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GOD AND PROBABILITY[1]

I. INTRODUCTION

My object in this paper is to consider what relevance, if any, current analyses of probability have to problems of religious belief. There is no doubt that words such as 'probable' are used in this context; what is doubtful is that this use can be analysed as other major uses of such words can. I shall conclude that this use cannot be so analysed and hence, given the preponderance of the other uses that can, that it is misleading.

I have three broad uses in mind, of each of which one of three widely accepted analyses is *prima facie* plausible. The first is what looks like a *statistical* use of 'probable' and related terms, exemplified in F. R. Tennant's argument from design.[2] The second is what looks like an application of *subjective* probability in talking of the lack of firmness or conviction in a person's religious belief or unbelief. The third is what looks like an application of *inductive* probability, in that some features of the world are taken to support a theistic cosmological hypothesis (or at least a cosmological hypothesis consistent with theism). My conclusion in each case will be, *not* that a state of the world, or belief, or hypothesis fails to be probable because it is improbable, but that it is misleading to speak of them in terms of probability at all.[3]

II. STATISTICAL PROBABILITY AND DESIGN

Tennant, in his weakened version of the argument from design relies upon the application of statistical probability in the following way. He argues that on any hypothesis that the world is the result of a chance process, it is extremely improbable that it should be as it is. He further argues that this is much more probable on the hypothesis that it is the result of intelligent design. He finally infers that the latter hypothesis is therefore more probably true. This last step involves an appeal to *inductive* probability, which I consider in §IV below. Of the first steps, I argue first that they would be

[1] Revised version of a paper given to the D Society at Cambridge on 26 April 1968.

[2] *Philosophical Theology*, Cambridge, 1928. Vol. 2, pp. 78 *et seq.*

[3] J. Hick (*Faith and Knowledge*, 2nd edition. London: 1967. Ch. 7) comes to the same conclusions. But Peirce's frequency analysis of statistical probability, and Keynes' 'logical relation' analysis of inductive probability, to which Hick appeals, have been too long superseded for his argument to be conclusive. His reference to Tennant's 'alogical probability' is an inadequate presentation of modern subjective analysis.

invalid even if the concept of statistical probability applied in this context. I also argue that it does not apply, and hence that the first hypothesis is incoherent and the second either incoherent or trivially true.

Tennant's general thesis is that 'the conspiration of innumerable causes to produce, by their united and reciprocal action, and to maintain, a general order of Nature [constitutes] the forcibleness of Nature's suggestion that she is the outcome of intelligent design' [*op. cit.*, p. 79], and the argument turns upon 'forcibleness' being understood in terms of probability. There is no doubt that, in more or less sophisticated forms, argument from the order we perceive in the world to at least the probability of intelligent design is still one of the most popular theist arguments.[1] It raises other conceptual problems as well, especially about the concept of order, but that raised by the use of probability is the crucial one.

First, it is worth commenting on the weakness of Tennant's conclusion. He emphasises that 'the empirically-minded theologian' [*op. cit.*, p. 78] who relies on this argument 'will . . . entertain, at the outset, no such presuppositions as that the supreme Being, to which the world may point as its principle of explanation, is infinite, perfect, immutable, supra-personal, unqualifiedly omnipotent or omniscient. The attributes to be ascribed to God will be such as empirical facts and their sufficient explanation indicate or require.' [*loc. cit.*] 'God' then, is just the name for whatever we need to postulate to explain the world's being as science finds it to be. The argument does not at all purport to show that whatever we need to postulate in this way is at all like the Christian, or any other well-known, God. What it purports to show is the following. Given that there are various possible explanations of the world's being as it is, the world's being as it is makes some of these explanations more probable than others. Then from the world's being as it is, it follows both that the most probable explanation or explanations of it postulate a uniquely fundamental, but otherwise unspecified, entity (which we shall call 'God'), and that it is more probable than not that some such explanation is true, i.e. some that such entity exists. Neither of these conclusions, weak as they are, follows from the admitted premises.

A parable. A man, *A*, takes a pack of cards and considers two possible explanations of the order (as yet unknown) of the cards in the pack: (i) that they have been arranged in that order by an intelligent designer, otherwise unspecified; (ii) that their order, whatever it is, is the result of some chance process, a product not of a designer, but of a shuffler. In order to decide, from an inspection of the pack, which of these two hypotheses is the more probably correct, *A* may proceed by one of two methods, which I call '*a priori*' and '*a posteriori*' respectively.

