<u>Response to criticisms by Andrew Briggs, Andrew Steane, and Hans Halvorson's *It keeps me seeking*.</u>

On pp. 153-159 of their challenging book these authors cast considerable doubt on the possibility or desirability of any probabilistic argument for the existence of God, and in particular of my probabilistic argument contained in my book *The Existence of God* which I illustrate with the aid of Bayes's theorem. Bayes's theorem analyses the posterior probability of a hypothesis h on evidence of observation e and background evidence k.

$$P(h|e\&k) = P(e|h\&k)\ P(h|k)/P(e|k).$$

I respond to seven separate (explicit or implicit) objections by these authors to my enterprise.

1. They object (pp.154-5) that "when scientists see a formula, they instinctively want to put values in… We simply have no idea what values to attribute to each of these [quantities]", when h is the hypothesis that God exists and the e's are the different pieces of evidence for (or against) his existence. My response is that our understanding of some hypothesis being very probable, probable, not very probable, or very improbable, is much more fundamental than the use of Bayes's theorem to give precise numerical values to the relevant terms of the equation. We judge scientific theories, as well as theories of history or detective work, to be probable, very probable or whatever, even if we cannot ascribe numerical values to their probability. I devoted most of chapter 3 of my book to stating in qualitative terms what I claim are the criteria for making such judgements (both of personal explanation, that is hypotheses which purport to explain the evidence as brought about by an intentional agent; and also of scientific explanation, that is hypotheses which purport to explain the evidence in terms of the operation of some natural law on a preceding cause). I claimed that a hypothesis (of either kind) is probable insofar as it makes probable the observable evidence when otherwise that is not probable, in so far as it fits with

background evidence, is simple, and has small scope (that is, in so far as its claims are less precise and concern only a narrow area of the world). We can put this into Bayesian form by interpreting the probability of the evidence given the hypothesis as P(e|h&k), the probability of the evidence whether or not the hypothesis is true as P(e|k), and use P(h|k) as a measure of the probability conferred on the hypothesis by its fit with background evidence, its simplicity, and (smallness of) scope. Then we can use Bayes's theorem to assess the probability of the hypothesis on the evidence P(h|e&k). But only in so far as we can give precise values to the satisfaction of the different criteria, can we give a precise value to the probability of the hypothesis on the evidence. We can do that, only given narrow assumptions – for example that there are only a few possible hypotheses, which fit equally well with background knowledge, are equally simple and have equal scope; and that the evidence consists of some outcome of an experiment, which is one of a finite number of possible outcomes of the experiment which are each equally probable, given only the background evidence. So what does it mean to use Bayes's theorem to assess the probability of a hypothesis when no such narrow constraints apply, and we can ascribe no exact numerical values to its terms? Here is my answer from my book (p.68):

> I have claimed that Bayes's theorem is true, but I had better make clear what I mean by saying this. I mean that in so far as for various *e*, *h*, and *k*, the probabilities occurring in it can be given a numerical value, it correctly states the numerical relationships which hold between them. In so far as they cannot be given precise numerical values, my claim that Bayes's theorem is true is simply the claim that all statements of comparative probability which are entailed by the theorem are true. By statements of comparative probability I mean statements about one probability being greater than, or equal to, or less than another probability. … Thus it follows from Bayes's theorem that if there are two hypotheses h_1 and h_2 such that $P(e \mid h_1\&k) = P(e \mid h_2\&k)$, then $P(h_1 \mid e\&k) > P(h_2 \mid e\&k)$ if and only if $P(h_1 \mid k) > P(h_2 \mid k)$.

