address them in his Scholium to Proposition 9. So in this section, we discuss the argument Bayes gave in his Scholium for the claim that the rule described in Proposition 9 is the correct rule to use when reasoning about arbitrary unknown events. Bayes explicitly addressed himself to the challenge of the applicability of his results, and as we will see, he also showed that the infinite divisibility of his table was in an important sense dispensable. We then examine an interesting route by which Bayes might have argued that Hume should accept his postulate in a straightforwardly non-skeptical way.

In Proposition 9 of his paper, Bayes showed how to use a record of the occasions on which an event had happened and the occasions on which it had not-happened in order to calculate the second-order probability that the first-order probability of the same event happening on the next occasion lies between two arbitrarily-chosen values. Bayes' postulate and his proof of Proposition 9 may be regarded as purely geometrical—the table and balls being mere aids to the imagination. Understood as a purely geometrical argument, there is nothing Hume would find objectionable, but whether Bayes' result applies to anything in the actual world is another matter. In the Scholium to Proposition 9, Bayes argued that "the rule given concerning the event M in prop. 9 is also the rule to be used in relation to any event concerning the probability of which nothing at all is known antecedently to any trials made or observed concerning it" (306). In other words, Bayes' argued that his table provided a good model for reasoning about unknown events, such that one ought to use the rule in Proposition 9 to find the probability that the probability of an unknown event lies within some specified bounds.

In Bayes' experiment, we imagine rolling a ball haphazard on a table and taking a line through the point where it stops perpendicular to the bottom side. The resulting line divides the table into a region M and a region ~M. We then imagine rolling the ball *n* times and recording the number of times that it comes to rest on M and the number of times that it comes to rest on ~M. In his postulate, Bayes assumed that all equal sized regions on the table were equally probable, which is to say that the total probability is spread uniformly over the surface of the table. Consequently, some authors (such as Fisher 1922, 324 and Jeffreys 1939, 34) have interpreted Bayes as assuming the Principle of Indifference, which says that if one's evidence is symmetric with respect to some set of propositions—offering no more support to any one member of the set than to any other—then one should assign the same probability to each. In the case of an unknown event, one's evidence offers no more support to the claim that the probability of the event is in some interval than it does to the claim that the probability of the event is in any other interval of equal size. Hence, the Principle of Indifference says that every equal-sized interval of possible values for the probability of an event ought to be assigned the same probability.

But Molina (1931), Hacking (1965, 198-201), Stigler (1982), and others have argued that Bayes was doing something more subtle and interesting. Specifically, in his Scholium to Proposition 9, Bayes assumed that every way *the experiment* could turn out was equally likely. But unlike the infinitely divisible surface of the table, the experiment Bayes imagined was explicitly *finite*. In n rolls, the ball might never come to rest on the right-hand side of the line. Or it might do so exactly one time or exactly two times or … or exactly n times, and each of those possible results is equally likely. In modern notation, where X is a random variable whose value is the number of times the ball comes to rest on the right-hand side of the table, Bayes assumed that $\Pr(X = x) = \frac{1}{(n+1)}$ for each of the $n + 1$ possible values x of X. Notice that we have just described the possible observable results of a finite experiment, not some collection of unobservable, infinitely-small geometric points. Bayes then argued as follows. When we say that an event is unknown, we mean that every way the experiment could turn out is equally likely before any observations are made. Hence, consistency demands that we give the same odds for each way the experiment could turn out. For Bayes, it is not just that we do assign equal probabilities to all of the sides of a die of unknown character before ever throwing it. Rather, assigning the same probability to each of the sides is just what we mean when we say that the die has an unknown character. Continuing on, Bayes argued that one may "justly reason" about any unknown event on the basis of any model which entails that every way the experiment could turn out is equally likely before any observations are made. Moreover, Bayes proved that his postulate, understood as a mathematical model, entails that every way the experiment could turn out is equally likely before any observations are made. So, one may justly reason about any unknown event on the basis of Bayes' postulate.[19]

Would Hume have accepted Bayes' account of what makes an event "unknown"? If the rest of our arguments are sound, then the adequacy of Bayes' reply to Hume hangs on this question. We don't think there is any strong evidence either way as to whether Hume would have accepted Bayes' account of what makes an event unknown. But our own opinion is that Hume would have repeated his distinction between what people in fact do and what they ought to do. In this way, Hume could agree that an unknown event is one where we *do* distribute the full measure of our belief equally over all possible outcomes while denying that it follows from what we in fact do that we *ought* to do so or that our doing so is in any way *determined by reason*.

