Australasian Journal of Philosophy
Vol. 54, No. 3; December 1976

D. H. MELLOR

PROBABLE EXPLANATION

I

Must explanation make probable what it explains? Hempel[1] says yes; their critiques of Hempel lead Jeffrey[2] and Salmon[3] to say no. I say yes, while accepting most of their critiques. It remains to say why, and why it matters.

These probabilities must be objective. Subjective probability[4] explains nothing: people's expectations (apart perhaps from Uri Geller's) do not explain why coins land tails. I stick to statistical paradigms—coin tossing, conception, radioactivity—and take a propensity view of them.[5] The high chance that explains ten coin tosses giving some tails itself results from the coin's dispositional fairness. Propensities, however, are not crucial to what follows. True, one good if inessential argument in § III fails on frequency views of chance, and I discuss that then. But frequency apart, an explanatory probability can just as well be a relation between the explanandum and a suitable statistical explanans. I do not, however, call the relative probability epistemological[6] or inductive,[7] because explanation is not confirmation, as I argue below in § II. An explanans is not there to give evidence or inductive support for its explanandum, although it may do that on the side.

[1] C. G. Hempel: 'Deductive-nomological vs. statistical explanation' in *Minnesota Studies in the Philosophy of Science*, Vol. III, ed. by H. Feigl and G. Maxwell (1962) pp. 98-169.
[2] R. C. Jeffrey: 'Statistical explanation vs. statistical inference', reprinted in *Statistical Explanation and Statistical Relevance*, ed. by W. C. Salmon (1971) pp. 19-28.
[3] W. C. Salmon: 'Statistical explanation' in *ibid.*, pp. 29-87.
[4] See B. de Finetti: 'Foresight: its logical laws, its subjective sources' (1937) in *Studies in Subjective Probability*, ed. by H. E. Kyburg Jr. and H. E. Smokler (1964) pp. 93-158; and B. de Finetti: *Probability, Induction and Statistics* (1972).
[5] D. H. Mellor: 'Comment [on Salmon: 'Theoretical explanation']' in *Explanation*, ed. by S. Körner (1975) pp. 146-52; and D. H. Mellor: 'In Defence of dispositions', *Philosophical Review* 83 (1974) pp. 157-81.
[6] See chapter 7 of H. E. Kyburg Jr.: *Probability and Inductive Logic* (1970).
[7] As in R. Carnap: *Logical Foundations of Probability* (1950).

To start with, I put my tenet (T) like this: a (good) explanation raises or makes high its explanandum's probability, *p*; and the more it does so (*ceteris paribus*) the better it is. Harman puts it in terms of chance: 'the greater the statistical probability [i.e. chance] an observed outcome has in a particular chance set-up, the better that set-up explains that outcome'.[8]

T will not do as it stands. Which should a (good) explanation do: (i) raise *p* or (ii) make *p* high? It may do (ii) and not (i), if *p* was even higher before. We seem, if we equivocate, to face a paradox such as Popper[9] aimed at Carnap's 1950 concept of confirmation: *B* could explain (confirm) *A* (by raising *A*'s low probability to *p*), fail to explain (confirm) A^* (by lowering A^*'s high probability to p^*), yet explain (confirm) A^* better than *A* (because $p^* > p$). In fact, though, the paradox is no more real than Popper's is.[10] 'Explanation', like 'confirmation', is ambiguous; at least, it can refer to different things, and to each of these only one of T(i) and T(ii) applies. What explanations are may depend on whether we deal in chance or relative probability, so I make the point for both.

