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ON THE LIMITS OF SCIENTIFIC EXPLANATION: HEMPEL AND EVANS-PRITCHARD

JOSEPH AGASSI*

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In recent years, Hempel has questioned the universal applicability of the deductive model of causal explanation, and suggested supplementing it with a probability model.' When we explain the fact that one child got the measles by the suggestion that he caught it from another child, we are not using the deductive model, he says, since catching measles is a matter of mere probability and not of strict causality: playing with an infected child is not a sufficient condition for infection.

So much for the core of Hempel's argument. Now, a child catches the measles, we assume, from another person; on this matter the theory we hold is strictly causal: it is a necessary condition for catching the measles, to be in contact with a person who has: caught it already. (The premise here is, "All measles cases are contact cases." Whether our premise is true or false is not under discussion; even though it is obviously false, since it breaks down on the question, how did measles appear in the first place?) The contact of one child with an infected child can appear in a causal explanation proper ─ but only to explain the possibility of the infection. When we ask why a child who had the measles infected his older brother but not his younger brother, then the deductive model fails, as Hempel asserts. (The premise here is, "The probability of catching measles by contact is m/n" where m/n is specified.) Within the deductive model any probability theory explains only distributions, not individual cases; on this matter surely Hempel is right. Thus far, only various interpretations of probability statements have been tried, and it is Hempel's merit that he has attempted to go beyond the interpretation of statements to the interpretation of their use in deductive explanation, from probability of events or of statements to the probability of causation and thus / through the identification of the causal and the deductive models — to probable deducibility.

Let me explain in detail, if I can. Consider, first, the deductive model of explanation briefly. We have universal laws, and initial conditions, from which the final conditions follow. The final conditions may be explained, predicted, or retrodicted. The case of retrodiction may lead to verbal clumsiness since in it the final conditions precede the initial conditions in time yet they are called "final" since they appear in the conclusion. The time sequence of the initial conditions and final conditions and the performance of the inference from the law and initial conditions can be one of the following six; there is no name for the last two on the list since their use is very uncommon though not unconceivable.

Earlier → later

I	1.	IC	FC	TI	Explanation, Postdiction
	2.	IC	TI	FC	Prediction, test
	3.	TI	IC	FC	Conditional prediction, advice, specification
II	4.	FC	IC	TI	Retrodiction
	5.	FC	TI	IC	
	6.	TI	FC	IC	

IC = Initial Conditions.
FC = Final Conditions.
TI = The time the inference is made from the universal law and the initial conditions, to the final conditions.

When the universal law is statistical and the initial and final conditions concern distributions, the case may easily fall under the deductive model of explanation proper. When the initial conditions are then interpreted as describing single cases, however, Popper sees here no explanation at all, for the reason indicated by Hempel: Popper identifies explanation (or understanding or throwing light) with both deductive explanation and causal explanation. One reason for this is the desire (already expressed in Russell's *Leibniz,* Ch. 1 §19 as well as in Wittgenstein's *Tractatus*, §§5.135, 5.1362, 6.32) to get rid of causality as a category above and beyond the category of universal law and causal explanation. The desire is not gratified by any inductive model; but it is gratified by the deductive model. Indeed, the deductive model has a very significant and non-trivial enrichment: it embraces as causal all deductive explanations universal laws, causal laws, causal explanations, and statistical explanations — and in addition to all traditional or semi-traditional senses, it includes as causal even all the various levels of explanations. In this manner, when the accent is on deduction, the statistical explanations are embraced by the deductive model independently of whether they are interpreted as laws of distributions or as laws of measures of possibilities, or as causal explanations of observed distributions. And this, surely, is noncausal in the old metaphysical sense of "causal" but causal in the sense of nomological-deductive. In all cases of statistical explanation the facts to be explained must concern populations, and not individual events, since we cannot deduce from a population to an individual case. Here is the very limitation of the deductive model, rooted in its very strength.

(When discussing the deductive model of explanation, then, we are not worried about the difference between a universal statement which may be merely contingently true and between one expressing a natural law or any kind of natural necessity. This may be unsatisfactory, and we shall discuss later on the unsatisfactoriness this may cause. Here, then, for the truth of A to be a sufficient condition for the truth of B, is the same as for if-A-then-B to be true. Likewise for the truth of A to be a necessary condition for the truth of B is the same as for if-B-then-A to be true. All this is utterly traditional, at least until the introduction of the discussion on subjunctive conditionality and law likeness. Already in 1935 Popper discussed law likeness; yet I shall ignore this here, at least for the time being, as a topic which at least seemingly conflicts with the line of thought congenial to the deductive model of explanation.)

