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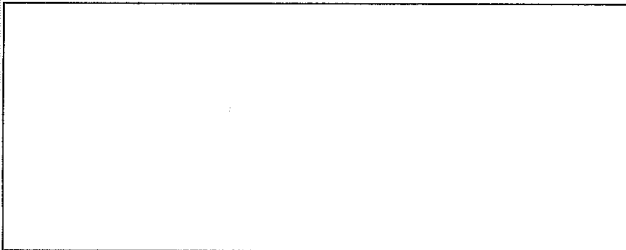
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Bas C. van Fraassen

THE PROBLEM OF OLD EVIDENCE*

1. INTRODUCTION

There is an underground stream in modern epistemology, starting from
Pascal and Huygens in the seventeenth century. In ours, it is represented
largely, but not wholly, by the writers who call themselves Bayesian. To
follow Pascal and Huygens means only to represent opinion in terms of
probabilities (possibly vague) and to insist that the subject of opinion can-
not be understood in isolation from those of value or practice. The
Bayesians add to this a specific attempt to reconstruct traditional epistemo-
logical concepts in probabilistic terms. This attempt (for an overview, see
Horwich [1982]) begins with the definition of *confirmation* as enhance-
ment of subjective probability. I wish there were a catchy name for the
three centuries old underground alternative as a whole. (Perhaps *probabil-
ism*, a term that has some currency, could do.) For the specifically
Bayesian addition, Clark Glymour raised a specific difficulty: *the problem
of old evidence.*

Since I.J. Good demonstrated that there are more varieties of
Bayesianism than Bayesians, it is perhaps not surprising that Clark
Glymour's chapter 'Why I am Not a Bayesian' (in Glymour [1980]) was
met with some incredulity. Explicit reactions included essays attempting to
show that his characterization of Bayesianism was too narrow, that his
theory of relevant evidence and bootstrap confirmation was compatible
with and indeed needed a Bayesian framework for its setting, and that
Glymour really is a Bayesian too (see Earman, ed., [1984]). Yet the main
problem he raised -- the problem of confirmation by old, i.e. previously
known, evidence -- required serious response. I will explain the problem
and then attack Garber's response, which I take to be the main contender
for a solution.[1]

2. THE PROBLEM

The paradigm case for the typical strict or orthodox Bayesian has evidence
coming to the agent/subject in the form of a *proposition* E, and his *taking it
as evidence* consists in his amending his epistemic state by simple condi-
tionalization on E. A very ideal agent would have this state represented by

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a simple probability function P, whose conditionalization is P_E, defined by $P_E(.) = P(. |E) = P(. \&E) / P(E)$. In a less idealized model, the person's probabilities are vague, and may be represented by a class of probability functions. Many problems, including that of old evidence I think, can be examined without loss of generality by attending to the ideal case alone.

The concept of confirmation is typically characterized by Bayesians as follows: proposition E confirms hypothesis H (itself a proposition) for an agent/subject if his credence in H conditional on E is higher than his credence in H *tout court*. (So, in the very ideal case, if $P(H|E) > P(H)$.) Note that this is a relation well-defined for any two propositions and that it is a relation which is not 'objective' but relative to the agent's present epistemic state. There is specifically no requirement that he places any particular degree of credence in E, as long as it is not zero. Now Glymour pointed out that if the agent has already, previously, become certain that E, then it follows at once that E confirms no hypothesis whatever for that agent, according to this characterization. (In the very ideal case, $P(E) = 1$ entails $P(H|E) = P(H)$.) So this seems to make nonsense of such claims as that the advance in the perihelion of Mercury, known for a century, and indeed for a half-century before Einstein's work, confirms Einstein's theory of relativity. Glymour, of course, holds that confirmation is a relation between evidence and hypothesis which is not relative to the agent's epistemic state at all.