[1] E.g. P. Lecomte du Noüy (*Human Destiny*. New York: 1947. Ch. 3); who uses a classical *Laplacean* definition of statistical probability in terms of numbers of equiprobable cases. But the well-known objections to this definition, and to the principle of indifference on which it relies, do not sufficiently dispose of the argument, for which it is not essential.

On the *a priori* method, *A* decides, *before he inspects the pack*, in what order or orders an intelligent designer might have arranged the cards. For example, suppose the intelligent designer to be a bridge player: he might be expected to arrange the cards in any one of a number of orders that would benefit himself as dealer without arousing the suspicions of his opponents. *A* assumes therefore that any order in such a set of orders has a *higher* statistical probability on this hypothesis than on hypothesis (ii), and any order not in the set has a *lower* statistical probability, possibly zero. On the basis of these assumptions, *A* decides that *if*, on inspection, the order of the cards turns out to be a member of the set specified under hypothesis (i), he will accept that hypothesis, and otherwise he will accept hypothesis (ii). In other words, *A* adopts *a priori*, before inspecting the pack, a *decision strategy*. He recognises that the explanation his strategy tells him to adopt, on the result of his enquiring into the actual order of the cards, will only have been shown to be *probable*, since, after all, if the order is a member of the specified set, it still *might* have occurred by chance. 'All that he can expect to emerge from his inquiry is grounds for reasonable belief rather than rational and coercive demonstration.' [Tennant, *loc. cit.*]

However, the smaller the *a priori* specified set of orders which might have been the result of design, i.e. the more definite *A*'s idea of the postulated designer's intentions, the smaller the probability that an order in such a set could have arisen by chance. Suppose, for example, that the set is reduced to exclude an order whose statistical probability on hypothesis (ii) is p. Then the statistical probability on hypothesis (ii) that the actual order is a member of the new, reduced set has been reduced by p. In the extreme case, with effectively complete *a priori* knowledge of the designer's intentions, the specified set will have just one member. *A* will then be able to write down just one order, *a priori*, which the designer, if any, would produce. If, on inspection of the pack, this order is revealed, the statistical probability of this on hypothesis (ii) is so minute that *A* may reasonably accept the design hypothesis with virtually complete confidence.

There is no doubt of the basic soundness of this *a priori* method. The difficulty with it is that of knowing *a priori* what the designer's intentions are, and hence of being able to write down, before inspecting the pack, *what* order, if found, will license the probable inference that it is the product of design. *A*, being aware of this difficulty, and of past failures of *a priori* arguments to anticipate successfully the results of empirical enquiry, resolves to adopt the second, *a posteriori*, method. He resolves, that is, to ascribe to the designer no intentions other than 'such as empirical facts [i.e. the actual order of the cards as revealed by inspection] and their sufficient explanation indicate or require'. So *A* proceeds as follows. He inspects the pack, and writes down the actual order in which the cards occur. He takes 'intelligent designer' to be the name for whatever entity has just such intentions as would

lead him to arrange the cards in the precise order they are actually in. He argues that the cards being in that order shows it to be, at least, much more probable that they were arranged by an intelligent designer, so defined, than that they were arranged by an entity with any other intentions. So *A* takes this as his hypothesis (i), and has no difficulty in showing that, on it, the statistical probability of the order of the cards being what is actually found is much higher than it is on hypothesis (ii). He finally concludes, as on the *a priori* method, that this shows hypothesis (i) to be vastly more probable than hypothesis (ii). *A* notes further that this method is free of the doubtful *a priori* assumptions that have to be made in the first method, and that it is solely the empirically observed order of the cards that has 'forcibly suggested' its own design. He might moreover note that this second, *a posteriori*, method could be applied to yield this conclusion *whatever* the order of cards turned out to be. But if he did note this, he might conceivably reflect that the method is too powerful to be valid. . . .

It does not need much insight into the nature of probability to realise that this *a posteriori* method is completely worthless. Yet, so far as I can see, it reflects faithfully the structure of Tennant's argument. The kind of fallacy involved is sufficiently widespread and persuasive to deserve a name and I propose to call it the 'bridge-hand fallacy', after the most obviously fallacious instance of it. A bridge player, who suspects the dealer of fixing the pack, writes down the hand he suspects and *then* is dealt just that hand, has good confirmation of his suspicion. A bridge player, however, who writes down whatever hand he receives, and then argues that its improbability 'forcibly suggests' that it has been fixed by the dealer, will soon and rightly lack sympathetic listeners. But not so, it appears, the theologian who argues, in strict analogy, from whatever 'hand' science shows him to have been dealt, that its improbability 'forcibly suggests' that it has been fixed by some suitably defined Supreme Being. It cannot be too much emphasised that, on any theory of probability, while improbable things do happen, the inference *a posteriori* from 'X happens' to 'X is improbable', on which the bridge-hand fallacy turns, is just not a valid inference, not even a valid probable inference.