First, relative probability. Suppose a request for explanation, 'Why *A*?', elicits the response 'Because *B*'. The respondent presumes a background *K* of relevant common knowledge, and either *B* alone or *B&K* together may be called the explanans. What applies to *B&K* is T(ii), that *A*'s probability, *p*, relative to it be high. This is what Hempel meant in urging T(ii): 'an argument of this [probabilistic] kind will count as explanatory only if [*p*] is fairly close to 1.'[11] What applies to *B* is T(i), that *B* raises *A*'s probability above its value relative to *K*:

(1) $p(A,B\&K) > p(A,K)$

or its value relative to $\sim B\&K$:

(2) $p(A,B\&K) > p(A,\sim B\&K)$

(1) is what Salmon meant in preferring T(i)—although he still rejected it—to T(ii): 'It is more accurate to say that an explanatory argument shows that the probability of the explanandum event relative to the explanatory facts is substantially greater than its prior probability'.[12] (1) is likewise what Cohen advocates.[13]

In statistical examples, T(i) mostly means (2). Chance is relative to alternative states of affairs rather than to alternative states of knowledge (but see § III below). I suppose a man's heavy smoking (*B*) explains his getting lung cancer (*A*) because its chance exceeds that for similar

[8] G. Harman: *Thought* (1973) p. 137.
[9] K. R. Popper: *The Logic of Scientific Discovery* (1959) Appendix *ix.
[10] See Carnap's preface to the 2nd edition of *Logical Foundations of Probability* (1962), and P. Baillie: 'That confirmation may yet be a probability', *British Journal for the Philosophy of Science* 20 (1969) pp. 41-51.
[11] C. G. Hempel: *Aspects of Scientific Explanation* (1965) p. 390.
[12] W. C. Salmon: *op. cit.* (1971) p. 36.
[13] In L. J. Cohen: 'Comment [on Salmon]' in S. Körner (ed.), *op. cit.* (1975) pp. 152-9.

(K) non-smokers ($\sim B$). I might have meant that learning of B raises p above its value relative to K alone, but there are problems with that reading, as we shall see in § IV.

It does not matter much how we read T(i). T(ii) is the fundamental form of T; the only point of raising p, whether from an alternative or from a previous value, is to help to make it high. The onset of a smoker's cancer is still not (well) explained while $p(A,B\&K) < \frac{1}{2}$. The question is, why not?

II

Hempel's answer to the question derives from his theory of explanation as inference. His (1962) account of probabilistic ('inductive-statistical') explanation complements his classic account of deductive-nomological (DN) explanation.[14] The DN explanation of events by their sufficient causes is what matters here; I shall call it causal explanation. Hempel thinks that all these explanations are arguments or inferences: to explain something is to give premises for inferring it. Causal explanations are deductive inferences; the explanantia could not be true and their explananda false. In probabilistic explanation that is still the ideal, albeit unattainable. The less the relative probability of the explanandum's falsehood, the closer to deductive validity, the safer the inference, the stronger the explanation. Hence the tenet T.

T of course merely relates explanation and probability; it does not itself identify explanations with inferences. Hempel in effect uses that identification to explain T by deducing it from what is a very evident virtue in inference. The aim of inference is evidently to make it safe to assert the conclusion, so that it may thereafter count as knowledge and serve on its own, detached from its premises, as a new premise for action and further inference. Increased probability in a conclusion perhaps equally evidently makes for safer assertion. Then if, as Hempel says, the object of explanation is (*inter alia*) to give premises from which to infer the explanandum, then the tenet T could no doubt be thus securely inferred and so explained.

But explainers don't need to come to conclusions (except about what the explanation is). An explanandum is not an hypothesis, whose truth needs inferring from more surely known premises. It is already taken to be true, its truth indeed being what calls for explanation (§ IV). The more surely known an explainable proposition is, the more it needs explaining; but, by the same token, the less it needs to be inferred.

I do not deny explanation's close links with inference, which may indeed tempt one to confound them. There is the principle of inference to the best explanation,[15] which says we have some reason to believe

[14] In C. G. Hempel and P. Oppenheim: 'Studies in the logic of confirmation', *Philosophy of Science* 15 (1948) pp. 135-75.

