SHOULD PHYSICIANS BE BAYESIAN AGENTS?

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ABSTRACT. Because physicians use scientific inference for the generalizations of individual observations and the application of general knowledge to particular situations, the Bayesian probability solution to the problem of induction has been proposed and frequently utilized. Several problems with the Bayesian approach are introduced and discussed. These include: subjectivity, the favoring of a weak hypothesis, the problem of the false hypothesis, the old evidence/new theory problem and the observation that physicians are not currently Bayesians. To the complaint that the prior probability is subjective, Bayesians reply that there will be ultimate convergence, but the rebuttal to this is that there will not be uniform convergence. Secondly, since the Bayesian scheme favors a weak hypothesis, theories turn out to be a gratuitous risk. The problem with the false hypothesis comes out in the denominator of the theorem, revealing that a factor which is not a theory at all is being considered in the reasoning. On the old evidence/new theory problem old evidence cannot confirm a new theory so that the posterior probability will equal the prior probability. Finally, empiric studies have shown that current physicians are not Bayesians. But on consideration of Bayesian inference as a system of inference, it can be reasoned that physicians should be Bayesians. However, the problem of physicians' and patients' own subjectivity continue to plague this system of medical decision making.

Key words: decision analysis, Bayesian inference, inductive inference

1. INTRODUCTION

The problem of scientific inference is one which involves the physician in several senses. First, as a physician scientist, the implications of a particular observation, or a set of observations may be generalized as the basic scientist does with experimental data in the laboratory, i.e. the individual observations are grouped in a certain way so that predictions may be made in observations of the same type. Consequently, the problem of induction pertains to this sort of inference. Secondly, the physician as practitioner must apply general knowledge, however acquired, to the particular patient, so the question of what general information is pertinent arises. At the time he is engaging in this practice he will also be accumulating data which may develop into collected knowledge from which generalizations may be made. Again Hume's problem of induction appears. Notice that the physician is required to act in ways that the 'pure'

Theoretical Medicine **13:** 349–361, 1992.

scientist is not. More will be said of this later.

The purpose and plan of this essay is to briefly introduce the problem of induction showing how it leads to a consideration of scientific inference. I will next introduce the Bayesian system of scientific inference and review Bayes' theorem along with its major objection. I will then consider the system of Bayesian inference to examine some objections in more detail. Finally, I will attempt to answer the question posed in the title by considering these objections in the light of the practice of medical diagnosis.

2. THE PROBLEM OF INDUCTION

The problem of induction, which pertains both to inference from the general to the particular and from the particular to the general, (or Hume's problem) is the challenge of determining the validity of judgments about the future or about cases which are unknown. Because the two are neither reports of experience nor logical consequences of it there is no warrant for the belief that there are valid predictions on what will happen. If observed instances of particular events conform to a generalization we would like to have an inductive principle that would allow us to infer that *un*observed instances conform to this generalization. Hume showed that we have no certain justification of this principle of induction, that is, that the inference is not necessarily truth-preserving [1].

Among the attempted solutions to the problem of induction is the endeavor to utilize the probability calculus to treat the probabilities of theories as a function of our degrees of belief. This is the subjectivist or personalist account, and a methodology based upon this account is that of Bayes' theorem. This methodology was previously popular, then suffered a reversal, but has appeared in recent years to be making a comeback in medical literature.

2.1. Bayes' Theorem

Bayes' theorem is a measure of the conditional probability of the hypothesis (H) on the evidence (E) and may be written as:

$$p(H/E) = \frac{P(E/H) \cdot p(H)}{\sum_{i=1}^{n} p(E/Hi)p(Hi) \cdot p\,(E)}$$

This says that the posterior probability of the hypothesis on the evidence [p(H/E)] is equal to the product of the probability of the evidence given the hypothesis [p(E/H)] and the prior probability [p(H)] of the hypothesis, divided by the probability of the evidence [p(E)]. The theorem of total evidence means that the denominator may be rewritten so that:

$$(p(E/H) \cdot p(H) + (p(E)/\text{-}H \cdot p(\text{-}H)$$

The Bayesian claim is that if we follow the strictures of Bayes' theorem, always conditioning our posterior probability judgments on the available evidence, then all rational investigators will eventually converge to the same posterior probabilities.