It does not follow that because I think the bridge-hand fallacy is committed in arguing *a posteriori* for the probability of intelligent design, I suppose a valid argument to exist for the world being the result of a chance process. On the contrary, this does not seem to be a readily intelligible hypothesis at all. The concept of a chance process is that of some device, such as a die, a coin, an ordered pair of parents, on which a trial can be conducted, such as throwing the die, or tossing the coin, or conceiving a child. Of such a trial a number of outcomes are possible and none is certain: e.g. throwing a five, landing heads, that the child born is male. Now it seems to me that the force of saying that an outcome of a chance process is 'possible but not

certain' rests on the observation of a number of chance processes of the same kind—throws of dice, tosses of coins, birth of children—of which sometimes there is one outcome and sometimes another. For there is a very close connection between something being probable and it happening more often than not. Now this latter concept can have no application unless it is at least possible for there to be some number, greater than 1, of occasions which the something could happen. In other words, it is essential to the concept of a chance process that it is a kind of process that *could* occur more than once, even if in fact it doesn't. Otherwise, the supposition that the outcome of a process is possible but not certain, which is implicit in calling it a 'chance' process, seems to me quite empty and unintelligible.

(In saying this, I am not subscribing to a frequency analysis of probability as it applies to chance processes.[1] I am not making the much stronger and, to my mind, fallacious claim that in such cases the probability of an outcome can be defined in terms of the frequency with which it occurs in many repetitions of the process. I am merely saying that the *possibility* of repetition is necessary to there being any probability of any outcome.)

Now the trouble with supposing the world to be the result of a chance process is that, not merely *has* the process only happened once, it *could* only happen once. The world comprising all there is, it does not make sense to suppose two worlds, which might be qualitatively the same or different as the two chance processes of which they are the outcomes had the same or different outcomes. We may, of course, talk of other possible worlds, but this is only a way of referring to the fact that there is no logical necessity in the world being as it is, that science and common observation cannot spin their results out of logical reflection. But this does not, and cannot, mean that these other possible worlds are lying around in a limbo of potentialities, waiting to be realised if some Supreme Dealer should decide to pick up the pack and deal again.

It should be observed that all this is compatible with every occurrence in the world being the outcome of a chance process, i.e. with every scientific law being statistical. There is no reason to suppose that this latter is true, but even if it were it would still not entail that the conjunction of all such occurrences, past, present and future, was itself a possible but not certain outcome of a chance process.

All this is merely to say that the concept of probability cannot be applied in connection with arguments from design as it is applied by statisticians to the outcome of chance processes. And in so far as the 'intelligent design' hypothesis is defined negatively, i.e. as a hypothesis that the world is *not* the result of some chance process, it either shares the incoherence of the

[1] The frequency analysis is that most widely accepted by statisticians. The most influential exposition of it is probably in R. von Mises: *Probability, Statistics and Truth*, 2nd English edition (London: 1957). My own view of statistical probability is stated in 'Chance', *Arist. Soc. Suppl. Vol.* 43 (1969), pp. 11–36.

'chance' hypothesis, or becomes trivially true. If the design hypothesis is that the probability of the world being as it is, considered as the outcome of a chance process, has been raised to 1 (or close to 1, on a sort of 'semi-design' view) by the Supreme Being, then it shares the incoherence of any view that regards the world as an outcome of a chance process. If, on the other hand, the design view is merely that the world is *not* an outcome of a chance process, with some probability, however high, then it is trivially true, since it is logically incorrect to call any process of which the world might be the result a 'chance process'.