I

Hempel has something very subtle to add to all this, concerning individual members of populations. First, in the sense of throwing light, explanation may be partial; if we throw light on an event by subsuming it under a universal law, we may be said to be throwing partial light on an event by relating it to an ensemble. Now the degrees of throwing light follow the calculus of probability; they offer yet a new interpretation of the formal calculus of probabilities, akin to, but not identical with, the propensity interpretation offered by Popper at about the same time.` Again we find here a feature successfully incorporated into the deductive model while attempts to incorporate it into the inductive model have persistently ended in failure. The feature is very well-known by the name of probable inference. It is very easy to show that no inductive model successfully incorporates probable infer6lce in any sense which may be said to generalize inference proper. The latest effort in this direction is perhaps Reichenbach's; perhaps it is Carnap's. I shall not dwell on the difficulties, philosophical or formal; let me merely state that in his new appendices to his The Logic of Scientific Discovery Popper has developed the formal calculus to a point where, in the logical interpretation, the calculus may yield probable inference as a generalization of deductive inference. If explanation is deductive and if deduction can be generalized, then explanation too can be generalized. And since the formal calculus yielding the generalization is the calculus of probability proper, it follows at once that the degree to which an event is explained by the help of a statistical law is the same as its relative frequency specified by that law; Hempel's new interpretation, as intended, does not quantitatively differ from the relative frequency interpretation. Hempel's novelty is just here. Ascribing a probability to a single event has to do with its relative frequency, or at least with its possible occurrence, usually to be understood as a prediction or a weighted possible prediction (with the weights being additive); this has already been attempted by Reichenbach. Similarly, declaring even a past event as probable may be understood in the same frequency interpretation — in the sense that it was highly predictable. But explaining that event with that probability, is — according to Hempel throwing light on its occurrence, namely saying, to an extent but not fully, why it has to occur, namely introducing a lame deduction. Thus, where the law is a distribution, the initial and final conditions singular, since there is no deduction proper, Popper uses the relative frequency interpretation of probability and sees there no explanation at all; but Hempel uses the logical interpretation of probability and sees there a probability explanation of the final conditions. Can we say that in this brilliantly easy way Hempel removes some limitation on scientific explanation by adding probability causes to strict causes? In what follows I wish to explain my affirmative answer.

To nail things down clearly first, let me introduce a terminological distinction to which I shall systematically adhere. "The probable cause of x" is a standard singular expression, when x is an event, or rather to be precise, a singular statement of an event, used to denote what is probably the strict cause of x; "the probability cause of x," however, will denote what is (putatively or hypothetically, but) definitely the cause of x, though not the strict cause of it: it is definitely what has brought x about, but things could (improbably) have happened otherwise. To take an intuitive example, when we say of Tom's measles that it is the probable cause of Dick's, we mean, perhaps it was Harry's measles but we do not think so; when we say the probability cause, etc., we mean it definitely was Tom's but the cause might have failed to produce the effect.

To be precise, genuinely singular statements occur in history (human or not) and in astronomy, but seldom elsewhere. The explanatory model with a universal and singular statement as premises is really a model: the singular statement is singular in virtue of its space-time co-ordinates which are allegedly singular; but science usually handles only repeatable experience. Hence the space-time co-ordinates are not strictly singular, nor strictly universal: they are parameters. When we say "at to . . ." in the minor premise and "at t, . . ." in the conclusion, we have two possible readings. First, t, is a definite moment of time, and we have here a strict causal explanation of one singular event or statement, of event. Second, "t„" stands for any arbitrary moment, and we have a matrix of a special kind of explanation, which Popper calls "model" (after the Cartesian idea of models in explanation in physics). In a model we can insert a member of a variety of instances of to and of t_l; we really mean, then, "for any t„. at to . . .," etc. Once we press this point further to probability explanation, we immediately have an intriguing case which is very subtle indeed. We have not just Tom who got the measles, but any Tom, Dick, or Harry who did. And the probability explanation of their measles is only a probability because our premises tell us distinctly and unmistakably, that for every m infants infected under conditions c_1 there are n–m under the same conditions c_1 not so infected. Here m/n is the asserted probability of infection under conditions c_l (c_l for contagion). And so both Tom's infection and the m cases of infection, equally, are explained only partially to the degree m/n.