The principal criticism of the Bayesian proposal has been that it takes a subjective approach to what is relevant about the scientific appraisal of theories [1]. Any probability you want may be used as the prior probability of the hypothesis [p(H)]. It may be plausible or wild and hair-brained. But Bayesians have no problem with the notion of subjectivity because they assert that, given time and enough evidence, the probability will converge to the true value. Yet, we could object, convergence does not mean uniform convergence at any given point in time so there is always some prior probability which will not have converged at any given time. We should consider in more detail these reported problems with Bayesian methodology and discuss how a Bayesian might respond to each criticism.

2.2. *Subjectivity*

The most persistent criticism against Bayesianism is that it begins with merely subjective prior probabilities which may have any distribution [1]. This could mean that any hair-brained prior probability is all that is needed to start the inference. Salmon [1] attempted to deal with this problem by requiring that the prior probability of the hypothesis be a plausible hypothesis of the type which has been shown in the past to be successful. But the mystery unsolved by this approach is to figure out how are we to know that the hypothesis will be successful in the future other than its success in the past. This is our old problem of induction, again. Secondly, Salmon utilizes the concept of type of hypotheses for plausibility considerations. Unfortunately this is really a composite hypothesis. The problem with a composite hypothesis is that there is no way to determine the likelihood of the evidence on the hypothesis without knowing the likelihoods of the simple hypotheses which comprise the more complex entity. So we are left with the predicament of the subjectivity of the prior probability which would seem to condone virtually any starting point for the inference process.

The Bayesian replies that there will be convergence after a time so that two persons will ultimately assign the same prior probability [2]. That is, as their data gather experience the probabilities will grow asymptotically [2]. Experience is allowed to dominate prior beliefs in a controlled way. The Bayesian would want to argue that the method is unimpeachably objective, because it is a theory of inference, though its subject matter, degrees of belief, is subjective [2]. This is a valid response but, of course, the problem with this reply is that we would want to determine how much data it takes to achieve this convergence, that is, how do we know when convergence to the truth is coming about. Furthermore there is obviously non-uniform convergence, meaning that there will always be some prior probability which, at any point in time of collection of data, has not yet converged. Some have attempted to cover this eventuality by corrective terms (Salmon's "normalizing conditions" [1]) but it is doubtful that this has been successful. To the Bayesian, experience is expected to eventually dominate prior beliefs in a controlled way. Disagreement is eradicated gradually. The tendency of experience to reduce disagreement is thus brought out as the defence against the charge of "idiosyncratic subjectivism" [2]. Yet it would appear that the Bayesian is again relying upon induction to predict that the future will be like the past in the sense that, since convergence has been observed in the past, it will be so in the future. If experience demonstrates convergence then Bayesian learning has been operative, but it hardly seems best to consider this sort of uniformity of nature prior to the application of the system of inference.

2.3. Prejudice in Favour of Weak Hypotheses

If one is seeking a hypothesis with a high prior probability then a hypothesis which is a disjunction of simple hypotheses may be expanded ad hoc until the probability is virtually one. Such a de-Ockhamized hypothesis may have any prior probability you want, but, in addition to the previously mentioned problem of being unable to determine the discreet likelihood of such a hypothesis, such a high prior probability of a hypothesis would make it difficult to relatively increase the posterior probability. The theory will be virtually non-falsiable. But if truth is what you are seeking, presumably you want a hypothesis which you think is highly probable, so a high prior probability is desirable. Still a hypothesis with a high prior probability is a weak hypothesis, whereas it would seem that what you would rather want are hypotheses with high content (high p(E/H)), a high probability that one would get the evidence on the hypothesis. At the same time you should want a low prior probability, for such a bold hypothesis would be more testable or falsifiable.

Glymour [3] makes this point by focusing on the fact that a theory is never any better established than is the collection of its observational consequences, i.e. the probability of a theory is never higher than the probability of the actual or probable observations. If one takes this tack the consequence is that theories are a gratuitous risk [3]. We have no real use for theories so we should not even embrace them. Salvaging theories by stating that they are explanatory, whereas observational consequences are not, cuts at the heart of Bayesianism. The way things are set up for the Bayesian is that degrees of belief are probabilities. But if we have no warrant for belief beyond the observational consequences of a theory then, how are we to grasp the notion that theories explain? It would seem that we would want explanatory power to go hand-in-hand with warrant for belief, but we have warrant for belief only of evidence, but for a hypothesis only insofar as it establishes the observational consequences. A tautology presumably would have a high warrant for belief, but would have no explanatory power. The subjective Bayesian has begun the inference with the notion that the a priori assessment of the prior probability is a degree of belief. But only observational consequences warrant belief, increasing the likelihood, leaving theory out in the cold. The Bayesian cannot now state that the theory has a feature different from warrant for belief.