III. SUBJECTIVE PROBABILITY

Having tried to show that statistical probability has no proper application in arguments from design, I now consider other *prima facie* applicable uses of probability statements. There is the use of probability statements dealt with in the theory of subjective probability[1] in which a probability statement merely expresses the odds at which a person making the statement would be prepared to bet on whatever he attaches probability to. The point of this theory is to provide a quantitative measure of degree of belief, to make sense, for example, of saying that a man's degree of belief in something is 0.7, by showing that he would adopt the corresponding betting rate. The theory applies equally to degree of belief on any subject matter, and it does not concern itself with whether the degree of belief is in any way justified. So two people, with exactly the same evidence, contemplating exactly the same possibility, can attach wildly discrepant degrees of belief to it, in that they are prepared to bet at wildly different rates on it. This is why the theory is called 'subjective' and why, even if it could be taken as a measure of a degree of religious belief, it would not bear at all on the question of whether such a degree of belief was justified. But some comments are called for on this theory, even as providing a measure of actual, as opposed to justified, degrees of belief.

First, the theory is a subjective theory of *probability*, because it has been shown that, under the conditions imposed on the person betting, the rates he adopts must satisfy the usual mathematical axioms for probabilities if his opponent is not to be able to make money off him whatever happens. So this measure of degree of belief does indeed interpret it as a probability; when a man expresses such a degree of belief by saying that he thinks something 'almost certain', 'extremely likely', 'highly probable', this is interpreted as expressing a subjective assignment of a high numerical probability, say between 0.9 and 1. This is no doubt a point in favour of the theory: until it was shown that reasonable constraints made betting rates satisfy the proba-

[1] As expounded, e.g. in L. J. Savage: *The Foundations of Statistics* (New York: 1954).

bility axioms, betting rates could hardly be taken seriously as offering an analysis of subjective judgements of probability.

However, other points need making against the subjective theory. It doesn't make sense to talk of betting unless the situation is one in which the bet could be settled, i.e. one in which the person betting will eventually come to *know* whether what he is betting on is so or not. Now in the case of religious belief, it rather depends on what the belief is whether this condition can be satisfied or not. To take the simplest example, suppose I am betting on there being an afterlife, it being given that I cannot know in advance whether there is one or not. Then, *prima facie*, the bet can only be settled in favour of the afterlife hypothesis, since if it is false there will be no settlement. Under these circumstances, on the betting rate analysis, the only reasonable probability to assign is 1, which appears to entail that I should be irrational if I acted otherwise than as if I were convinced that there is an afterlife. (Note that this has nothing directly to do with Pascal's wager, since it is quite independent of whether the afterlife is pleasant or unpleasant.)

However, this argument loses its plausibility on further inspection, as it becomes steadily less clear in what sort of currency a bet could be made now that is to be settled after death. In other words, it is not at all clear that there is any scale of utilities, in the sense required by the subjective theory, on which what is valued in this life is comparable with what may be valued after it. Alternative states of this life may be compared in utility with each other, and so perhaps may alternative states of the next life; but without a religious hypothesis to correlate states and their values in this and the next life, one cannot be sensibly compared with the other. Faust, who held such a hypothesis, could make these comparisons, but when it is the measure of belief in religious hypotheses themselves that is in question, betting rates on their truth cannot be based on them. It is thus not clear that an analysis of degree of such *religious* belief in terms of betting rates is of any value, since the postulated betting situation, taken to include the final 'pay-off' when the outcome is known, is one in which nothing of determinate value, that could be staked and won or lost, can be assumed to be preserved.

This seems to me the crucial objection to using subjective probability to provide a measure of *religious* belief in particular. But there are also more general objections to its use, which are worth mentioning. The conditions imposed by the theory on the betting situation, in order to ensure that the betting rate satisfies the mathematical axioms of probability, detract seriously from its claim to be a measure of *actual* degrees of belief. Notably, the person is supposed to be *compelled* to bet, and a bet, of course, must be at some definite rate. But a man may merely think something *probable* while having no views as to whether it is *very* probable or only *fairly* probable. Such a man will certainly refuse to bet at any rate corresponding to a probability less than $\frac{1}{2}$, but it does not follow, and is not true, that there is any one

higher rate he would wish to bet at in preference to any other. Yet if he is compelled to bet, he will have to pick some such rate, and some subjective theorists have talked as if this gambling machinery thereby exposes a precise degree of belief, of which its owner was previously unaware. Now while it may be possible to be unaware of some of one's beliefs, or of their strength, this inference is absurd. The obvious conclusion is that, in such a case, the compelled choice of betting rate is purely arbitrary, and has nothing to do with a strength of conviction in whatever the bet is about.

It is important to resist the specious air of scientific precision which the subjective theory carries in such cases, and to insist that sometimes even when one thinks something probable and a bet on it could be settled, still it would be irrational to bet at all. But if so, the claims of the subjective theory, that it is a universally applicable measure of partial belief and hence that partial belief is always measurable, become very suspect. Where there are no quantitative data, it is a mistake, to which scientists are sometimes prone, to suppose that they can be conjured up by forcing people to pick some number off a scale. And I am inclined to think that, in the case of religious belief, there simply are no quantitative data.

