PHILOSOPHICAL LECTURE

PROBABILITY—THE ONE AND THE MANY

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1. *One criterion, or many?*

A PHILOSOPHICAL theory of probability, like many other kinds of theory, is concerned to increase understanding by revealing the underlying unity that generates superficial diversity. We are familiar with many apparently different types of probability: the probability of drawing an ace from a well-shuffled pack of cards, the probability that a British lorry-driver (any one you choose) will survive till the age of seventy, the probability that a radium atom will disintegrate within twenty-four hours, the probability that Excalibur will win the 1975 Derby, the probability (on the evidence before today's court) that the defendant uttered the slander of which the plaintiff complains, the probability (on available experimental evidence) that all cervical cancer is caused by a virus, and so on. A philosophical theory of probability seeks to interpret all these probabilities as relative frequencies, perhaps, or as degrees of belief, or as natural propensities, or as logical relations between propositions, or as truth-values in a multi-valued logic, or as some other unitary type of structure.

The unity sought has thus characteristically been a unity in criterion of gradation rather than in method of assessment, and the difference between these two types of unity is sufficiently important to deserve a brief initial digression.

A criterion of gradation is a framework of intrinsic differentiation in degree or quantity, while a method of assessment is a strategy for discovering such degrees or quantities within appropriate limits of accuracy. The centigrade scale is a criterion of gradation for temperature, while the application of an appropriately marked off mercury thermometer is a method of assessment for it. Correspondingly we may need to distinguish between the truth-conditions and the justification-conditions, respectively, for a proposition stating the temperature of, say, a person's blood. Some methods of assessment may be applicable to more than one criterion of gradation, like the use of a speedometer marked off in kilometres as well as in miles. Also a particular criterion of gradation may admit several methods of assessment, some perhaps more accurate than others. The hours and minutes of the passing day admit of being measured by more than one kind of clock. And the logical structure of a criterion of gradation may not always be entirely the same as that of its methods of assessment. For example, if car A is travelling at more m.p.h. than car B, and car B than car C, then A is travelling at more m.p.h. than C; but the actual measurements cannot be relied on to exhibit this transitivity in every case since the readings on any actual speedometer are subject to some margin of error.

Realists and anti-realists may dispute the nature of the relation between a criterion of gradation and a corresponding method of assessment. But the distinction has at least to be formulated in order for this dispute to be meaningful, and failure to draw the distinction at all produces no less confusion in regard to probability than it does in regard to other topics. In particular a mono-*criterial* account of probability is quite compatible with accepting a plurality of methods for *assessing* probabilities. The normal method for assessing the probability of drawing an ace from a well-shuffled pack is rather obviously different from the normal method for assessing the probability that a lorry-driver will survive till the age of seventy, even if the criterion of relative frequency is in fact appropriate in both cases. Assessment of the lorry-driver's probability requires statistical data while the card-picking probability can be calculated *a priori*.

Nevertheless, even when all this has been said, a monocriterial theory of probability is notoriously difficult to sustain. A relative frequency account, like von Mises's, seems to fit the actuarial probability of a lorry-driver's survival till age seventy much better than it fits the prediction that Excalibur will win the Derby in 1975, since it seems to imply that a probability should be predicated collectively rather than distributively—i.e. predicated of sets rather than of their members. A personalist account, like Savage's, seems to fit tipsters' predictions better than it fits scientific generalizations over unbounded domains, since bets on the truth of a single prediction can be decisively settled in a way that bets on the truth of an open-ended generalization cannot, and betting-quotients are therefore a more realistic criterion of belief-intensity in the former case than in the latter. And similar objections—some of which will be mentioned later—can be made to each of the familiar monocriterial accounts.