[15] See G. Harman: 'The inference to the best explanation', *Philosophical Review* 74 (1965) pp. 88-95.

what would if true best explain a known fact. Theories of prehistory, for instance, are largely assessed by how well they would if true explain the archaeological record. So prehistorians try to explain artefacts, although their real interest is in inferring prehistorical conclusions. A prehistorical proposition B may thus be inferred from its providing the best explanation of artefact A; but that, of course, does not make the explanation itself an inference. Likewise with the principle of inference to what the known facts best explain. This is the principle by which the defendant's obvious motive for the crime may clinch the case against him. But this again does not make the explanation itself an inference. The premise for the inference to A (guilt) is not the proposed explanans B (motive), but the fact that B explains A better than $\sim A$.

So explanation is not inference, as Jeffrey[16] observes for probabilistic explanation and Salmon[17] for causal explanation. To be an explanans is not to be the premise of an inference to the explanandum. Nor, for the same reasons, is it to be evidence confirming the explanandum. With peripheral exceptions (like the crime case above), we know the explanandum to start with; it is not confirmation we lack when we call for explanation. T therefore gets no support from the virtues that high probability evidently has in confirmation and inference. If T needs support, it must come from elsewhere.

III

Then why accept T? Well, it is plausible *prima facie,* even when probabilistic explanation has been disentangled from statistical inference. Smoking would help to explain an onset of lung cancer, and seemingly because it would raise the chance of that event. If it did not, it would not; and the more the smoking raised the chance the better explanation it would surely be. The best would be causal explanation, with the chance raised to 1. Plausible examples only shift the onus of argument, of course, and T is anyway less plausible in other examples. So it needs general support, to meet Jeffrey's and Salmon's case against it. I support it first, then attack their case.

T gains support from a general thesis about explanation, which in no way confounds it with inference. It is best put in terms of what prospective explanantia we know and so of the explanandum's probabilities relative to them; it applies to chance as a special case (with suitable constraints on the explanantia). The thesis is that we call for explanation only of what, although we know it is so, might have been otherwise for all else of some suitable sort we know.

The thesis is itself supported by explaining why explanation figures less in logic and mathematics than it does in history and science. To

[16] R. C. Jeffrey: *op. cit.* (1971) p. 20.
[17] W. C. Salmon: *op. cit.* (1971) p. 70.

know something in logic and mathematics is mostly to have proved it, and so to know also that it could not be otherwise. There is not the gap, between knowledge of what is and what must be, that calls forth explanation to close it. (Where there is, as perhaps with Fermat's Last Theorem, a proof would indeed explain.) So proofs generally are not explanations; despite, be it noted, being very paradigms of deductive inference.

In history and science, by contrast, new phenomena perennially reopen the explanation gap. Nazi Germany and Soviet Russia did not seem inevitable allies, so why the Ribbentrop-Molotov pact? The radium atom that decayed was just like the rest that did not, so seemingly it need not have; so, why did it? We do not want more evidence, inference, or proof to tell us what happened: observation has told us that. We want to know why what might not have happened nonetheless did. Causal explanation closes the gap by deducing what happened from known earlier events and deterministic laws. So in this respect it satisfies the demand for explanation: what follows from what is true must also be true. Given the causal explanans, things could not have happened otherwise than the explanandum says.

Sometimes, however, suitable causal explanation is not to be had. An event may lack sufficient causes, as radioactive decay does. Or the causes may not be discoverable, as so far for onsets of lung cancer. Or their sufficiency may not show up in the terms required: a boy's genetic make-up at conception, for instance, causally explains his sex at birth, but it gets only a middling probability from the act of conception itself.[18] In all these cases what happened might not have happened for all we can learn of the prescribed prospective causes; we cannot, in the prescribed terms, close the gap that calls for causal explanation. But perhaps we can narrow it. This epistemic possibility of an explanandum's falsehood comes by degrees, and relative probability I take to be (*inter alia*) the measure of it. So gaps that causal explanation would close completely may be partly closed by probabilistic explanation; that indeeed I take to be its object. That being so, it is the better, *ceteris paribus,* the less epistemic possibility it leaves the explanandum's falsehood; *i.e.* the more it raises the explanandum's probability relative to the complete explanans. Hence the tenet T.

