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PROBABILITY AS A THEORETICAL CONCEPT*

I

The well known difficulties in plausibly identifying objective probabilities with relative frequencies in a simple-minded way have led to several proposed alternative accounts. Subjectivists as tough-minded as de Finetti simply deny the very sense of the notion of objective probabilities, reserving probability as a measure of partial belief. Dispositional theorists, each in his own idiosyncratic manner, take probabilities to be propensities, usually defined in terms of some appropriate subjunctive conditional.

Another approach, sometimes assimilated to that of the 'dispositionalists' but better distinguished from it, takes attributions of objective probabilities to be 'theoretical assertions.' Here the argument is that the difficulties with the naive frequentist approaches are to be seen as problems on a par with those encountered by naive operationalist programs in physics. The frequentist attempts to give an explicit definition of probability in terms of frequencies relative to some appropriate reference class, just as, it is claimed, the operationalist attempts to construct explicit definitions of theoretical terms in science in terms of a purely observational vocabulary.

But, this approach continues, such a naive operationalism is now generally eschewed in the analysis of theories in science. Instead, it is alleged, we now realize that theoretical terms receive their meaning from the role they play in a total theory. While the theory as a whole functions to establish observable correlations, it is naive to expect that a term-wise reduction of the theoretical to the observational vocabulary can be expected. Instead, the best we can hope to do is to display the theory as a whole and simply see the place played in it by each of the theoretical terms. Such a holistic examination of the role played by the theoretical terms in the overall theory is the most that can be expected in the way of a 'meaning analysis' of the theoretical concept.

The parallel approach in an attempt to analyse the meaning of objectivistic probability assertions takes propositions about relative frequencies (usually in well-defined finite reference classes) as an 'observation basis' over which a

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theory is to be constructed. The theory will contain assertions about 'probabilities.' But these are taken to be assertions at the theoretical level. No definitional reduction of the concept of probability to that of relative frequency (or 'long run' relative frequency in ordered reference sequences, etc.) is claimed. Nor, it is alleged, need such a reductivist definition be produced in order to legitimatize the use of the concept of probability at the theoretical level. Just as the physicist legitimately invokes the concept of an electron in a theory designed to predict and explain observable phenomena like the clicks of Geiger counters or the visible paths in cloud chambers, not pretending to be able to offer an 'observational' definition of 'electron,' so the probability theorist invokes the concept of probability and the theoretical assertions about probabilities (statistical generalizations) in a theory whose aim is the explanation and prediction of relative frequencies, but without any allegation that the meaning of the probability assertions can be reduced to assertions about relative frequencies without loss of content.[1]

In this paper I will sketch out in very general terms what such a theory might look like. I will not pursue the internal structure of such an approach in any detail, however. For what I hope to show is that any such approach must face up to certain deep difficulties, difficulties which it encounters not because of its specific program as a theory of probability, but simply because of its programmatic assumptions about the meaning of theoretical assertions in general.

II

A statistical assertion is taken as having its meaning fixed by the role it plays in theory. And what does the remainder of the theory consist in? Primarily, it must contain 'upward' and 'downward' rules of inference. If the 'data' relative to which statistical assertions are made consists of known relative frequencies, what is needed to give the statistical assertion a meaningful role are two rules (or, perhaps, sets of rules): those which tell us what the legitimate inferences are from known frequencies to statistical assertions and those which tell us what the legitimate inferences are from statistical assertions to newly inferred or predicted relative frequencies. While classical statistical theory has focussed on the former types of rules, insightful statisticians and philosophers have realized that the status and nature of the 'downward' rules are crucial as well.[2]

But what *justifies* adopting one such rule of inference as opposed to another? What are the kinds of reasons we can offer for or against adopting one such rule as opposed to any of its alternatives? What is crucial for the moment is simply the realization that this has traditionally been thought to be an important, and indeed very difficult, question. But from the present point of view, statistical assertions as having their meaning fixed by the role they play in the total theory in which they appear, is there any question here at all? The answer requires some care.

If the meaning of the statistical assertion is given only by its place in the total theory, and if the inference rules adopted fix what the theory is, how could the inference rules possibly be unjustified? Fixing, as they do, the meaning of the assertion inferred to and inferred from, do they not constitute *definitions* (Reichenbachian coordinative definitions) of the theoretical assertion? In Dummett's perspicuous terminology, there is no independent meaning of the statistical assertion to which the rules of inference must "be responsible."[3] Hence there is no open and difficult question of rationalizing or justifying adopting one such rule as opposed to the other. It begins to appear as though one simply could not be wrong in adopting, say, a particular rule of upward statistical inference, for whichever rule one adopts fixes the meaning of the statistical assertion to which one is inferring. Apparent disagreement about an upward rule, then, is only apparent disagreement, for those who adopt incompatible rules are simply meaning different things by the statistical assertion, and the apparent incompatibility is due only to semantic equivocation.

Of course, the situation isn't quite that simple. A joint adoption of both upward *and* downward rules is not arbitrary in this way. For such a joint adoption commits one to inferences to unknown relative frequencies from known relative frequencies. Since the meaning of the relative frequency assertions is totally independent of the joint inference rules adopted, one can still ask, without being forced to accept a trivial answer, what justifies such an adoption of *joint* principles as opposed to a posited, contending, incompatible set. How to answer that question is, of course, since Hume, one of the outstanding crucial and deep questions of philosophy. For what it amounts to is a description of a set of inductive rules and a justification of their adoption.