Three possible responses come immediately to mind, at least in outline form. The first is that no one is that certain about E, whatever it may be. It is remarkable that even Richard Jeffrey has not dismissed Glymour's problem so curtly; remarkable, because that response would be the most effective possible, it would remove the problem altogether. The second response is that if today we say that E, long since believed with certainty, confirmed H, we make reference not to our present actual epistemic state but to some alternative(s) thereto. (The tacit 'for us' or 'for me' that usually fills out the ellipsis, is to be understood as replaced by some other clause.) This sort of response Glymour already considered and criticized when he raised the problem; I shall consider this a bit further below. The third response is that in this sort of case, the phrase 'E confirms H' may well be used in the speech of the vulgar, but attention is being drawn to something else that has to do with E, and really does confirm H for us. Indeed, when the scientific community got to the point when it could say that the advance in the perihelion of Mercury confirmed Einstein's theory, they had indeed just newly learned *something* -- though not *that* fact about Mercury, but another fact that has to do with it -- had conditionalized on that something, and the conditionalization had increased their credence in Einstein's theory. Daniel Garber proposed this response, and elaborated it (Garber [1982]). His solution to the problem was further developed by Jeffrey, who observes that in view of the solution, the problem should be renamed 'the problem of new explanation'.

The main purpose of this paper is to criticize Garber's solution. This project of specifying the relevant alternatives as required by the second sort of response (let us call it the *alternative priors* solution) appears to me hopeless and misguided, and if I am right, Garber's solution runs into exactly the same sort of difficulties. In the discussion I shall proceed with reference to the very ideal case of an agent/subject whose epistemic state can be characterized by a single probability function. (More sophisticated theories accommodating, for instance, vague probabilities, such as Levi's and Jeffrey's, come with explanations of why looking at single function is a good way to proceed.) After the critique I shall attempt to be more constructive, and indicate how both Glymour's problem and Garber's solution raise other problems whose importance extends well outside the special context set by discussions of the concept of confirmation.

3. ALTERNATIVE PRIORS SOLUTIONS

It is important to note the main difficulties with what I called the 'alternative priors' solutions -- for I will argue that Garber's solution is subject to the same problems. If (E) the advance in the perihelion of Mercury had been predicted by Einstein and observed only after that prediction, we could say that, before the observation the credence in (H) Einstein's theory conditional on E was higher than it was *tout court*, and that the observation establishing the fact that E raised the credence in H. This fictional case tempts us to say that, in the real case, E was said to confirm H, because if it had been established sufficiently later than it actually was, it would have confirmed H, strictly speaking. To verify that counterfactual assertion however, requires a good deal of confidence about what the scientific community's credence function would have been, had these facts about Mercury not become previously known. How could they not have become known? Only if scientific practice during the preceding century had been considerably different from what it was. And how could that have been? Only if either the political and economic situation had been very different, or if it had been the same but scientific practice as such had developed very differently in the modern era. But in view of this, we are so confident that the scientific community would, in either case, have been intellectually ready and equipped not only to take Einstein's theories seriously, but to see the import of his theories for the motions of the planets?

Nor are the difficulties to our imagination assuaged by asking what would have happened if Einstein's theories had been presented to the world in the nineteenth century, before these facts about Mercury became known. For in that case, we might feel, he would have remained a misunderstood and unappreciated genius, and we might still not understand him.

There are indeed epistemological theories in which the idea of an epistemic state maximally like my own among those in which E does not

receive total credence, is taken very seriously. In such a theory, the crucial counterfactual has an important and objective status, and it may be even assumed or postulated that I can have a pretty good idea about its truth-value. But all the doubts I have voiced, and which I think many would share, about that counterfactual itself apply *a fortiori* to such theories. This is not to belittle the great achievements of the theory of counterfactuals in our day; only to emphasize that they have not, and were not intended to have, significantly increased our ability to determine the truth-value of specific counterfactuals.