2.4. The Problem With the False Hypothesis

The denominator of the Bayes' theorem may also be written as:

$$P(E/T) \times P(T) + P(E/{-T}) \times P({-T})$$

where E = evidence and T = theory.

The problem is that the last term, –T, is not a scientific theory at all, but a composite of theories. Given the evidence of the tides that Newton's theory is true, what is the evidence given –T. –T could be Aristotle's theory, the theory of relativity, or any other theory.[1] The likelihood cannot be determined because –T is a composite hypothesis and is incapable of being discreetly identified.

Glymour makes this point,

> At about the time T is introduced, there will be a number of alternative competing theories available; call them T1, T2, ...Tk, and suppose that they are mutually exclusive of T and of each other. Then P(e) is equal to
>
> $$P(T1)P(e,T1)+P(T2)P(e,T2)+ \ldots$$
> $$\ldots +P(Tk)P(e,Tk)+P(-(T1 \ldots \vee Tk)P((e,T1 \vee \ldots \vee Tk)$$
>
> and we may try to use this formula to evaluate the counterfactual degree of belief in e [3].

But any way you try to determine the last term [P(–(T1...etc.)] will lead to incoherence. If you ignore the last term (the negation of the competing theories) then the likelihood or prior probability of T is zero, because if you ignore the negations of the theories which compete with T (one of which will be T) then you have to have either the prior probability of T or its likelihood be zero.

Perhaps the last term could just be replaced by T but, if the degree of belief in $P(T1 \vee T2 \vee \ldots \vee Tk \vee T)$ is not unity then the set of prior degrees of belief will be incoherent. So when you have all the theories together in a disjunction, the degree of belief must be unity (mutually exclusive and exhaustive) the probability must be 1. If however T does not actually equal all of –T then the resulting Bayesian inference will not work. Or in other words, the Bayesian must restart the inference process every time experience is gained, redistributing the a priori probability over a partition of the theory space that contains the new theory as a member. If the true theory is not included in the initial partition over which the prior probabilities are distributed, Bayesian inference will not converge no matter how much data is collected.

A Bayesian would respond that they are not advocating a source of rules for computing all the probabilities in Bayes' theorem. People are supposed to merely characterize their beliefs subject to the sole constraint of consistency with the probability calculus [2]. So their characterization of degree of belief in the negation of T (–T) is constrained solely by to what extent they think it likely relative to the other theories being considered. However, Joe Hanna[2] has pointed out that, if their partition space does not happen to include the true theory they will never achieve conditionalization on this true theory; furthermore we have no reason to believe that the true theory is always initially included.

2.5. The Old Evidence/New Theory Problem

Glymour argues that according to Bayesian kinematics old evidence cannot confirm a new theory [3]. Supposing e is known before Theory T is introduced at time t. Because e is known at t, Prob (at t) (e)=1, and Prob (at t)(e/T) =1. Thus the posterior probability is equal merely to the prior probability. Then we have the absurdity that old evidence cannot confirm new theory.

One line of defense against this problem has to do with how one assigns the actual degree of belief in old evidence. We have seen the problem with this in the previous discussion concerning how you assign the degree of belief in the negation of a theory. We found that there is no known consistent rule for assigning such belief so the problem was not solved.

Howson takes the position that Glymour's reasoning appears to have the consequence that no data, whether obtained before or after the hypothesis is proposed can, within Bayesian theory of confirmation, confirm any hypothesis [2]. Even if the hypothesis is proposed before the evidence is collected tnen by the time Bayes' theorem is calculated (P)e, and P(e/T) must again be set equal to 1, since by that time e will be known and hence in the contemporary stock of background information. To avoid this absurdity Howson comes up with a rule for assigning the degree of belief in a given piece of evidence:

the mistake lies in relativising all the probabilities to the totality of current knowledge: they should have been relativised to the current knowledge minus e. The reason for the restriction is, of course, that your current assessment of the support of h by e measures the extent to which the addition of e, to the remainder of what you currently take for granted, would cause a change in your degree of belief in h [2].