4. Concluding Remarks

The upshot of our arguments is that Bayes failed to provide a non-skeptical solution to Hume's skeptical doubts, if those doubts are understood as normative. However, Hume would have endorsed the Bayesian proposal as a mathematically precise rendering of his skeptical solution. If one takes custom and habit to generate a commitment to Bayes' postulate, then Bayes' results show what the precise consequences have to be if one reasons correctly thereafter. If one reads Hume as treating custom and habit as justifying, then the skeptical Bayesian account shows how one can come to be justified in having very specific degrees of belief about unobserved cases on the basis of observed cases. Alternatively, if one reads Hume as treating custom and habit as unjustified, causal

[19] See Murray (1930), Molina (1931), Stigler (1982), and Earman (1990) for further discussion of the argument in Bayes' Scholium.

sources for beliefs, then the skeptical Bayesian account underwrites quantitative predictions about how the machinery works. Either way, Bayes' mathematical approach fits nicely with Hume's goal of establishing an experimental philosophy of man in the same vein as Newton's experimental philosophy of nature.[20]

[20] Compare our position to the view elaborated in a remarkable undergraduate thesis by Flores (2015).

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Appendix A: Bayes' Solution to the Inverse Problem

In this appendix, we sketch a simplified version of Bayes' geometrical reasoning. The overall structure of his argument is given in Figure 1.

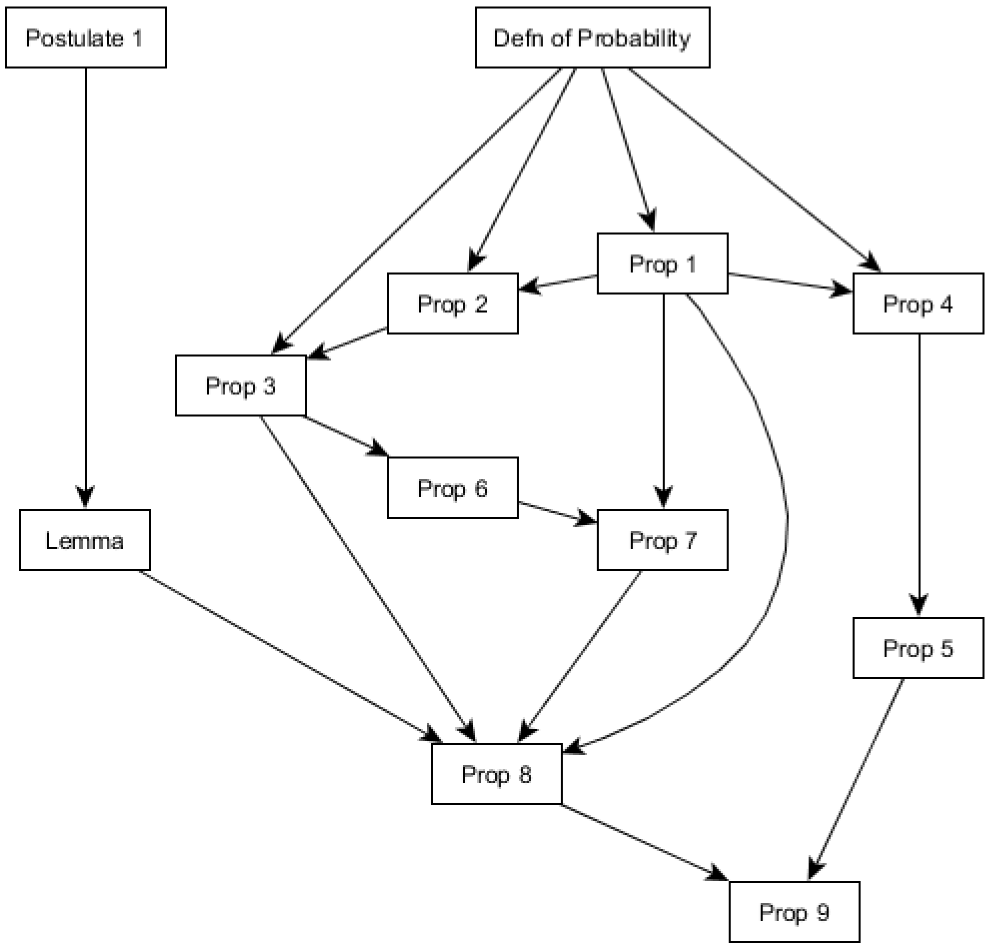

Figure 1: The Structure of Bayes' Argument

We think it is worth noticing that the overall structure of Bayes' argument supports Earman's (1990) conjecture that in Proposition 3, Bayes was arguing for the standard ratio account of conditional probability, while in Proposition 5, he was arguing for conditionalization as an update rule.