This argument, I must admit, will not be uncontentious. In it I have taken probability to measure objective epistemic possibility. That conception has a respectable ancestry in classical and range theories of probability.[19] It does not fit frequency theories[20] apart from Braith-

[18] R. C. Jeffrey: *op. cit.* (1971) p. 25.

[19] P. S. de Laplace: *A Philosophical Essay on Probabilities* (1819) and W. Kneale: *Probability and Inductive Logic* (1949) respectively.

[20] R. von Mises: *Probability, Statistics and Truth,* 2nd English edition (1957); H. Reichenbach: *The Theory of Probability,* 2nd edition (1949); B. Russell: *Human Knowledge: Its Scope & Limits* (1948); W. C. Salmon: *Foundations of Scientific Inference* (1967).

waites's.[21] The defects of frequentism, however, have been argued enough elsewhere, in this connection amongst others by Kneale[22] and myself.[23] Incorrigible frequentists need not despair of T, though; there is more argument to come, which they may accept.

Are these explanatory probabilities chances? The probability of radioactive decay certainly is.[24] Here sufficient causes cannot be known because they do not exist. No more data on prospective causes would alter the relative probability. Determinism here is false; there is a real physical possibility of alternative outcomes. *p* undeniably displays a propensity; it is a feature of the world, not of our ignorance. Conception differs in that we can causally explain each male and female birth. Determinism need not be false (*pace* Mellor[25]); there need be no physical possibility of alternative outcomes. But unlike the lung cancer case (perhaps), we are not just ignorant of genetic causes; we also know a statistical law. That law says that something is common to these acts of conception, besides their causal differences, which probabilistically explains relative frequencies of male and female births. So I reckon male births still have a chance, display parental propensities.

Perhaps all explanatory probabilities are chances, perhaps not. All that matters here is that all chances are explanatory probabilities and the above argument applies to them. Conception may be 'a lottery with causal explanations [*inter alia*] as prizes',[26] but even lotteries can explain their more probable outcomes.

IV

Now for the case against T. On any conception of an explanandum's probability, the explanans must do more than raise it. The explanans must be true; and it must include something like what Carnap called the 'total available evidence'.[27] 'Evidence' is the wrong word, of course, betraying the confusion of explanation with confirmation and inference. This constraint on explanation therefore does not follow from the need for it in statistical inference.[28] But something like a 'total available data' requirement is nonetheless indisputable. Our explanans must incorporate any suitable propositions we know to be statistically relevant,[29] *i.e.* to affect the explanandum's relative probability. Yet, as Jeffrey and Salmon

[21] R. B. Braithwaite: *Scientific Explanation* (1953).
[22] *Op. cit.* § 33.
[23] In chapters 3 and 8 of D. H. Mellor: *The Matter of Chance* (1971).
[24] *Ibid.* pp. 97-100.
[25] *Ibid.* p. 153.
[26] R. C. Jeffrey: *op. cit.* (1971) p. 26.
[27] *Op. cit.* (1950) p. 211.
[28] See C. G. Hempel: 'Maximal specificity and lawlikeness in probabilistic explanation', *Philosophy of Science* 35 (1968) pp. 116-33.
[29] W. C. Salmon (ed.): *Statistical Explanation and Statistical Relevance* (1971) p. 11.

remark,[30] relevant data may lower p as easily as raise it. According to T, that should mean a weaker explanation; yet evidently it is not so. We cannot overlook Robin's not smoking just to keep up the explanatory probability of his cancer. What matters in probabilistic explanation, Jeffrey and Salmon conclude, is using all the relevant data available, however probable or improbable that makes the explanandum. So T is false.

