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COMMENTS ON 'HEMPEL'S AMBIGUITY' BY J. ALBERTO COFFA

ABSTRACT. Using Coffa's paper as a point of departure, this brief note is designed to show that Hempel's inductive-statistical model of explanation implicitly construes explanations of that type as defective deductive-nomological explanations, with the consequence that there is no such thing as genuine inductive-statistical explanation according to Hempel's account. This result suggests a possible implicit commitment to determinism behind Hempel's theory of scientific explanation.

As a result of a subtle and penetrating analysis of Hempel's account, Coffa has shown, quite correctly I believe, that God would be unable to construct an inductive-statistical explanation of any physical event. What might seem at first glance to be a limitation on God's power (Do you mean to tell me that even God could not devise a genuine inductive-statistical explanation?!?) turns out to be a reflection of His omniscience. As Hempel characterizes inductive-statistical explanations, lack of perfect knowledge is a necessary condition for their use. Coffa seems to me to have established this point quite convincingly. If God were to set about trying to explain any event using the inductive-statistical pattern, He would do such a good job that the explanation would turn out to be deductive-nomological instead. For us humans, in contrast to the Deity, probability is the very guide of life.[1] Some events must be explained inductive-statistically if they are to be explained *by us* at all. The reason, of course is human ignorance. For God, all explanations are deductive-nomological.

Hempel's God is a well-known Deity; He is Laplace's Demon. Knowing all of the laws of nature, and any suitable set of initial conditions, He can deduce any actual occurrence whatever. Laplace's Demon can obviously explain everything deductive-nomologically. He has no use for inductive-statistical explanation; why should He be satisfied with high probability when He already has deductive certainty?

Coffa has put his finger upon a crucial point, yet one that is so simple that it almost seems silly. It is, nevertheless, absolutely serious. On Hempel's account, we may have genuine deductive-nomological explanations, and we may have well-confirmed deductive-nomological explana-

Synthese **28** (1974) 165–169. *All Rights Reserved*

tions. Because the former is possible, we understand what sort of thing the latter is. In brief, a well-confirmed deductive-nomological explanation is one which we have good reason to regard as a genuine deductive-nomological explanation. But if inductive-statistical explanation is necessarily relativized to the knowledge situation, then there is no such thing as genuine inductive-statistical explanation per se. To what, then, is a 'well-confirmed' or 'epistemically sound' inductive-statistical explanation supposed to conform? Not to a genuine inductive-statistical explanation, for there is no such thing. The answer, I think, must be that an inductive-statistical explanation is an approximation to a deductive-nomological explanation, and the goodness of an inductive-statistical explanation is measured by the degree of approximation to the deductive-nomological ideal. That, I take it, is what the degree of inductive probability attaching to the relation of explanans to explanandum signifies. (Inductive probabilities have sometimes been characterized as partial entailments!) The high probability requirement imposed by Hempel thus translates into a requirement that the approximation to the deductive-nomological pattern be as close as possible. Why is inductive-statistical explanation relativized to a knowledge situation? Because the degree of approximation to deductive-nomological explanation attainable is a function of the available store of knowledge.

An acceptable inductive-statistical explanation is not like a well-confirmed deductive nomological explanation, for there is no such thing as a genuine inductive-statistical explanation (not relativized to a knowledge situation) to which it might conform. I see something moving in the bushes. I look, and believe it to be a cactus wren. I listen, and hear the call of a cactus wren coming from that direction. I take up my binoculars and look closely. It appears, upon close inspection, to be a cactus wren. It is well-confirmed cactus wren. I believe, on the basis of good evidence, that there is a real cactus wren in the bushes. That's the way it is with deductive-nomological explanation, but not with inductive-statistical explanation. I have a dog which is a pretty good show collie – occasionally it even wins a blue ribbon. But it is not an ideal collie; it lacks some of the characteristics of a perfect collie. I can go out and buy other collies, some of which might come even closer to the ideal, but I cannot buy the perfect collie. I can even try to breed better collies, but no bitch can give birth to the platonic ideal because the platonic ideal is not a dog. An inductive-statistical explanation is something like an enthymeme – indeed, perhaps it is literally an enthymeme. We can supply missing premises to an enthymeme, making it a stronger and stronger argument. But we cannot make it into a valid enthymeme, because once it becomes valid it is no longer an enthymeme. We can make better and better inductive-statistical explanations, but we cannot make a genuine inductive-statistical explanation (objective, not relativized to the knowledge situation). When something becomes a genuine explanation in its own right, it is no longer inductive-statistical, but has been transformed into a deductive-nomological explanation. An inductive-statistical explanation, in short, is merely an imperfect deductive-nomological explanation. Of course, not all defective deductive-nomological explanations are acceptable inductive-statistical explanations; not every fallacious deduction is an acceptable induction.[2]