How you came to accept the truth of the evidence, and whether you are correct in accepting it as true is beside the point. Thus we have turned back to the response to the criticism of subjectivism. This sort of Bayesian is not interested in a method of determining how we come to accept evidence. But once that evidence is accepted he wants Bayesian kinematics to take hold and determine our posterior probability compared to the prior probability, which we held before that evidence. This seems consistent and to solve the old evidence/ new theory problem but it again uncovers the objection to the subjectivism.

2.6. Physicians Are Not Bayesians

Several empirical studies have concluded that people are not naturally Bayesian agents, but this conclusion is contested [4]. Of more interest to my current project is to determine if physicians (who some might consider to be both scientists and in a position to be frequently in need of something like Bayesian kinematics) are Bayesian agents. Such a study has been done and is reported in Giere[4]:

In an experiment at Harvard Medical School, 20 fourth-year medical students, 20 residents, and 20 attending physicians were asked the following question in hallway interviews:

If a test to detect a disease whose prevalence is 1/1,000 has a false positive rate of 5 percent, what is the chance that a person found to have a positive result actually has the disease, assuming that you know nothing about the person's symptoms or signs.

Answers ranged from 0.095 percent to 99 percent. Almost half (27/60) of the respondents gave the answer as 95 percent. The average response was 56 percent. Only 11 of the 60 participants – four students, three residents and four attending physicians – gave the correct answer, which is 2 percent! The answer is based on the assumption, which the experimenters seem to have taken for granted, that the test has a false negative rate of zero, that is, it never fails to detect a person who in fact has the disease. With this assumption, the correct answer is provided by simple application of Bayes' theorem.

This experiment is an example of a natural human tendency to ignore the base rate when judging probabilities. One tends to ignore the information that only 1/1000 have the disease which, because it is relatively small compared to the false positive rate of 1/20, leads to a mistaken estimate [4].

Of course the demonstration that people are not Bayesian agents does not prove that they should not be Bayesians. Perhaps they should be but we have not as yet succeeded in proving that the Bayesian schema is a correct method of scientific inference. What further course might the Bayesian take?

2.7. Bayesianism as a System of Learning

With the issue of Bayesianism still being unresolved as an explanation of scientific inference the Bayesian naturally turns to the argument that the Bayesian system provides us with a *theory of learning*. We may start off with any a priori degree of belief that we want but if we follow the method of reasoning dictated by the probability calculus we will learn ultimately the truth. This gives us a theory of personal learning and it seems to be correct, as far as it goes. A cardiologist I recently heard stated it this way, "It makes sense that your chances of catching fish are determined by whether you are fishing in a lake with fish to begin with". Yes, that seems to be true. Evidence gathered from experience with fishing in the lake will presumably lead me to come up with a successful method to catch fish. But this method tells me nothing at the outset as to whether there actually are fish in the lake. If there are none then none of my evidence will count towards or even against the hypothesis. I may eventually give up because of failure to make any catches but the truth or falsity of the hypothesis has remained untouched. This point is similar to that referred to above by Hanna, and the problem of the neglect of the base rate. Clearly the kinematics will converge and learning will be achieved, but only if we assume beforehand that we know the base rate and that it is included in our process. (There has to be fish in the lake.)

3. THE PROBLEM OF INDUCTION REVISITED

Goodman showed how the old problem of induction has been solved by the process of *reflective equilibrium* which is a process of give and take of rule and inference: "A rule is amended if it yields an inference we are unwilling to accept; and inference is rejected if it violates a rule we are unwilling to amend" [5]. The agreement between rules and accepted inferences accounts for the process of justification. So the old problem came about because of the separation of the source of prediction from its justification. But a new problem arises, that is, of deciding 'What is the positive instance of a hypothesis?' and 'What hypotheses are confirmed by their positive instances?' [5]. How are we to decide what is valid evidence and how are we to determine what hypothesis is being confirmed by the evidence? Hume argued that the problem of induction is solved by psychological habits, but a more fundamental part of the problem is determining which regularities in the past do and which do not establish such habits. We would want to be able to determine which observations are projectible. Goodman's bleen/grue example demonstrates that such projectibility is a function of the theory under which the observation is being made. This brings up the problem of 'theory dependence of observation', that is, the very observation is a function of the theory we bring to the perception. Similarities and regularities do not necessarily present themselves without our applying some groundwork or premises of observation.