What is wrong with this argument? As an argument for using all the suitable relevant data which are available, nothing. But the tenet T is not in fact incompatible with that, and must, I now argue, be added to it. Recall that what matters in explanation is the truth of the explanandum. Explanation really has non-linguistic objects, such as events or facts, although for analytic convenience we refer instead to propositions saying that the events occur or facts obtain. So explananda (like explanantia) must be true: otherwise there is no event or fact to explain, and there's an end. Now suppose one puts up a theory of explanation by listing features requisite for a successful explanans, including its requisite relations to its explanandum. If the features listed are indifferent to the explanandum's truth-value, the theory is inadequate. An 'explanans' that could as well relate to a false as to a true explanandum is no explanans at all. Nothing, therefore, explains A that would by the same tokens explain $\sim A$.[31] Call this adequacy test S.

Deductive explanation passes this test automatically: if A is deducible from true propositions, $\sim A$ is not. Jeffrey's and Salmon's accounts of probabilistic explanation fail the test. What is relevant to A, *i.e.* raises or lowers p, is *ipso facto* relevant to $\sim A$, *i.e.* lowers or raises $1\text{-}p$. The explanation of A is to contain all and only the suitable data which are relevant to A, and therefore all and only the suitable data relevant to $\sim A$. Thus it is also the explanation of $\sim A$. Jeffrey explicitly accepts this corollary of his view:

> To explain the phenomenon that there was at least one head in two tosses of a coin, I would point out that the process is stochastic with probability $\frac{1}{2}$ of heads on each toss, and with different tosses independent of each other. I would give the same explanation if matters turned out differently . . . the difference between the two cases would lie entirely with the gloss: in the first case one would point out that the probable happened, . . . in the second . . . the improbable happened. But the strength of the explanation would be the same in each case.[32]

Salmon also commits himself to this by tying explanation to making predictions 'concerning' events: 'To explain an event is to provide the

[30] In *ibid.* p. 24 and p. 63.

[31] See B. D. Ellis: 'Explanation and the logic of support', *This Journal* (1970) pp. 177-89, at p. 177.

[32] *Op. cit.* p. 27.

best possible grounds we could have for making predictions concerning it.'[33] That is, to explain A is to provide the best possible grounds for predicting whether A is true or false. But that is also to provide the best possible grounds for predicting whether $\sim A$ is true or false, and thus equally to explain $\sim A$.

V

This is a rather startling view of explanation. Smoking hardly seems to explain not getting cancer as well as getting it; still less does it seem to explain not getting it just *because* it would explain getting it. We may recall complaints at Freudians' supposed ability to explain any outcome of psychoanalysis; but at least they offer different explanations of success and failure, while Jeffrey and Salmon offer the same! They are not, of course, really more Freudian than Freud; they only say they can also explain the negation of whatever they can explain probabilistically, not that they can probabilistically explain everything. But the grounds of complaint are similar; here they are those of the test S.

Perhaps S fails for probabilistic explanation and it is, as Salmon says, 'a peculiar prejudice to maintain that only those events which are highly probable are capable of being explained'.[34] Take one of Jeffrey's cases, a sequence of ten coin tosses: 'It is possible, although highly unlikely, that there will be ten tails, and if this happens we shall know all there is to know about the why of it and the how, when we know that the process which yielded the ten tails is a random one and when we know the probabilistic law governing the process.'[35] Whatever the outcome of the ten tosses, that is the explanation of it. Can the conclusion be resisted, that it is equally good as an explanation of any such outcome? If not, test S and tenet T are both done for.

Take another case, a hand of bridge. An honest deal explains why each player does not get cards of just one suit. Would it equally have explained their doing so, had that much less probable outcome occurred? The same explanation, certainly, but would it really have been as good? Jeffrey and Salmon must say so.