I conclude then, for the two principal reasons I have given, that subjective probability has no more application in the context of religious belief than does statistical probability.

IV. INDUCTIVE PROBABILITY

I turn finally to the use of probability statements that is dealt with in inductive logic, in talking of scientific hypotheses being supported by inconclusive evidence. One might say, for example, that on the basis of such evidence one hypothesis is more probable than another, or that some extra piece of evidence has made such a hypothesis more or less probable than it was before. *Prima facie*, this is the use of probability statements that most plausibly applies to religious belief. It is not, of course, necessary to suppose that religious belief just is belief in some hypothesis which could be said to be probable or improbable on some evidence, merely that this is a component of religious belief and that, where one talks of religious belief as being probable, it is to this component one refers.

The use of the concept of probability in inductive logic is both technical and controversial; in picking out what I take to be the salient points I fear I shall oversimplify in some relevant respect. However, it seems clear that no existing quantitative inductive logic is adequate to the analysis of the support given to religious hypotheses by inconclusive evidence. By a 'quantitative inductive logic' I mean one assigning numerical degrees of confirmation, or corroboration, to scientific hypotheses on the basis of evidence. It should be remarked that some of these logics are probabilistic, in the sense

that they base themselves on the mathematical axioms for probabilities, and others are not.[1] I don't think this distinction is of any consequence, since either could be taken equally well as a quantitative account of our use, in these contexts, of such non-quantitative terms as 'probable', 'likely', 'almost certain', 'barely possible'. This use carries no serious commitment to particular mathematical axioms.

Those, such as Carnap, who have constructed probabilistic inductive logics, have used the same arguments in terms of betting rates that are used by subjective probability theory. Their systems are therefore open to the objections raised in §11, at least in their possible application to religious hypotheses, that it is unclear what sense could be made of settling bets on such hypotheses.

There are, however, further objections than those based on the betting rate analysis, to existing quantitative inductive logics, whether probabilistic or not. One is their very rudimentary state: none gives any significant numerical value to the degree of confirmation of any important scientific theory. Even with their languages extended sufficiently to express such theories, it is not clear that they would be adequate to express anything that religious belief could be belief in. This is perhaps only an objection of degree. I would not claim that these confirmation theories could *never* provide a quantitative measure of the extent to which evidence makes religious hypotheses more or less probable. But equally, a claim that they *will* be able to do so is, in their present primitive state, no more than a promissory note issued by a rather insubstantial authority.

A greater difficulty seems to me to concern the kind of hypothesis involved in religious belief, a difficulty closely related to that raised in §11 about the argument from design. The scientific hypotheses to which one can imagine systems of inductive logic, suitably developed, being applied are universal hypotheses, i.e. that everything of a certain kind has some property or stands in some relation to something else. This includes statistical hypotheses, that everything of a certain kind has a definite *chance* of having some property or of standing in some relation. Now there may be few or many things of the kind referred to in the hypothesis; there may even, as a matter of fact, be none at all. But these will not be logical facts, and cannot enter into the assessment of the probability of such hypotheses on whatever evidence there is for or against them. Their influence is on what evidence there is, not on the extent to which it supports the hypothesis.

Now a difficulty arises with hypotheses about the world as a whole, that seem to be the components of religious belief to which, if at all, inductive probability might be applied. The difficulty is that it is essential to such a

[1] The chief exponent of probabilistic inductive logic is R. Carnap: *Logical Foundations of Probability*, 2nd edition (Chicago: 1962). The chief opponent of it is K. R. Popper: *The Logic of Scientific Discovery* (London: 1959). See also I. Lakatos: 'Changes in the problem of inductive logic.' *The Problem of Inductive Logic*. Ed. I. Lakatos (Amsterdam: 1968).

hypothesis that it has only one instance. Certainly, a cosmological hypothesis can be put in universal form, that every world has such-and-such properties, or such-and-such a chance of having such properties. But this form is quite misleading. It is in this case a logical fact, which may enter into the assessment of the probability of the hypothesis, that there is only one world. The hypothesis in its universal form in fact makes no more than a singular statement that our world has such-and-such properties. Consequently, we cannot expect that a confirmation theory developed to deal with genuine universal hypotheses will apply at all to cosmological hypotheses. For example, Carnap's confirmation theory assesses the probability of universal hypotheses in terms of that of their next instances: that the next thing we observe of the specified kind will have the property the hypothesis ascribes to all things of that kind. This is not a procedure that makes sense when applied to cosmological hypotheses. We cannot wait to see if the next world we observe has the property our hypothesis ascribes to all worlds.