Suppose that my present epistemic state is indeed representable by a probability function. Some notions of alternatives to this, are certainly very clear. These are the alternatives that characterize other possible ideal agents/subjects, as rational and coherent as I am, who had different priors and different evidence. This class of alternatives is very large and perhaps includes all probability functions. Secondly, there are all the alternatives 'open to me', to adopt in response to the deliverances of experience to come -- this class includes all probability functions reachable through rational probability kinematics from my present state. On the most conservative view this means all functions definable as conditionalizations of mine on some proposition that receives a positive value from mine. On such a more liberal view as Jeffrey's, it may include all probability functions absolutely continuous with respect to mine ('no raising of zeroes'). And on a still more liberal view, which allows for small individual conceptual revolutions ('throwing your prior away') it may again includes the class of all probability functions. These classes are the extensions of intelligible notions of alternatives to the present epistemic state. They are of no use to the 'alternative priors' solution for the old evidence problem; and I do not believe that any but the most highly controversial, postulational approach could do better.

4. GARBER'S SOLUTION

Let us begin by considering what Garber's solution is not, or should not be. Suppose that after listening to Glymour, some very old fashioned Bayesian had said: After all, changes in one's state of opinion, if rational, are always cases of conditionalization. So if a scientist comes to place greater credence in hypothesis H when he takes another look at old data, E, then what is really going on *must be* conditionalization on some proposition, though it is clearly not E. If he said this, and stopped there, we would hardly be inclined to say that we had just heard a Bayesian solution to the problem of old evidence.

Indeed, suppose this old fashioned person added that the two salient propositions in the process were clearly H and E, so that it would be convenient to refer to that proposition (the he-knows-not-what on which

the credal state was conditionalized) by the functional name ⌜H⊦E⌝. (Note: I shall omit the corner quotes when too cumbersome.) We would obviously still not be inclined to speak of a solution, for he has only given a name to the proposition there must be if his views are to remain tenable.

The value and importance of Garber's work, and the reason why we speak here of a solution, is that he attempts to tell us what this proposition is -- to identify a proposition which can play the required role and to show us that it can.

For Garber tells us that this proposition is to be identified as the proposition that H implies E, and when an agent claims that previously known evidence E confirms H, he should be understood to assert that, with respect to his epistemic state P, P(H|that H implies E) is greater than P(H). Clearly some story must be told. To establish Glymour's scenario as fitting into the Bayesian mold we must be able to say that if this person believes that E, it is plausible (at least in the important examples) that his conditional probability for H on the supposition that H implies E is greater than for H simpliciter. This point is explored by Jeffrey at some length.

But of course we worry that the same problem should not occur at second remove. If H implies E, is that not a logical truth, which this person must assign 1 on pain of incoherence? Here Garber steps in to write a (neo-) Bayesian theory for the logically non-omniscient subject. For this subject, the proposition ⌜H⊦E⌝ which we read as 'H implies E', is logically contingent. It is *by definition* the new evidence on which Glymour's scientist conditionalizes but it is *by Garber's hypothesis* identified as the proposition that H implies E. Given the preceding paragraphs we recognize that Garber should be sensitive to the demand that identification of the proposition called ⌜H⊦E⌝ be reasonably warranted, and indeed he is. For exactly this reason he imposes the validity of *modus ponens* as a minimal condition on the conceptual role of ⌜H⊦E⌝.

Well, does the validity of *modus ponens* provide reasonable warrant for the identification? Conjunction obeys *modus ponens* too; so do many other propositional functions. Apparently to obviate this sort of objection, Garber remarks that ⌜H⊦E⌝ could have, for the intended generality of his account, other interpretations as well. I find these remarks very puzzling for two reasons. First, the value of his essay over the grumpy, dogmatic retrenchment of a diehard Bayesian is exactly that he is not just postulating the existence of the required sort of proposition. He is meant to be showing us that there really is one, by showing what it is. Secondly, the alternatives he mentions are entirely unsuitable. Of course, *conjunction* and *material implication* would not do, because in the examples the agent does *not* learn H & E, and he has already learned $\sim$H $\vee$ E when he learned E. Instance confirmation and bootstrap confirmation, which Garber mentions explicitly, will not do because they do not satisfy *modus ponens*. For example the data E = (P = 2 & V = 4 & T = 8/r) both instance-confirms and, under suitable conditions, bootstrap-confirms the universal hypothesis

H = (PV = rT), but is certainly not such that if H is true then E is true (nor of course conversely).