In a sense the Bayesian induction scheme is directed at the old problem of induction. If certain probabilities are known then the application of experience will, by the rational application of the probability calculus give one information about the probability of certain features of the future or the unknown. Just as with reflective equilibrium, where the give and take of rule and inference lead to an understanding of justification, the give and take of the Bayesian analysis of the various probabilities will lead to a conclusion consistent within the subjective Bayesian scheme. But the new problem of induction, that of theory dependence of observation, is totally missed by the Bayesian mechanism. Bayes' theorem assumes that we already know what is valid evidence but gives us nothing on which to determine what it is or what hypothesis is being proven by the evidence. The subjective Bayesian would reply that that is no problem, let one decide what evidence you want to test and what are your prior probabilities and then merely calculate. The Reverend Thomas Bayes is believed to have stated:

> It is not the business of a Mathematician to show that a strait line or circle can be drawn, but he tells you what he means by these; and if you understand him, you may proceed further with him; and it would not be to the purpose to object that there is no such thing in nature as a true strait line or perfect circle, for this is none of his concern; he is not inquiring how things are in matter of fact, but supposing things to be in a certain way, what are the consequences to be deduced from them; and all that is to be demanded of him is, that his suppositions be intelligible, and his inferences just from the suppositions he makes [6].

4. SHOULD PHYSICIANS BE BAYESIAN AGENTS?

I have presented the problem of induction and the Bayesian probabilistic method of solution of the problem, but which has failed. I have not yet, however, answered the question asked in the title. It would seem obvious that the answer should be negative. However, this is only in the context of the physician as medical scientist who is performing experiments and analyses in the laboratory (including the clinical laboratory). It seems clear that neither Bayesian analysis will provide the sort of scientific inference which will be projectible in the future – unless the pertinent evidence is gathered in the first place. But it is not the purpose of this paper to try to decide how that is done. That is in the balliwick of each scientific discipline to determine. So I would conclude that there is no real reason for the *physician-scientist* to be a Bayesian agent.

But that is not the problem I asked in the title. I asked if the *physician* should be a Bayesian agent. This is a different question altogether. Physicians in their practice with patients are not scientists. True they have had training in the basic sciences where the methods of scientific inference have been studied. But in the practice of medicine they are applying general knowledge to a particular patient situation. So if the general knowledge is known then its application to a particular patient is a simple deductive enterprise and thus not subject to the problem of induction. In this light, should physicians be Bayesian agents?

An entire literature has grown up in the past several years arguing for Bayesian analysis in medical decision. It goes under the rubric, "Decision Analysis" and a rich literature is available (see e.g. [6–10]). The way Bayesian diagnostic works is:

A diagnostic test is evaluated regarding its sensitivity and specificity. These characteristics are used to formulate new probability statements about the presence or absence of disease in a particular patient examined by the diagnostic test. If a patient has an abnormal test result the probability of disease may be written as P(D+/T+) and if she has a normal test result as P(D+/T–). Bayes' theorem allows us to calculate the posterior probabilities that we wish to know from information that we already know beforehand about the implications of a diagnostic test. If we wish to estimate the probability of disease in a patient with an abnormal test result we must know the probabilities that the diagnostic test will be positive in patients with and without disease – the true positive and false positive ratios and an estimate of the prior probabilities, P(D+) and P(D–).

$$P(D+/T+) = \frac{P(T+/D+)P(D+)}{P(T+/D+)P(D+)+P(T+/D-)P(D-)}$$

It is interesting to note that unlike the typical situation in Bayesian inference (e.g. urn sampling) the value P(T+/D+) is not analytic. Rather P(T+/D+) is itself an unknown or an estimated empirical probability. Also notice that there is no basis in the formulation of Bayes' theorem for approximation to the truth – one would need a sequence of tests T1, T2, T3, etc.[3]

But, clearly the Bayes' theorem utilized in this manner will give us the posterior probability of disease given a positive result. So as a method of decision analysis in diagnosis it is simply an analytic technique. Let us now examine the previously mentioned problems with Bayesianism in this light.

4.1. Subjectivity

Subjectivity is not a problem with utilizing the Bayesian schema for medical decision making, because it is assumed that the prior probabilities, sensitivities and specificities, are known. How these are arrived at is another matter and goes back to the more fundamental problem of induction.