This point deserves stressing, since its subtlety, first drawn by Hempel, is easily overlooked. A model may be viewed as an explanation of a generalization plus a higher-level law; and when a model, seen that way, is applied to a probable case we tend to view it as the explanation of one distribution by another plus a statistical law. And this, as we have observed, is a case already satisfactorily handled. This looks very clear and straight-forward, but Hempel's subtlety will bring out a few difficulties here. Take any ensemble of infected people; it is as respectable as an ensemble of people immunized to the illness. The immunity is causally explained. The infection, too, is in one sense causally explained: each victim has become a carrier of the cause of the disease, a victim of a condition c_2 (c for carrier).

But in a sense the infection is only partially explained: condition c_2 (carrier) fully explains, and condition c_1 (contagion) only partially explains the illness. And yet we are handling the same phenomenon, even the same ensemble!

II

The limitations to scientific explanation were studied in a different context by the social anthropologist, E. E. Evans –Pritchard. He stressed the fact that scientific explanation is significantly limited, and precisely in a manner complemented by magical explanation. When we explain any phenomenon, Evans-Pritchard argues, we assume certain initial conditions which we do not explain. A brick kills a passer-by; we assume a few laws, and initial conditions concerning both the brick and the passer-by; the coincidence is explained by another coincidence; the first coincidence, the death of the passer-by, is explained (with the aid of laws) by another, rather meaningless coincidence, of two events that seem utterly unconnected, and the causal origins of which need not even be simultaneous. Magic, however, links the initial conditions by some meaningful and co-ordinating event: the malicious intent of a magician is just what has directed the brick and the head of the passer-by into their collision courses.

Evans-Pritchard implicitly accepts the deductive model of explanation, and as the only model. He insists that magic handles that part of the phenomena that the deductive model of explanation leaves unexplained ─ namely coincidence. He does not defend magic, of course, but he does show that magic and science are not necessarily in conflict: the limitation on scientific explanation leaves (logical) room for magic. This is all I wish to borrow from Evans-Pritchard for present note.

Science and magic do, of course, clash no less than science and a rather primitive science or a rather primitive common sense do. In a tribal society one calls a witch-doctor just as in our society one calls a qualified doctor, whatever this means. In many societies both alternatives exist and clash with each other. Theories endorsed by cultures governed by magic include universal statements that science rejects; and some of these universal statements are unmistakably magical ─ some of them may be statistical in a sense, some of them even strictly causal. And, no doubt, Evans-Pritchard will join other social anthropologists and defend belief in the universal statements of magic against the charge that the magic-minded are primitive. But he will do that in another way, discussed in a recent paper by Jarvie and myself. So I shall not discuss this case here, and confine my discussion to the case specific to Evans-Pritchard, and to the argument he has invented to handle this case, one based on the incompleteness of scientific explanation.

It is possible to treat Evans-Pritchard's case with the new move proposed by Hempel. Hence, should we accept Hempel's proposal, it would seems finally possible to close the gap left by science (and occupied by magic). We may treat all initial conditions statistically: we can study ensembles of bricks on rooftops and their chances of falling to the street below; we can study the probability of pedestrians passing by any street; we can from these two probabilities compute by the most elementary laws of chance what is the chance of a brick falling on a head of a passer-by. Thus, Evans-Pritchard's inexplicable phenomena (or more precisely, classes of phenomena) may be easily converted to Hempel's partly explicable (classes of) phenomena.

There may be a quantitative difference here, of course. We assume that the chance is high of catching the measles from a playmate whereas the chance is low of bumping into a falling brick while walking along. So we might view the first explanation as more satisfying than the second. I very much doubt whether Hempel would endorse this; rather, I suppose, he will agree that the logic of these two cases is the same and that nothing else matters in the present context (of the logic of explanation). To be sure, there is an intuitive difficulty here. If we explain measles to the degree p, we may explain non-measles of a similar case to the degree (1— p); think again of the child who has infected only one of his two brothers. We shall return to this point at the end of the present note.