Jeffrey suggests, if only by his usage and examples, that the relationship is that of *explaining*. In an explanation, the hypothesis implies anyway part of the data; in the above example, the hypothesis implies that if P = 2 and V = 4 then T = 8/r. It is not such that its truth guarantees the truth of E. So perhaps the examples of confirmation by old data could all be reconstrued as examples of new explanation of part of the data. There are two objections, sufficient to leave us as puzzled as at the beginning of this paragraph. First of all, the amount of confirmation by the whole of the data will even in these cases generally be larger: P[(x) (Fx ⊃ Gx) | Fa & Ga] could be greater than P[(x) (Fx ⊃ Gx) | Fa ⊃ Ga] for example if Ga is stochastically relevant, in P, to Gb, Gc,.... But more important, qualms about Hempelian views concerning explanation aside, Jeffrey appears to think of explaining as implying plus perhaps a little more, so his alternative is one on which ⌜H⊢E⌝ has to mean *at least* that H implies E, so it is hardly a reason to consider *modus ponens* enough of a restriction on the conceptual role of that sentence.

The conclusion, apparently inescapable, is that Garber's solution has not been substantiated unless he can make it work in the presence of minimal adequacy conditions for the interpretation of ⌜H⊢E⌝ as ⌜H implies E⌝, and that *modus ponens* alone is less than minimal. To highlight this conclusion, look at Garber's proofs of his adequacy theorems: there is not *prima facie* warrant that they can be adjusted to meet more stringent conditions than the validity of *modus ponens*; and the grumpy, stubborn old-fashioned Bayesian of my fiction could have proved these theorems more easily without tempting us for a moment to call him more than stubborn!

5. THE CONDITIONAL PROOF REQUIREMENT: PRELIMINARY DISCUSSION

The most obvious extra condition to be demanded for ⌜H⊢E⌝ is the validity of some suitable form of *conditional proof*. At this point we must proceed as sympathetically as possible, to see how Garber could have imposed such a requirement in addition to *modus ponens* with no or minimal consequences for the rest of his solution.

Garber himself raises the problem: since coherence requires that we never be less than certain about tautologies, does it not also require that we are certain that H implies E if it does? The ambiguity in 'tautology' becomes important here. If I were to bet against any necessarily true statement, I would lose money whatever turns out to be the case. However, that behavior could not convict me of irrationality if I could not have known that the statement was necessarily true. The usual Dutch Book argument, is given for a language only just rich enough to provide parallels

to the usual axioms of probability theory. So it appears to proceed on the assumed equation of necessarily true statements with tautologies of classical propositional logic. Now if H implies E, this may well be because it does so in virtue of a correct logic or necessarily true theory which goes well beyond propositional logic. Hence if such criteria of rationality as coherence require the *a priori* mastery only of the laws of truth functional connectives, and we are counted rational provided we do not violate those criteria, then we are not required to have certainty about all true statements of implication. There are a number of 'ifs' here, but the conclusion seems independently plausible anyway.

But this rationale immediately suggests a picture, a doctrine about the conceptual role of the turnstile, that makes a hash of Garber's solution. For suppose that I hold there to be some true theory T -- perhaps I know it only by description as the theory axiomatized in *Principia Mathematica* and do not pretend to understand it very well -- such that H implies E if and only if T and H together imply E by the standards for classical propositional calculus. Then my characterization of the conceptual role of the turnstile is this:

> ⌜H⊢E⌝ expresses the logically weakest proposition which together with H and T implies E in the propositional calculus.