Bayes' geometrical reasoning begins by supposing we have a square table that has been constructed so that if any ball is rolled on the table, it must stop somewhere on the table's surface and the probability that it stops on any given region of the table is equal to the probability that it stops on any other region of equal area. That supposition is Postulate 1. (For simplicity, in the main text, we have used the label "Bayes' postulate" for Postulate 1. Bayes' second postulate is trivial.) Label the corners of the table A, B, C, and D. Now, suppose we roll a ball so that it stops at some point W on the table. We draw a line through W that is parallel to the side AD. Let S be the point at which the line intersects CD, and let O be the point at which the line intersects AB.[21] Say that the line OS divides the table into two regions M and ~M. (To avoid some possible confusion, let the line OS be part of region M.) The construction so far is pictured in Figure 2.

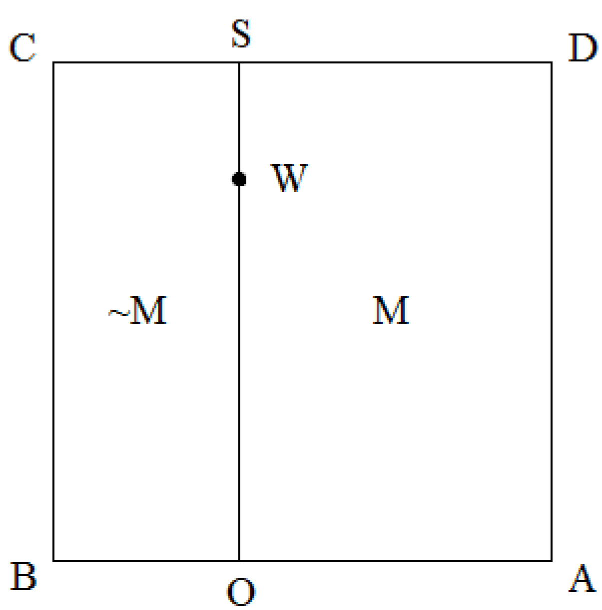

Figure 2: Bayes' Table Divided into Regions M and ~M

[21] Bayes used a lowercase "o," but we will use uppercase letters throughout. We also use a standard font, rather than italics, for geometric points in our versions of Bayes' diagrams.

Let F and G be arbitrary points on the line AB. Bayes proved the following as a lemma: the probability that the point O is between the arbitrary points F and G on the line AB is equal to the ratio of the length of FG to the length of AB. Bayes then argued that if we picked two points F and G on the side AB and set up lines as in Figure 3, the probability that the point O falls between F and G should be equal to the length of the interval FG relative to the length of the side AB.