And, I should add, $P(h_1 \mid e\&k) >> P(h_2 \mid e\&k)$ if and only if $P(h_1 \mid k) >> P(h_2 \mid k)$. So in order to discover the overall probability of a hypothesis h evidence e, we need to know the values of the other terms in Bayes's theorem P(e|h&k), P(h|k), P(e|k).We discover what are the criteria which determine the values of these different terms by reflecting on innumerable thought (and actual) experiments describing evidence relevant to assessing the probability of different hypotheses in which it is obvious (= almost everyone would agree) that a particular hypothesis is more probable, or much more probable, or just as probable as, some other hypothesis; and by then extrapolating from these many experiments the criteria at work in determining what makes each hypothesis probable (or whatever). The initial crucial strategy is to take a group of examples in which two of the terms—P(e|h&k), P(h|k), and P(e|k)—obviously have the same value, but the posterior probability of the hypothesis P(h|e&k) is obviously different; it then follows the difference arises from the remaining term in Bayes's theorem. And then, again by considering many thought experiments, we can see what are the features of that remaining term that make all the difference to the probability of the resulting hypothesis. Thus consider a thought experiment in which the rival (mutually incompatible) hypotheses h1, h2, etc., concern the path along which some planet moves, put forward at a time when there was no general theory of gravitation; and so the background evidence does not give preference to any particular theory about how the planet would move—the only relevant evidence e is its previously observed positions. In the thought experiment each of these hypotheses entails the observed evidence within a certain particular degree of accuracy. Each of these hypotheses makes predictions about exactly the same area (the future positions of the planet) and claims to predict them equally precisely (within the same limits of accuracy). Then since P(e|k) remains the same, whichever h we are considering, and since for each of these hypotheses P(e|h&k) has the same value, if some particular one of these hypotheses h1 is more probable than any other hypothesis, that must because P(h1|k) is greater than P(h2|k), P(h3|k) etc; and since all the hypotheses have the same scope and fit equally well with the background evidence, that higher value must arise because of the greater simplicity of h1. And then, reflecting on which of these hypotheses is obviously (= almost everyone would agree) more probable on the evidence, we can see which aspects of a hypothesis make it simpler. And so more generally for each of the other factors involved in determining the probability of a hypothesis, we hold other factors constant and see how varying the nature of our chosen factor affects our obvious judgements about the overall probability of the hypothesis. And we are able to judge not merely whether varying the nature of the chosen factor affects the overall posterior probability, but whether changing it makes a great difference or only a small difference to the posterior probability of the hypothesis—I emphasise again that some probability can be greater than another probability without there being a number by which each of these probabilities can be measured. Then we can proceed to the more difficult task of varying two of the factors—P(e|h&k), P(h|k), P(e|k)—and seeing what difference that makes to judgements of the posterior probability of the resulting hypothesis. I began to spell all this out in more detail in *The Existence of God*, especially in chapter 3 but also at other places in the book (and somewhat more fully in my book *Epistemic Justification* chapter 4).

2. Briggs, Steane, and Halvorson write (p.155): "the issue in scientific work is first and foremost accuracy, rather than simplicity". Of course scientific theories have to be such as to entail the observed data fairly accurately—nowhere in my book did I deny this. But for a finite collection of data there will always be an infinite number of mutually incompatible hypotheses of the same scope which satisfy that requirement. Unless we could choose between them on the basis that some of them fit better with the background evidence than do others, or that some of them are simpler than others we could not choose between them. In the case of the planetary hypotheses which I considered above, our background evidence is the evidence which makes probable some theory of gravitation (or perhaps eventually a "theory of everything") which makes it more probable that the path of the planet will have a certain shape rather than a different shape. The

criteria which makes that theory of gravitation (or "everything") probable are just the same as the criteria which make narrower theory of planetary motion probable – except in the respect that for it there is no background evidence for it; we do not judge between theories of gravitation (or "everything") on the basis that they fit yet wider theories better, because there are no wider theories. But there can still be an infinite number of mutually incompatible such widest-of-all-theories of equal scope which entail all observed data with equal accuracy. Unless some of those theories were more probable than others on the basis of their simplicity, we would have no grounds for believing any background theory, and in consequence no grounds for believing any scientific theory at all. For any prediction about the future, there will always be some hypothesis which entails observed observations with total accuracy and also entails that prediction. If we could not use the criterion of simplicity to discriminate between hypotheses, no prediction about the future would be any more probable any other prediction; science would be totally useless. Simplicity is just as important as accuracy—both are vital for science

3. Briggs, Steane, and Halvorson then go on to add that "there can also be differences of opinion on what is the more simple or elegant or cogent set of ideas", and they give examples. This looks as if it is a criticism of my view; but I agree with them—nowhere in my book did I deny this. Where there is significant disagreement about which hypothesis is the simpler of several hypotheses clearly we cannot use simplicity as a criterion for judging between them; and so we must in effect regard them as equally simple. But my point was that while there may be a small finite number of hypotheses which satisfy the other criteria equally well and which for this purpose we must judge to be equally simple, it remains the case that—as I argued above—there are always an infinite number of hypotheses which also (in the absence of relevant background evidence) make predictions just as accurate as the former ones but are manifestly less simple. I illustrate this point by a trivial example—suppose

(in the absence of any relevant background evidence) we are seeking a hypothesis h which explains the connection between two variables x and y, and our evidence e is that the observed values of x and y are (1,1),(2,2),(3,3),(4,4),(5,5), and (6,6), every hypothesis of the form $x = y + \phi(x-1)(x-2)(x-3)(x-4)(x-5)(x-6)$, where '$\phi$' is a constant, will entail the observed evidence with equal accuracy. But clearly on that evidence the hypothesis with $\phi = 0$, '$x = y$' is more probable not merely then any of the hypotheses resulting from giving particular values (other than 0) to ϕ, but more probable than their disjunction. And it won't remove the need for a criterion of simplicity by making a few more observations, because there will still remain an infinite number of hypotheses equally well able to predict those observations, as well as entail the previous observations.