But that proposition is the material implication (T & H) $\supset$ E; hence, since I hold T to be true, I am certain that ⌜H⊢E⌝ is true as soon as I am certain that E is true.[2] On this construal, Garber's solution falls prey to the very same objection that Glymour brought to the more old-fashioned view.

Note especially that this reasoning does not require that the auxiliary theory T be expressible in the object language -- only some meta-linguistic understanding is required on the part of the speaker to become clear on what *sort* of thing he means by his turnstile.

Of course, similar puzzles have been encountered in the study of modal logic. So from a formal point of view at least we should know how to help Garber (at this point of extrapolation I should probably call the protagonist Garber*) out of this predicament. The sentence ⌜H⊢E⌝ will have to mean that H implies E in some more full-blooded sense; something like that H guarantees the truth of E in a large variety of cases and not only in the actual one -- or something that bears some formal similarity to such modal statements. Whether this suggestion does help, we shall now explore.

6. THE CONCEPTUAL ROLE OF 'IMPLIES'

When Garber imposes the validity of *modus ponens* as a condition on the language containing the statement $\ulcorner H \vdash E \urcorner$ he does so by suggesting first

K. $P(B|A \ \& \ (A \vdash B)) = 1$

then replacing it by

K*. $P(B \ \& \ A \ \& \ (A \vdash B)) = P(A \ \& \ (A \vdash B))$

because, as he remarks, Popper and Renyi aside, equation K is undefined when $P(A \ \& \ (A \vdash B)) = 0$. I will leave the tantalizing reference to Popper and Renyi aside too for now[3], and ask if we can impose some condition analogous to K* to honor the intuition of the validity of conditional proof.

We must now proceed a bit more formally than heretofore. Let the agent's language be L, containing at least the usual propositional connectives and having Garber's turnstile too (let us leave open whether or not the grammar allows this turnstile to be nested), and perhaps other logical machinery. Garber suggests the picture of a hand-held Bayesian calculator, to work with probability functions possibly defined only on a sublanguage of L, for use in coping with actual everyday problems. Presumably the possible inputs for such a calculator would be probability functions defined on some sublanguage L', and so would its outputs (possible posteriors). The operations it could perform correspond to those of rational probability kinematics (see my [1980]). Whether these operations be conditionalizations, or more generally Jeffrey shifts, or even more generally selections of new probability functions with minimal information relative to the input (the prior), we leave open. All such operations have in common that they do not raise zeroes: if a proposition has prior probability zero, its posterior probability is not larger.

Turning back now to the constraint of conditional proof, its general form must be of the form

KK*. If $P'(B \ \& \ A \ \& \ . \ ) = P'(A \ \& \ . \ )$ for all P' in $\Sigma(P)$
then $P(A \vdash B) = 1$

where (P) is the 'correct' class of alternatives to P to allow the sort of generalization involved in conditional proof.

Our preceding discussion should already have made clear what dilemmas we face here. If the evaluation of $A \vdash B$ involves the consideration of the class of priors we might have had, if A had not previously been taken as evidence (if it has), Garber's solution runs into the insurmountable philosophical objections that beset the 'alternative priors' solution. So

$\Sigma(P)$ must be characterizable independent of such doubtful counterfactual deliberations.

The best possibility that suggests itself is that we choose as $\Sigma(P)$ the set of possible posteriors which the Bayesian calculator can read from input prior P. Since, however, its operations does not raise zeroes, we see that if $P(E) = 1$ then for every such possible posterior probability P', we have $P'(E \ \& \ H \ \& \ . \ ) = P'(H \ \& \ . \ )$, so $P(H \vdash E) = 1$; and Glymour's objection has not been defeated.

Indeed, to show how little elbow room we have here we can make the following observation.

> *Little theorem*: If all members of $\Sigma(P)$ are absolutely continuous with respect to P ('no raising of zeroes') and KK* holds generally, then $\vdash$ is probabilistically indistinguishable from the material conditional.