This is one difficulty in converting the argument from Evans-Pritchard to Hempel. The other is more obvious: the initial conditions in the magical explanation are humanly meaningful — charged with good or bad intentions — whereas in the probability explanation all intention is excluded *a priori*. One can easily circumvent this point, but since it is hardly related to the logic of explanation, we may overlook it here. For, from a logical point of view, intention is as much an initial condition assumed in the magical explanation as any other alleged cause is in its related explanation.

In any case, Evans-Pritchard was not concerned with convincing us about either the truth or the validity of magical explanation. Rather, his claim is that magical explanation fits a logical vacuum in scientific explanation ─ the same logical vacuum that concerned Hempel.

The logical vacuum, one may stress, is not eh product of probability; rather, it is a logical vacuum within the deductive model; Hempel has mobilized probability theory to fill it. To see this we have to survey first the cases of dissatisfaction which the deductive model gives rise to. Take any case of the deductive model. Consider the following question: why is the law it uses what it is? This question may be answered by the use of higher-level laws and the deductive model. Can this go on indefinitely? Plato and Aristotle have suggested, and many a modern philosopher has concurred, that, when the final laws of nature are known, their rationale is known too and this then leaves no room for the question, why they are what they are. Other philosophers, particularly Kant and Einstein, suggested that the final law will remain a mystery, that the greatest mystery is that nature is law abiding. In any case, as long as the final law is not available, dissatisfaction with any given law need not be utterly frustrated, as it may stimulate the search for some deeper laws.

III

So much for the dissatisfaction with the laws of nature. Can we view the initial conditions the same way? Some initial conditions indeed are explicable by other initial conditions and certain laws; indeed, in some cases, such as the Newtonian two-body case, all complete sets of compatible conditions of one case are equivalent: given one complete set of conditions of a system: the set of the position and momentum of each body in the system at any given moment of time suffices to deduce all other such sets, past and future. And yet, particularly in such a case, the dissatisfaction is not alleviated: our understanding of the case is not deepened, the whole set of conditions associated with one system may well have been different without the laws of nature suffering any change; certain arbitrariness hides here, a certain accidental character of the system. This accidental character, however, may perhaps be explained with the aid of a different application of the deductive model, the application of the model of probability explanation.

This idea, too, is not new. Though the deductive model has not been explicitly and fully treated in the classical literature, and though its extension is even less satisfactorily treated there, it is clear that the deductive model is traditional, well understood, say, by Galileo and Descartes, for example. We can see already Kant adumbrating the ideas just outlined here in more. In the preface to his *Universal Natural History and Theory of the Heavens*, he explains his motive in developing his cosmogony as an attempt to use natural laws alone, namely, to free the explanation of the present state of the solar system of the need to assume some initial conditions ─ as these are necessarily arbitrary ─ by showing that the present state of the solar system is independent of any set of initial conditions, by showing that every set of ancient initial conditions leads to the present state of the solar system, by showing that the set of chaotic initial conditions leads to the present state of the solar system. I may be reading too much into Kant, or perhaps I delineate his ideas more sharply than permissible to a historian. I cannot say.

All this is just a somewhat sophisticated way of expressing the initial and intuitive dissatisfaction with the meaninglessness of the explanation of one event by another, remote and no more comprehensible. The magic-minded explanation of an accident not by another accident but by some evil or good intent intuitively does away with this. The intuition is mistaken. If the evil intent is generated by the victim of the accident, then the accident is explained in a fashion no more satisfactory than the Newtonian case. If not, the contact between evil-doer and victim is accidental. In any case, the present discussion is not a defense of the magical mode of thinking. The first point to notice is, simply, that the magical explanation of the type discussed by Evans-Pritchard comes to fill — however unsuccessfully and even unsatisfactorily — exactly the gap that Hempel declares in the beginning of his discussion to be unfilled.

To repeat: are Hempel and Evans-Pritchard offering competing ways of filling the same gap? Assume, to begin with, that both Evans-Pritchard and Hempel are right. It is all too easy and all too unprofitable to reconcile two seemingly conflicting views and declare both true by viewing them as related to different topics, by agreeing never to apply them both to one and the same case. Let us rather try, then, to reconcile the views of Hempel and Evans-Pritchard as applied to the same case. That Hempel and Evans-Pritchard have much in common is all too obvious: they both stress that traditionally scientific explanation was viewed as strictly deductive-at least in intention, emphasis, paradigm, etc. Evans-Pritchard adds that this mode is so obviously defective, that pre-scientific modes of explanation may complement it; Hempel adds that statistical explanations, though not in the least new, have become sufficiently significant in recent years to permit a shift of paradigm so as to generalize the concept of explanation and include probability explanation as a species next to deductive explanation.