4.2. Prejudice in Favour of Weak Hypotheses

In this context the weak hypothesis would be a high prior probability that the patient has the disease. This would not appear to cause trouble with Bayesianism in this use. All you would need to do is apply it to those patient populations which, going in, have a high probability of having the disease and this is part and parcel of the physician's other diagnostic methods. This is the mistake the physicians made in the experiment at Harvard Medical School, i.e. they neglected a low base rate of the disease (1/1000). Certainly one would want to have a very sensitive test which produced a high likelihood of the evidence given the disease and the need for this highly sensitive test would rise as the prior probability of the disease declined.

4.3. The Problem With the False Hypothesis

In this context the false hypothesis problem would be the probability of getting a positive test in the absence of disease. Clearly one would want a test with a low false positive ratio. But the presence of atherosclerotic coronary artery disease would be determined by other means, e.g. coronary arteriography following stress testing determines this ratio.

4.4. The Old Evidence/New Theory Problem

The old evidence/new theory problem is that old evidence cannot confirm a new theory. This is not a problem with the physician as Bayesian because if the result you are looking for is present before the test is performed then you know there is no reason to do the test.

So as a method of decision analysis in medical diagnosis the utilization of Bayes' theorem appears to be entirely appropriate.

There are other problems however. It is assumed that all the prior probabilities are known and applied by the diagnostician. This is seldom the case. Most physicians bring their own subjective prior probabilities to the analysis. Consider the physician with an aggressively interventionist philosophy. (Most cardiologists.) This person would view medicine as a discipline that seeks, above all, to thwart the natural course of disease. Such physicians would accept a high complication rate for a diagnostic test, high-risk or unproved therapies, and would bring a high prior probability of disease to the study population. Cardiologists, after all, treat heart disease. Heart disease is highly prevalent, highly disabling, devastating, etc. and 'it doesn't take a doctor to do nothing'. Such physicians would expect most patients sent to them to have the disease, until proven otherwise. That is clearly bringing a high prior probability to the analysis.

On the other hand, the more passive noninterventionist physician would harbor the philosophy that illness reflects a certain destiny, and that the body is not to be tampered with unless the intervention has a reasonably high probability of success. Such a physician would obviously bring a low prior probability of disease.

More importantly: How do we objectively place patients in appropriate risk categories? This is the actuarial problem.[4]

Furthermore, how do we incorporate individual patients' attitudes into the analysis? Risk taking favors a high prior probability, but some people are simply not comfortable with an analysis which favors high risk. Others, the 'wishful thinkers' would want a low initial probability to be incorporated when the specific values are unknown. The 'Aristotelian mean' would probably satisfy the most people but the point is the same as that made earlier, that is, that what we want is not to be applying the diagnostic test to those patients we suspect, on other grounds, to probably have the disease, but instead some way of coming up with a diagnostic test which has a high likelihood of result, even if the presence of disease is not known on other grounds.

In other words we want a diagnostic test with a high P(E/H) whatever the prior probability of the disease in the population. Any diagnostic test might be tried but it will have to be validated by means outside of itself. So, Bayesian kinematics will work both for learning by the physicians and in the utilization of diagnostic tests for decision analysis. But the input data, what counts as evidence and the likelihood relationship of the diagnostic test will have to be gathered by other means. If we succeed in that then the problems of decision analysis will become relatively insignificant.

5. CONCLUSION

Several problems with the utilization of Bayesian inference as the basis of a medical decision procedure have been considered. The major criticism against Bayesian kinematic has always been the degree of subjectivity of the prior probability. Although the ongoing kinematics, as a consequence of experience, has a tendency to converge to the truth, there is no way of knowing if the true value is part of the initial judgment. This subjectivism appears again in the consideration of whether physicians should be Bayesians, for an 'interventionist' will come with a high prior probability of disease, while a 'noninterventionist' will begin with a low relevant probability. The actual incidence of the disease in the population will have to be derived by other means, and only after such derivation, will this value be of use to the physician. So the Bayesian system of inference does not and cannot solve the problem of induction, but as a means of decision-making over a population in which the incidence of disease is already known, it can prove useful.

NOTES

[1] Personal communication, Professor Joe Hanna, Department of Philosophy, Michigan State University.
[2] See note 1.
[3] See note 1.
[4] See note 1.

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