First let us prove that the probability of $A \vdash B$ is never higher than that of $\sim(A \ \& \sim B)$. This is obviously so if the former receives zero; if its value is positive the result follows from the validity of *modus ponens*. For in that case $P(B|A\&(A \vdash B)) = 1$ and therefore $P(\sim(A\& \sim B)|A \vdash B) = 1 - P(A|A \vdash B)[1 - P(B|A\&(A \vdash B))] = 1$; hence $P(\sim(A\& \sim B)\&(A \vdash B)) = P(A \vdash B)$.

Let us secondly prove the converse. If $\sim(A\& \sim B)$ receives zero, its value is no greater than that of $A \vdash B$; if it receives a positive value let us consider the function $P'(\ . \ )=P(\ . \ |\sim(A\& \sim B))$. We clearly have $P'(A\& \sim B) = 0$, so for all P" in $\Sigma(P')$, $P"(A\& \sim B)=0$, and so $P"(A\&B\& \ . \ ) = P"(A\& \ . \ )$, hence by KK*, $P'(A \vdash B) = 1$. But recalling what (P' is, we see that $P(A \vdash B|\sim(A\& \sim B)) = 1$ so $P((A \vdash B)) = 1$ so $P((A \vdash B)\& \sim(A\& \sim B)) = P(\sim(A\& \sim B))$.

We have now deduced, in effect, that $A \vdash B$ and $\sim(A\& \sim B)$ will receive the same value from any probability function. I see no way out of this except to start considering *alternatives not absolutely continuous with respect to the given prior*, that is, *priors we might have had if we did not have all this old evidence* that raised the problem in the first place. And formal technicalities aside, I see no possibility of a convincing philosophical story about the meaning and use of the turnstile along those lines.

7. CONCLUSION

In my opinion we had in this literature on Glymour's problem of old evidence, a conflation of two problems. The first problem has -- it seems to me -- a mistaken presupposition. This is the problem of explicating confirmation in terms of subjective probability. The second problem is a real one, but not peculiar to probabilism. It is the problem of relating the

propositions (or sets of possibilities, or events, or...) to which truth-values and probabilities are rightfully assigned, to the *sentences* which express them. To this second problem Garber offers a solution, but I do not believe that the solution works.

To elaborate on the first problem, let me return to the perspective I introduced at the beginning. The main stream of modern epistemology, beginning with Descartes, brought forth also an epistemology of science which focussed on the question of how evidence supports acceptance of theories. The question was split into two parts, and the main answers elaborated were these: (1) acceptance is belief which must be justified by the relation of support by the actual evidence of the contemplated theories; (2) the relation of support is 'objective', that is it is entirely determined by the information taken as evidence and the information contained in the theory, and not a function of subjective or historical parameters. While (1) is largely tacit, (2) was canonized in Herschel's distinction between context of discovery and context of justification.

With respect to (2), I abstracted of course from the diversity of further opinions about whether this relation of support can be explicated through ideas about induction, consilience, inference to the best explanation, or what have you. But attempts to settle these questions were beset by many difficulties. Indeed, parameters belonging to the subjective or historical context were always cropping up in the analyses of evidential support -- or else the analysis would include promissory notes with little value besides hope. And example of the first is Whewell's consilience, in which the historical novelty of prediction, the independence of classes of phenomena with respect to previously accepted theory, and the range of historically proposed hypotheses all played an important part. The second is illustrated by Hempel's theory of confirmation, which only applied to simple cases, and moreover needed great sanguinity with respect to certain paradoxes and the notion of 'projectible' predicate.

Attempts to use the notion of probability to elucidate evidential support began with a tacit agreement at least to (2) above. They also fell into the two sorts of failure noted. Thus Reichenbach's inductive logic, based on the frequency interpretation of probability, uses the idea of right reference class -- and to this day we have not seen a successful non-subjective cash-value for this notion as used in inductive inference. (See my [1983]). Carnap's theory on the other hand is premised on the hope that a sufficient number of logical symmetry principles will single out a unique Ur-probability function as correct 'informationless' prior probability.