As usual, the point made about the arbitrariness of any upward (resp. downward) rule considered independently of the adopted downward (resp.

upward) rule can be pointed out by noting the existence of dualities. This is exactly on a par with the familiar allegation that geometry and physics are 'dual' in the sense that in the light of any data whatsoever a particular geometry can be maintained if one is willing to make sufficient changes elsewhere in one's total theory (Reichenbach's 'universal forces') and with Quine's well-known claim of the 'underdetermination' of radical translation, where specific translations of features of a language can be maintained in the light of any behavioral response by simply translating other aspects of the language in a fashion designed to hold constant the predictive consequences of the translation with respect to overt verbal behavior.[4]

Suppose *A* adopts a particular upward and downward rule of inference (simplifying by assuming that there is only one rule of each kind). *B* adopts an incompatible upward rule, inferring, in the light of identical facts about known relative frequencies, to different, incompatible, statistical generalizations. But *B*'s downward rule differs from *A*'s downward rule as well, the net effect being that *A* and *B* infer the same results about unknown relative frequencies on the basis of known. If the only assertions whose meaning is established independently of the rules of inference adopted are the assertions about relative frequencies, and if the only 'responsibility' rules bear is to correctly (justifiably) take one from known to unknown relative frequencies, then what is there to choose between the two posited sets of inference rules? Indeed, why not say, with Reichenbach, that *A* and *B* really advocate the *same* statistical theory differing only in their manner of presentation of the principles (the *real* principles) of statistical inference?[5]

And on the ontological side, why posit the existence of 'probabilities' as theoretical features of the world over and above observable relative frequencies at all? If *A* and *B* are asserting the same thing, although they posit on the basis of the same evidential data about relative frequencies quite different 'probabilities,' why take their positing of 'probabilities' seriously at all? Here, of course, we are drawing just the same instrumentalistic consequences familiar in the physical and semantic cases. If theories positing curved and straight geodesics amount, given their differences in the non-geometric portion of physics, to the same theory, how can one take the positing of spacetime as an entity with a determinate structure seriously at all?

III

As an alternative to the account discussed above, consider an approach which assigns to statistical assertions a meaning independent of the particular rules of upward and downward inference which one adopts. Consider for example the theory which takes probabilities to be relative frequencies in the large (but finite) total reference class. One observes and predicts only relative frequencies of black swans in specified finite reference classes of swans. But what one means by the probability of a swan's being black is the relative frequency of black swans in the class of all swans, past, present and future.

Now that the statistical assertion has a determinate meaning, a meaning given totally independently of the rules for inferring to or from the probability to relative frequencies in specific finite classes of swans (except, of course, the uninteresting inference from and to the relative frequency in the total reference class relative to which the probability is defined) one can ask of each proposed upward or downward rule *why* one ought to adopt that rule as opposed to any other.

The response to this approach which argues that we want to understand 'probability' as it functions in use; that such use is only in taking us from observed relative frequencies to unknown, but knowable, relative frequencies; and that rationalization of the rules, if it can be given at all, can only be rationalization of the joint body of upward and downward rules as they work with each other to take us from data to prediction, is one of great persuasive power. Such an approach avoids, for example, a notorious difficulty with the particular theory discussed above in that it makes the old problem of the indeterminacy of frequencies in infinite reference classes of no concern. For now the relative frequency in the 'total' reference class enters not at all into our account. Indeed, 'probabilities,' as something over and above determinate and empirically determinable relative frequencies are eschewed altogether. Dismissing such an account with the label 'positivistic' or 'instrumentalistic' expressed with pejorative tone is, of course, no argument against this approach at all.

My argument above, however, if it is correct, does block the response which retains 'probabilities' as essential features of the world over and above mundane relative frequencies and places it in the realm of 'theoretical features of the world,' the terms referring to it having their meaning fixed only by their role in the total theory, including the upward and downward rules of inference. For, I have argued, when that account is looked at closely it quickly degenerates into the 'instrumentalist' account.

Of course, even in the instrumentalist account we need still ask ourselves what the direct relative frequency to relative frequency rules, compounded out of upward and downward rules function jointly, are to be and *why* we should adopt these rules and not others. This is still the fundamental problem of the logic of statistical inference.

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NOTES

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[1] See, for example, H. Cramer, *Mathematical Methods of Statistics*, Princeton, Princeton University Press, 1945, pp. 148–49; R. B. Braithwaite, *Scientific Explanation*, Cambridge, Cambridge University Press, 1955, pp. 151–153; I. Hacking, *Logic of Statistical Inference*, Cambridge, Cambridge University Press, 1965, chap. II. For an especially lucid and full account see I. Levi, *Gambling With Truth*, New York, Knopf, 1967, pp. 197–214.

[2] See Levi, *op. cit.*, pp. 205–208.

[3] M. Dummett, 'The Justification of Deduction,' *Proceedings of the British Academy*, 1974, pp. 5–6. See also Dummett's *William James Lectures*, mimeo., pp. 6:18–6:19.

[4] H. Reichenbach, *The Philosophy of Space and Time*, New York, Dover, 1958, Chap. I, secs. 1–8. W. Quine, *Word and Object*, New York, Wiley, Chap. II.

[5] Reichenbach. *op. cit.*, pp. 34–35. See also L. Sklar, *Space, Time, and Spacetime*, Berkeley, University of California Press, 1974. Chap. II, sec. H.