There is one strong attraction in this suggestion. It is a traditional part of the theory of deductive (causal) explanation to view prediction and retrodiction in exactly the same light. Indeed, this is the gist of Laplace's point in his preface to his *Essay on Probability*, where he introduces his demon who both predicts and retrodicts on the basis of all the laws of nature and one complete set of simultaneous initial conditions. But, Laplace admits, we are not such demons and so we use probabilities. We may, though he ignores the analogy, both predict and retrodict with probabilities; we may say your parents came from

Lithuania with probability such and such, and we may say your children will settle in Israel with probability such and such. We may use here probable prediction and probable retrodiction in exactly the same interpretation of probability, whether as relative frequency, a measure of weight of possibility, or perhaps other. The stress here is not on what sense of probability we use but on symmetry between prediction and retrodiction, or more generally, between the initial conditions preceding the final conditions and the initial condition succeeding the final conditions, in perfect analogy to the symmetry that Laplace declared to exist in strictly causal cases.

But this symmetry may well be worth careful examination (for the sake of the present discussion and for other studies, such as the reduction of the wave packet in quantum theory). To begin with, the symmetry of the previous paragraph is not in accord with Hempel's example, since in it we have no retrodiction but only explanation, not from initial conditions to prior final conditions, but the reverse. A retrodiction would be a conclusion from a child's having the measles today to the likelihood of his playmates of yesterday having had the measles yesterday. We have two analogies here that may easily be confused. First, take the case where the initial conditions precede the final conditions in time. If the final conditions are in the past of the deduction, then it is an explanation of the final conditions; otherwise it is a prediction of them. Here is a perfect symmetry between explanation and prediction, and rendering the causal explanation statistical need not matter. Second, take the case where the final conditions antecede the initial conditions. This may be a causal explanation and a retrodiction, depending on whether the deduced final conditions are known or not. For example, Kepler wanted to find out whether a (total) solar eclipse happened on the Judean hills on the original Good Friday. This is retrodiction. He made it as a possible explanation of the story of the occurrence of darkness at noon that day. He refuted that explanation. The retrodicted statement, "there was a total eclipse on the original Good Friday on the Judean Hills" will deductively explain ─ with the aid of certain uncontested additional claims ─ a certain assertion made in the Gospels. Again, the symmetry between explanation and retrodiction is here complete, and again one may apply probabilities here as well.

Of course, it is no more than logical exercise to render all retrodictions into predictions. But this move is highly suspect, and for a very obvious reason. It is easy to render a prediction about things and events to a prediction about observers and observations. Indeed, there are two reasons for doing so, whose value is under dispute. One is epistemological, one psychological, and both highly positivist. Epistemologically a positivist may wish science to sum up our knowledge of phenomena, past and future, not make ontological commitments concerning the existence and nature of things. Psychologically, a positivist may wish to consider knowledge as a behavior pattern, actual or dispositional. Now, suppose we reduce both prediction and retrodiction to predictions on observers; then we may distinguish between prediction-prediction and retrodiction-prediction. If, however, we convert only retrodiction to predictions on observers, we may find this *ad hoc*, at least until we can elicit some good reason for it. No doubt, in order to render retrodictions testable we must use them for predictions about observers; the same, however, is true of predictions; yet, neither retrodictions nor predictions are reduced to predictions on observers: predictions are about future events, retrodictions are about past events, and rendering either testable requires additional (uncontested) statements about observers.

IV

Somewhere something has gone wrong. For a purely deterministic world, say, a world consisting of the two-body Newtonian case, we may perhaps assume intuitively, prediction and retrodiction are symmetrical (assuming uniqueness of the solutions to our equations, etc.) even if our equations were not indifferent to the arrow of time (i.e., even if our equations are not invariant to the transformation of the time co-ordinate to its reverse). In a deterministic world cause and effect are perhaps interchangeable; causality, intuitively speaking, may be a matter of both necessary and sufficient causes. Indeed, Aristotle's theory of causation is just this, and one may show that in the nineteenth century causation was still so understood. Sir John Herschel, in his *Preliminary Discourse to the Study of Natural Philosophy* of 1830 declared causality as necessary and sufficient causes. Faraday proves his specific theories of causation regularly by the empirical claims that when the causes appear so do the effects and when the causes are removed the effects likewise disappear. And it is obvious that here the strict analogy between strict cause and probability cause breaks down, for all singular conclusions, in either of the two senses described above. Indeed, this is so obvious that only confusion between the tame probable cause and the more problematic probability cause — could lead to oversight on this point.