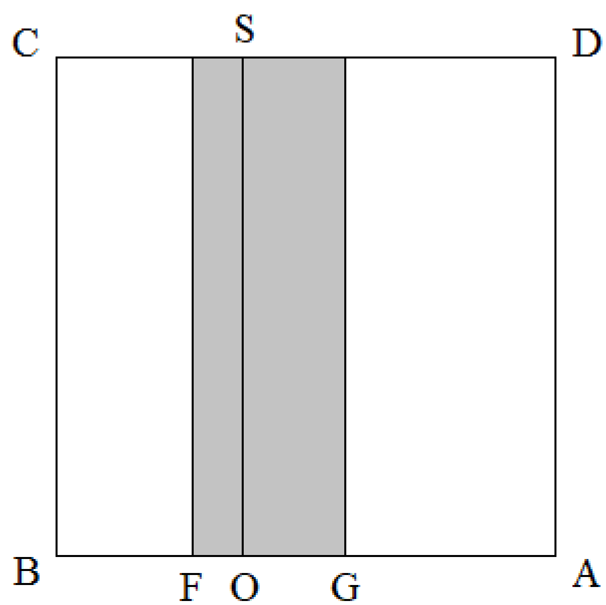

Figure 3: Bayes' Table with Interval FG

Equivalently, the probability that the ball rests in the shaded region of Figure 2 is equal to the ratio of the area of the shaded region to the area of the table. By Postulate 1, the probability that the point O lies in any interval of length equal to FG is always the same. Hence, the probability must simply be measured by the length of the interval FG relative to the length of the side AB. (Bayes' proof at this point was elaborate and careful, since he needed to be sure that the argument works regardless of whether the relevant intervals—BF, FG, and GA—are all commensurable.) Bayes observed that it follows immediately from his lemma (identifying F with O and G with A) that if we roll the ball and make the construction as in Figure 3, then the probability of event M occurring in a single trial—which is to say, the probability that the ball next comes to rest in the region M—is equal to the ratio of the length of the interval AO to the length of the side AB.

Bayes asked his readers to "erect" a curve (or figure) on the side AB. We have labeled the curve BHJKA. We imagine that the side AB is divided into two parts at the point G, and we construct a line, perpendicular to AB, from G to a point K. Let *y* denote the ratio of the length of the interval GK to the length of the side AB. Let *x* denote the ratio of the length of AG to the length of AB. And let *r* denote the ratio of the length of GB to the length of AB. Then the length of the interval GK is chosen so that $y = \binom{p+q}{p} x^p r^q$, with *p* and *q* to be given.

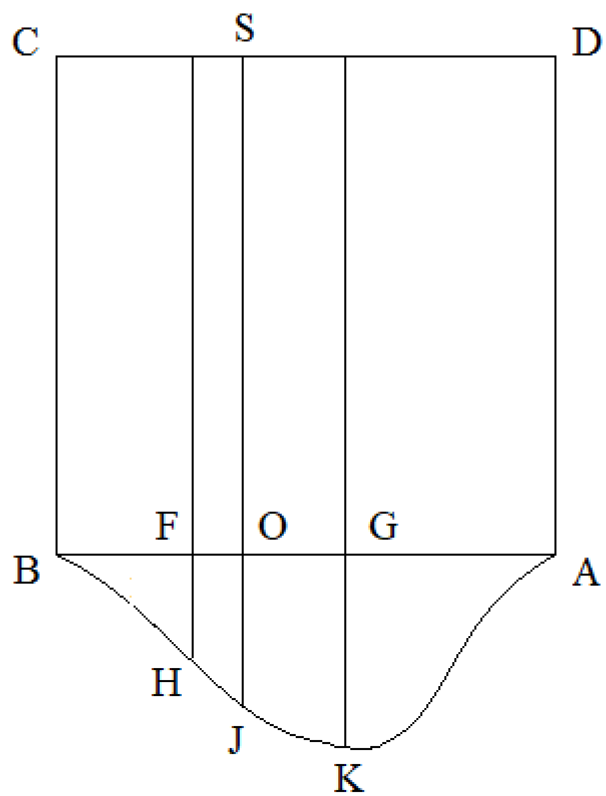

Figure 4: Bayes' Table with Curve Erected

Bayes argued (Proposition 8) that for arbitrary points F and G on the side AB, the probability that O falls between them, that event M happens *p* times, and that event ~M happens *q* times in *p* + *q* times that the ball is rolled on the table is the ratio of [1] the area between the constructed curve BHJKA and the side AB from F to G and [2] the whole area of the square on AB.

The argument is a proof by contradiction. Suppose that the probability is not the given ratio. Consider two cases. First, suppose the probability is a ratio of a figure *larger* than the given one. Bayes found that this supposition leads to a contradiction—one of the constructed lines must be the longest erected and also not the longest erected. Second, suppose the probability is a ratio of a figure *smaller* than the given one. Bayes found that this supposition leads to a similar contradiction. Hence, the probability is the ratio given in Proposition 8.

Now, suppose we roll the ball without knowing where on the table it stops (which place we mark as O). That defines the event M, as before. We proceed to roll the ball on the table *n* times without looking at where it stops. We suppose that a friend observes the table as we do this and records how often the ball stops on region M and how often it stops on region ~M. Our friend tells us that the event M happened *p* times and the event ~M happened *q* times. Bayes imagined guessing that the point O is somewhere between the points F and G. He noted that such a guess is equivalent to guessing that the probability of M occurring in a single trial is between the ratio of AG to AB and the ratio of AF to AB. He then gave a short argument based on Propositions 5 and 8 for the following claim (Proposition 9): The probability that the guess is right is the ratio of the part of the figure BHJKA from F to G to the whole area of the figure.