But why, Coffa asks, is there no such thing as an inductive-statistical explanation in its own right? The answer is that there are no such things as objectively homogeneous reference classes. Actually, as Coffa is fully aware, that is not quite right. There are no homogeneous reference classes *except* in those cases in which *either* every member of the reference class has the attribute in question *or else* no member of the reference class has the attribute in question. If all *A* are *B* or no *A* are *B*, then *A* is unquestionably a homogeneous reference class for *B*. It is not merely thought to be homogeneous; it is objectively homogeneous, for there is no way in which a relevant partition can be made in *A* with respect to the occurrence of *B*. The interesting question, however, is whether under any other circumstances *A* can be homogeneous with respect of *B* – e.g., if one-half of all *A* are *B*.

The foregoing question, I have argued, is tantamount to the question of determinism. Laplace's Demon, let us recall, is the God of determinism. Some people maintain, often on a priori grounds, that *A* is homogeneous (not merely practically or epistemically homogeneous) for *B* only if all *A*'s are *B* or no *A*'s are *B*; such people are determinists. They hold that causal factors always determine which *A*'s are *B* and which *A*'s are not *B*; these causal factors can, in principle, be discovered and used to construct a place selection for making a statistically relevant partition of *A*. I do not believe in this particular form of determinism. It seems to me that there are cases in which *A* is a homogeneous reference class for *B* even though not all *A*'s are *B*. In a sample of radioactive material a certain percentage of the atoms disintegrate in a given length of time; no place selection can give us a partition of the atoms for which the frequency of disintegration differs from that in the whole sample. A beam of electrons is shot at a potential barrier and some pass through while others are reflected; no place selection will enable us to make a statistically relevant partition in the class of electrons in the beam. A beam of silver atoms is sent through a strongly inhomogeneous magnetic field (Stern-Gerlach experiment); some atoms are deflected upward and some are deflected downward, but there is no way of partitioning the beam in a statistically relevant manner. [Lucretius vindicatus?] Some theorists maintain, of course, that further investigation will yield information that will enable us to make statistically relevant partitions in these cases, but this is, at present, no more than a declaration of faith in determinism. Whatever the final resolution of the controversy, the homogeneity of *A* for *B* does not logically entail that all *A*'s are *B*. The truth or falsity of determinism cannot be settled a priori.[3]

In my discussions of statistical explanation, as exemplified in the preceding quotation, I have repeatedly stated that I do not believe determinism is necessarily true. I do not think determinism is true. But if it is true, that is a fact of nature, not a necessary a priori truth. It seems to me, therefore, that we should construct an account of scientific explanation that is consistent with the possibility that determinism is false – i.e., our characterization of scientific explanation should *not* entail the truth of determinism.

Hempel, in contrast, appears to have made a tacit a priori commitment to determinism; otherwise he could admit the existence of objectively homogeneous reference classes which would serve as a basis for genuine inductive-statistical explanations. The impossibility of genuine inductive-statistical explanations involved in the claim that inductive-statistical explanation *must be* relativized to the knowledge situation thus seems to yield a commitment to determinism.[4]

I am inclined to think that some of the mischief could be avoided by refusing to think of inductive-statistical explanation as somehow derivative from deductive-nomological explanation. If we begin with an account of statistical explanation – I purposely omit the qualifier 'inductive'[5] – that can stand on its own two feet, and perhaps regard deductive-nomological explanation as a special or limiting case of statistical explanation, a very different picture may emerge. Objective homogeneity and statistical relevance relations become the focus of attention, and the high probabilities demanded by the effort to make induction an approximation to deduction vanish from the scene. When deduction is moved from center stage, determinism has a much harder time getting into the act.

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NOTES

[1] Bishop Butler's famous aphorism.
[2] See my *Foundations of Scientific Inference* (Pittsburgh, Univ. of Pittsburgh Press, 1967), pp. 109–111, for a discussion of the 'almost deduction' theory of induction.
[3] Wesley C. Salmon *et al.*, *Statistical Explanation and Statistical Relevance* (Pittsburgh, Univ. of Pittsburgh Press, 1971), pp. 45–46.
[4] I am aware that Hempel has not argued his case for the necessary relativization of inductive-statistical explanation to a knowledge situation on grounds of determinism. I am only trying to suggest the implications (presuppositions?) of the view that genuinely homogeneous reference classes exist only in the cases of *all* or *none*.
[5] See *Statistical Explanation and Statistical Relevance*, p. 11, for discussion of the reasons for this omission.