Again, it might be said that this is only a deficiency in present inductive logic, that so far it has concentrated on the universal hypotheses which are the principal objects of scientific belief. The fact, if it is a fact, that special techniques would have to be devised for assessing cosmological hypotheses in terms of probability doesn't itself show that such techniques could not be devised. At this point I return to the criticisms of §II, which are relevant here, although they ostensibly pertain to statistical rather than to inductive probability. To show this relevance, I need to make a brief digression into the relations between these various kinds of probability.

I have not talked, as Carnap does, of different *concepts* of probability, i.e. different senses of the term 'probability', because I do not think there are such different concepts. Terms like 'probability' are not ambiguous, in the sense that something could be both probable in one sense and improbable in another. A variety of things can be probable or improbable, and we may express this fact by talking of *kinds* of probability. Kinds of probability differ in the ways they are established, subjective probability by psychological enquiry, statistical probability by statistical experiment, inductive probability perhaps by logical enquiry. But each is a probability, in the same sense of 'probability', just as religious truth, scientific truth, mathematical truth is all truth in the same sense of 'truth'. We do not infer from the existence of kinds of truth, differing in the ways they are established, that there are as many concepts of truth, and the analogous inference should be resisted in the case of probability.

The point of these remarks is this. In § II I objected to the argument from design that the necessary uniqueness of the world as a whole deprived the hypothesis, that it is the outcome of a chance process, of any sense. I concluded that one could not apply statistical probability to such hypotheses. Now this is just to say that such hypotheses do not, despite

appearances, confer any probability on the world being as it is. But then the world being as it is confers no probability on such hypotheses where this latter (inductive) probability has to be inferred from the former (statistical) probability. Such an inference is made, albeit tacitly, in the argument from design; the 'two-concept' view of probability disguises it by making the ascription of inductive probability seem independent of that of statistical probability, which in this case it is not.

The basis of the required inference is the following principle.[1] Suppose there are two statistical hypotheses, *h* and *i*, and *i* assigns a higher statistical probability than *h* does to some piece of evidence *e*. Then on the evidence *e*, if we may infer anything about the inductive probabilities of *h* and *i*, it is that *i* is more probable than *h*. Whether we may infer anything about these inductive probabilities on this evidence is controversial, but it is not controversial that this, if anything, is what we may infer. At all events, this is clearly the inference made in the argument from design: the statistical probability of the world being as it is is greater on the design hypothesis than on the chance hypothesis, *therefore* the world being as it is gives the design hypothesis a greater inductive probability than the chance hypothesis. I am very willing to grant that this is a sound inference, but if, as I have argued, such hypotheses are incoherent, the premises required for it are just not available. There is no statistical probability of the world being as it is, and hence from it no inductive probability is derivable of a hypothesis purporting to prescribe such a statistical probability.

This difficulty, that any quantitative inductive logic would face, is not one that can be expected to be overcome by technical ingenuity. The problem is not, as it may be with ordinary scientific hypotheses, that probability judgments are coherently made whose rationale it is difficult to expose. On the contrary, it is that there seems to be no coherent basis for probability judgments, in terms of the concepts with which confirmation theory deals. Hence there is simply nothing for a quantitative inductive logic to account for.

V. CONCLUSION

My tentative conclusion then is, that none of the three main kinds of probability statements, which have recently been the object of philosophical study, is properly applied in matters of religious belief. It is not meaningful to say, in connection with the argument from design, that the world being as it is is statistically either probable or improbable on some cosmological hypothesis, and hence we have no reason to say that the world being as it is renders such a hypothesis inductively probable or improbable. As for subjective probability, no doubt I may express my lack of either firm belief

[1] Given, e.g., by I. Hacking (*Logic of Statistical Inference*, Cambridge: 1965, p. 55), under the name of the 'law of likelihood'.

or of firm disbelief in such terms as 'probably', 'perhaps', 'almost sure', etc., but no significant account can be given of such remarks in terms of expressing quantitative degrees of belief measured on any scale of probabilities. Consequently, the serious use of 'probable' and related terms in these three contexts should be avoided, since it misleadingly suggests the applicability of the corresponding analyses.