The great appeal of the Bayesian entry into the field is that it cuts the knot, and disowns (2) altogether. To regard personal opinion -- explicated as subjective probability -- as the central concept of epistemology, is to introduce a perspective in which (1) and (2) no longer look like the forced form of the response. There is a shift in problems as well as concepts; the problem of rational change of opinion replaces that of rational

opinion as such, and itself takes on a new form. All this is actually true as soon as we follow the lead of Pascal and Huygens, and before we make what I called the distinctive Bayesian addition.

But that distinctive addition is exactly what Glymour's critique calls into the question. And even independently of his critique we must surely say that the Bayesians do us not service when they take the word 'confirmation' -- which was a technical term in a context in which (1) and (2) were presupposed -- and give it the meaning they do. For in their sense, confirmation is a relation involving three terms: the evidence, the theory, and someone's prior opinion. It is a little as if another philosopher defined freedom as the ability to do what is good for you, or explanatory power as the ability to predict, or knowledge as true belief.[4] So it should not surprise us if Glymour can raise a problem which drives a wedge between the traditional concept and the Bayesian explanation.

A Bayesian may reply to this that we must still account for the fact that a person's opinion of a hypothesis does improve once he realizes that it fits old evidence. I agree. That is the second problem: we are in uneasy commerce with the range of possibilities about which we mean to have opinions. We can only express our judgments in words, and have not perfect insight into what we mean. So we may *come to realize* that a theory implies something that we already believed, or that some cherished full belief has decidedly odd consequences. But I think this problem, which concerns the relation between imperfect understanding and opinion, has nothing to do *specifically* with the explication of scientific methodology. It is, for instance, exactly the same as the main general problem investigated in the first five chapters of Stalnaker's *Inquiry*. But, however that may be, Garber proposes an approach to this second problem, a general form for its solution; and it does not seem to me to be successful.

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NOTES

2 There appears to be some connection between my criticism of Garber and arguments Stalnaker attributes to Larry Powers, Saul Kripke, and Hartry Field. See Stalnaker [1984], p. 76 and note 17 to Chapter 4.

3 Condition K is not strong enough even if we do take conditional probabilities seriously; it should be $P(B*A \& (A*B) \& .) = 1$ (I use the dot as functional notation; $f(.)$ $= a$ means that $f(x) = a$ for all arguments x in the domain of f.) See my [1981] for a treatment of the conditional and other connectors in the context of Popper or Renyi conditional probabilities.

4 This description of the Bayesian approach may be contentious. Consider the following ploy: the Bayesian explains as correct all traditional ideas about confirmation which hold, on his interpretation, for all priors. For example, it is correct that if H implies E, then (finding) E confirms H. This is because it is a theorem of probability theory that if H and E both have non-zero probability, and H implies E, then the conditional probability of H on E is indeed greater than the probability of H.

As an explication of traditional methodology this fails in that it is never applicable to a case in which a hypothesis is actually considered to be confirmed by a successful prediction. The reason is that the total evidence is never a proposition logically entailed by the hypothesis. For example the total evidence is that we found a white swan in the meadow; but the hypothesis implies only that if we found a swan, it was white. Now it is easy to describe a prior probability function for which that total evidence lowers the probability of the hypothesis in question. Thus we have not explained this case of confirmation by showing that the evidence raises the probability of the hypothesis regardless of the prior. If the correct methodological principles are the ones that hold on all possible priors, then they are all trivial. If instead they are the ones that hold on all priors in a certain range (or 'reasonable' ones), then the methodology still violates what I called condition 2 if the content of that range is historically determined. If not, as in Carnap's program, we must be shown in detail how the exact content is determined, and how it issues in real, adequate explications of traditional methodology. Needless to say, perhaps, I do not believe this will happen.

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