So any serious investigation into the criteria determining the probability of some causal explanation must extrapolate from our agreed judgements of the probability of different hypotheses on different kinds of evidence, the criteria for when a hypothesis is simple. If we can do that, then we can apply these criteria to disputed cases of 'which is the more simple' hypothesis. That is what I have tried to do in *The existence of God* and elsewhere. I argued in that book (p. 53) that "the simplicity of a scientific theory is a matter of it postulating few entities, few properties of entities, few kinds of entities, few kinds of properties, properties more readily observable, few separate laws with few terms relating few variables, the simplest formulation of the law being mathematically simple". And (p. 54) "One formulation of a law is mathematically simpler than another insofar as the latter uses terms defined by terms used in the former but not vice versa". And I showed there how this formulation which looks more suited to scientific explanation functions also to assess the probability of personal explanation.

4. Briggs, Steane, and Halvorson write (pp. 156-7): “different people have vastly different opinions about whether an appeal to God can correctly be called simplifying.” But a central theme of my book was to show which of these vastly different opinions is correct. To do this I analysed the criteria which we use for other hypotheses, extrapolated from our agreed judgements about whether one hypothesis is simpler than another (in the way described above). I then claimed that by these criteria the hypothesis that there is a God is a very simple hypothesis. But Briggs, Steane, and Halvorson do not attempt to consider whether my account of the criteria of simplicity is correct, let alone to consider whether those criteria yield the result which I claim.
5. Briggs, Steane, and Halvorson then cast doubt on the worthwhileness of my whole enterprise, when they write (p. 157) “to talk of God as a hypothesis with the probability of being true reveals a woefully incomplete appreciation of who God is.” The hypothesis which I discuss is not God himself, but the hypothesis that God (defined as having traditional properties ascribed to him) exists. And what they write casts doubt not merely on the worth of my particular account of arguments for the existence of God, but on all such accounts. The production of such arguments has been part of the Christian tradition (and to a lesser extent of Jewish and Islamic traditions) for the past 2000 years. All that I have done is to articulate the arguments of the past in a probabilistic framework. The Existence of God was concerned solely with the force of arguments for the existence of God; it did not discuss their relevance—that is why I followed *The Existence of God* with *Faith and Reason* which is a book devoted entirely to the issue of the relevance of rational considerations to the practice of religion. To put its conclusion succinctly: the relevance of such arguments to the practice of religion is that the more probable it is that God exists and so the more probable it is that the Christian revelation is true, the more probable it is that the goodness of a human life consists in conformity to the will of God; and so the stronger reason we have to try to live in a God-orientated way. But if it turned

out that was immensely improbable that God exists, it would follow that it would be almost certainly a waste of our lives to devote much time to prayer and worship, when that time could be spent in more useful ways.

6. But, Briggs, Steane, and Halvorson imply (p157), God is “utterly different from our categories of discourse about everyday objects and concepts” and so “we must watch our language”. God couldn’t be “utterly different” from mundane things, otherwise we could not talk as they do about the desirability of having a “relation to” him, since we wouldn’t have the slightest understanding of what they are talking about. But clearly God is very different from most mundane things, and that is why I wrote *The Coherence of Theism* to analyse, in so far as it can be analysed, what it is to claim that God exists, before I wrote The Existence of God to discuss arguments for his existence.
7. And finally they comment (p. 158) that “the person who already knows God may rather quickly lose interest in discussing God’s existence”. I rather doubt whether most religious “believers” can rightly be said to “know” God. But even if they themselves do know God, they will surely want to tell other people that God exists; and in the 21st century they are likely to have more success if they can back up their personal testimony to their awareness of God by arguments from public evidence that try to use the criteria of contemporary science (history, and detective work) which so many of our contemporaries revere so highly. And I have not ignored the importance of that testimony. For while most of *The Existence of God* was devoted to assessing the probability of God’s existence on the basis of the most general features of the universe, I went on to claim (p. 341) that unless that probability was very low, “the evidence of religious experience is in that case sufficient to make theism overall probable”.