Trading intuitive examples is not enough, of course; I need to explain Jeffrey's cases away (as he needs to do with mine). Note first that the sequence of ten coin tosses has a number of distinct if related features. It has the feature of being all tails; consequently, it also has the feature of being not all heads. These are distinct facts about the event, and they may have distinct explanations. Even if the prospective explanans is the same, it may not explain each fact alike. Since explananda are propositions (which Jeffrey and Salmon do not dispute), it must be elliptical to talk as we do of explaining events.[36] What we mean is, explaining

[33] *Op. cit.* (1971) p. 79.
[34] *Ibid.* p. 63.
[35] *Op. cit.* p. 24.
[36] *Op. cit.* (1975) pp. 150-1.

some fact about the event, usually the one we used to refer to it. Thus, to explain a death is to explain the fact of that event being, *inter alia*, a death. It may also have been a strangling, have taken less than twenty seconds, and have occurred at the full moon.[37] These three further facts about it may well call for three quite different explanations. The explanation of the strangling may be causal or intentional, of the duration probabilistic, and of the date non-existent.

So some facts about a sequence of coin tosses may well be explicable and others not. My arguments for S and T make me claim that Jeffrey's probabilistic explanation explains only those facts which it makes probable. The explanans explains why the ten tosses are not all heads, but not why they are all tails.

This claim is quite consistent with accepting that probabilistic explanation must use all the suitable relevant data which are available. But what is available may still not satisfy our original demand for explanation. Just because 'no further explanation can be required or can be given'[38] I need not infer in the teeth of S and T that what we have must *be* an explanation. To make Hell inescapable is not to make it Heaven.

I conclude that Jeffrey and Salmon have not made out their case against the tenet T. Its rejection is not required by their impeccable doctrines that probabilistic explanation is not inference and that it must use all the suitable relevant data which are available. On the contrary, adding T to their doctrines enables their account to pass the test S. For if a relevant datum B raises A's probability p when added to the explanans (or substituted therein for $\sim B$), it cannot also raise $1 - p$; and if all the available relevant data make p exceed $\frac{1}{2}$, $1 - p$ must be less than that. So either way what explains A does not explain $\sim A$.

VI

The case for T therefore stands. It is recommended by its own *prima facie* plausibility and by the arguments offered for it and for S. But why does it matter? I need to say something of what follows from T, to exorcise the suspicion that the dispute is trivial.

First, T promises to reconcile the accounts, divorced in both Hempel and Jeffrey, of causal and probabilistic explanation. It thus serves to further Salmon's own subsequent efforts in 1973 to provide a unified account of both. Causal explanation becomes a desirable extreme case of probabilistic explanation, subject no doubt to other constraints of its own.[39] The connection between causal and probabilistic explanation is quite opaque if T is false.[40]

[37] *cf.* p. 81 of D. Davidson: 'The Logical Form of action sentences' in *The Logic of Decision and Action*, ed. by N. Rescher (1967) pp. 81-95.
[38] Salmon: *op. cit.* (1971) p. 63.
[39] See Salmon: *op. cit.* (1973) and Mellor: *op. cit.* (1975).
[40] Cohen: *op. cit.*

The tenet T explains moreover why we go for causal explanation where we can get it, and elsewhere go for explanatory theories in whose terms what happens does so with the highest probability. There is thus, as Harman has remarked,[41] a close connection between the principle of inference to the best explanation (§II above) and the maximum likelihood principle,[42] *i.e.* the principle of inference to what makes the known facts most probable. As T both provides the best explanation of this connection and makes it most probable, the principles themselves unite in T's support.

Nancy Cartwright, in an unpublished paper, has put modal epicycles on the Jeffrey-Salmon system to cope with a case that T takes in its stride:

> The standard way to get rid of poison oak is to spray it with . . . *X*. *X* attacks the whole plant, and causes all of its leaves to drop off. Some plants, however, are not affected, and they lose no leaves at all. . . . *X* is (say) 80% effective: with regularity, 80% of poison oak plants treated with *X* lose their leaves. On this information we can explain that a particular plant lost its leaves by pointing out that it was sprayed with *X*. But . . . we cannot explain why a poison oak keeps its leaves by citing the spraying. Spraying can explain defoliation, but it cannot explain non-defoliation. The probable outcome is explained by the spraying but not the improbable.