A difficulty lurks here even regarding strict causes: we do have an intuitive concept of a sufficient but not necessary strict cause: bleeding is a sufficient but not necessary cause of death; similarly, we have an intuitive concept of necessary but not sufficient cause — both strict and probability. It is indeed this non-symmetry between strict and probability causes that prevents the perfect analogy!

Little reflection will show that it is the intuitive idea of necessary but not sufficient probability cause that enables Evans-Pritchard to stick to his point even after Hempel had made his. Even if we do agree that for an event to be possible or probable, certain phenomena must first occur, one still may raise the question, why did the probability become an actuality here and not there rather than the other way around? This question has been raised before, and has led, for example, Robert Leslie Ellis, Karl Popper, as well as William Dray and Michael Scriven to deny the possibility of probability explanation of single events. And, indeed, here there is a lacuna — the distance between the probability and the strict cause — that the magic-minded may explain by positing some unobserved intent, malicious or benevolent as the case may be. The logic of Hempel's probability model is the same for the occurrence and non-occurrence of the probability property — say, contagion of measles among members of the ensemble — of all those under the condition c_l that leads to contagion with the probability m/n. Thus we may explain albinos (single or groups) and non-albinos (again, single or groups) alike by the theory of distribution of albinos in the population at large or in the population of descendents from albinos, or in the Albino Family Robinson. Here deductive explanation, causal explanation, and throwing light, are all blocked, and in the same manner; the determinists see the blockage as temporary and they are all too ready to work with imperfect explanations. The indeterminists see here an objective block and will find it harder to accept lame deduction as deduction in any legitimate sense. They will simply reject defective deduction as no deduction at all. But when all is said and done, when the determinist program is entirely carried out, the problem may be raised all over again as we have noted — introduce probability explanation, and already. We may then— or earlier — introduce probability explanation and find it only partially successful.

The question, then, is what is the gain from calling a statistical explanation of a distribution the probability explanation of an individual member of the ensemble? The answer seems to be, that the necessary conditions for the effect are described. This is not so, since in the beginning of this note (second paragraph) it has been stressed that the necessary conditions (being infected) for the effect (measles) is explicable well within the deductive model of explanation. We may deduce that the sick child has been in contact with an infected child, and even say when, within well-specifiable boundaries, relying on the strictly causal theory of the necessary incubation period of the specific disease. Here we see another analogy between moving forward and backward in time: with strict causality, it seems, we have sufficient conditions moving forward in time and necessary conditions going backward; with probability causes it is the other way around, it looks. It all seems to be in need of clarification and elaboration. Whether one calls this symmetry or anti-symmetry I would not know.

(It will be noticed, I hope, that in the manner indicated here, all claims made within science are interpreted as claims for the highest degree of objectivity and enlightenment, yet with no justification of any kind. I clearly advocate the rejection of both the subjectivist interpretations and interpretations which accept objectivist claims to the extent that these might — allegedly — be justified. And so I also reject the claim that some universals may be distinguished from others as more law like, and hence justified. All universals used are claimed to be law like and thus enlightening, and we may question this and wish to explain some universals, whether as law like or as mere accidents, or even as sheer approximations. Similarly, we may claim that a statistical explanation is enlightening as a probability explanation and we may raise doubts about this as well. All this depends on the background metaphysical framework.)

In the meantime, we may conclude, perhaps, that what seriously matters in Hempel's discussion is still the strictly deductive; the rest may indeed be accepted, but seems to be much a matter of taste. Considering explanation as understanding or as enlightenment, and Hempel's probability explanation as partial understanding or enlightenment, I do tend to endorse his taste, especially since, quite counter-intuitively, it tallies very well with Evans-Pritchard's theory by showing exactly the lacuna in scientific explanation which magical explanation attempts (unsuccessfully) to fill. Yet the matter has to remain a matter of taste: as the scientific tradition since the scientific revolution has it, it even observation reports are not admissible unless they are reported at least twice by independent witnesses and come with the claim of repeatability (for whatever reason).

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