That last sentence is not quite right: the situation would have been the same had *X* been only 40% effective. The case, in fact, is like smoking and cancer, and has been tacitly dealt with in §I. The pertinent tenet is reading (2) of T(i). Poison oak (K) is more likely to lose its leaves (A) when sprayed with X (B) than when not so sprayed ($\sim B$). That is why B helps to explain A but not $\sim A$. It does not take Cartwright's proliferation of possible world semantics for statistical and nomological operators to cope with *X* and the poison oak. A little T does it all.

Finally, T restores Hempel's plausible thesis that a good explanation is *ipso facto* a good basis for predicting the truth of its explanandum.[43] To have a good basis for a prediction is to have premises for a safe inference to it, which is no doubt what high probability on all the relevant data available provides. (Waiving the problem of induction, which would make all prediction guesswork, I take it that my explanatory probabilities must also be inductive. That inference, unlike the converse, is surely sound. If the measure of epistemic possibility does not also measure warranted expectation, I do not know what does.) But if a good explanans could as well give its explanandum a low as a high

[41] *Op. cit.* (1973) pp. 137-8.

[42] See pp. 68-75 of R. A. Fisher: *Statistical Methods and Scientific Inference*, 2nd edn. (1959).

[43] *Op. cit.* (1965) p. 367.

probability, then evidently the thesis would fail: a good explanation would not at all guarantee safe prediction. That is indeed the conclusion which Jeffrey (p. 23) rightly draws from his account. Salmon, oddly enough, wants to keep a symmetry thesis almost as a matter of definition, as we have seen in §IV: 'To explain an event is to provide the best possible grounds we could have for making predictions concerning it.' But this is not Hempel's thesis: the 'best possible grounds . . . for making predictions concerning' A might leave A with probability $\leqslant \frac{1}{2}$, *i.e.* might leave no grounds at all for predicting A in preference to $\sim A$.

Incidentally, Scriven's notorious counter-example[44] of paresis and syphilis merely reflects an ambiguity about prediction which corresponds exactly to the ambiguity of 'explanation' discussed in §I. To take our more homely example, smoking *prima facie* explains cancer without being a good basis for predicting it (since even smokers mostly die of something else first). So Hempel's thesis seems to be refuted. Not so in fact, since like has not been compared with like. Suppose that I want to know why Robin got cancer. In one sense, your telling me that he smoked a lot explains it, since that makes it much more probable than if he had not smoked. By the same token, we have that much better a basis for predicting cancer of Robin than of a non-smoker.

Of course Robin's cancer would have been better explained if its probability p had been raised more. If p in the end had exceeded $\frac{1}{2}$, *i.e.* exceeded $1 - p$, the explanation of A would have been better than that of $\sim A$; which seems a suitable point to start admitting that in the more fundamental sense it really is an explanation. By the same token, we would have a better basis for predicting A than for predicting $\sim A$; which seems an equally suitable point to start admitting that there really is a basis for predicting A.

So Hempel's thesis stands, albeit not for Hempel's reasons. It stands or falls with T; and T stands. Jeffrey, by the way, knows in his heart that this is so. As he says (p. 24), 'in the statistical case I find it strained to speak of knowledge *why* the outcome is such-and-such'; except in the 'beautiful, extreme cases' (p. 20) where the probability is extremely high. Quite so. All Jeffrey has to do, to see the light, is to see a little more beauty in only a little less extremity.[45]

University of Cambridge Received July 1976

[44] In M. Scriven: 'Explanation and prediction in evolutionary theory', *Science* 130 (1959) pp. 477-82.

[45] The first version of this paper was given, while I was a Visiting Fellow at the Australian National University from July to December 1975, to staff seminars at La Trobe, Monash, Macquarie and the Australian National Universities. A revised version was given, and replied to by Professor Jeffrey, at a History and Philosophy of Science Graduate Seminar at Cambridge in May 1976. I am indebted for detailed comment and criticism to those who took part in these seminars and especially to Professors Jeffrey and Salmon; and to many friends made at the Australian National University for making this and much other work so pleasant to do.