Accordingly in recent decades there has been a substantial trend of opinion towards some kind of polycriterial account of probability. Rival monocriterial theories claim to refute one another. But a polycriterial account supposes this or that monocriterial analysis, if internally consistent, to be not wholly refuted by the diversity of the facts—merely restricted in its domain of application. Popper[1] made an important contribution to this trend by showing, in 1938, that it is possible to construct the mathematical calculus of probability as a purely formal system, in which nothing whatever is assumed about the nature of the probability-function except that its logical syntax conforms to the axioms of the system. For example, its arguments do not have to be sets, as Kolmogorov's treatment implies them to be.[2] This establishes that the syntax of mathematical probability can be given a representation which is entirely neutral between all the rival monocriterial theories. But it does not suffice to show that more than one semantical interpretation of the calculus can function as a viable theory of probability that is properly so called. Here the work of Reichenbach and Carnap has been a turning-point. Reichenbach[3] sought to demonstrate in 1934 that the possibility of conceiving probabilities as the truth-values of a multi-valued logic was not incompatible with the possibility of achieving a semantic analysis of probability in terms of relative frequency. And

[1] K. R. Popper, 'A Set of Independent Axioms for Probability', *Mind*, xlvii (1938), pp. 275 ff., reprinted with further developments in K. R. Popper, *The Logic of Scientific Discovery* (1959), pp. 318 ff. I am grateful to Professor S. Körner, Dr. J. Leiber, and Mr. C. Peacocke for helpful comments on an earlier draft of this lecture.

[2] A. Kolmogorov, 'Grundbegriffe der Wahrscheinlichkeitsrechnung', *Ergebnisse der Mathematik*, ii. 3 (1933), translated by N. Morrison as *Foundations of the Theory of Probability* (1950). On the limitations of Kolmogorov's axiomatization cf. Tom Settle, 'Induction and Probability Unfused', in *The Philosophy of Karl Popper* (1974), p. 733. But Kolmogorov's treatment is an abstract one in so far as it does not restrict what can be elements of the (finite) sets with which it deals.

[3] H. Reichenbach, *Wahrscheinlichkeitslehre* (1935), translated by E. H. Hutton and M. Reichenbach as *The Theory of Probability* (1949). Cf. also George Boole, *An Investigation of the Laws of Thought* (1954) pp. 247 ff. There were difficulties in Reichenbach's proposal: cf. E. Nagel, *Principles of the Theory of Probability* (1939) for a general critique of the different interpretations that have been proposed for the calculus.

Carnap[1] showed later that the possibility of an analysis in terms of relative frequency was also not incompatible with the possibility of conceiving probability as a logical relation.

But the trouble with a polycriterial account of probability is that, when faced with this apparent plurality of criteria, it seems content to pursue the ideal of step-by-step descriptive adequacy rather than that of explanatory simplicity. When confronted with the inherent difficulties of the problem, it acknowledges the anomalies that face this or that monocriterial theory, and seems content to be driven by them to postulate a fundamental duality or multiplicity rather than a fundamental unity. It has apparently to surrender the prime goal of theory—unification—and then risks degenerating, as difficulties multiply, into mere natural history.

2. *The mathematicist theory*

Is there any way in which polycriterial tolerance can be incorporated into a single explanatory theory? Can any unifying principle be found to underlie the plurality of probability-criteria that the diversity of the facts drives us to accept? Or is the word 'probability' a merely accidental homonym as we move from one criterion's domain of application to another? That is the central problem to which this lecture is devoted. But before proposing my own solution I shall first briefly consider two other possible proposals.

One tempting way to deal with the problem is to argue that the required unification is to be found in the identity of the underlying calculus. A probability-function, it may be said, can now be defined quite formally by the axioms of the calculus, and it can also be shown, in the case of each worthwhile criterion, why any function that conforms to the criterion must be a probability-function. For example, Ramsey[2] showed this in regard to the belief-intensity criterion. What more, it may be asked, is needed? What more can a theory of probability do?

But this proposal serves to highlight the nature of the problem rather than to solve it. It sheds no light on why there should be such a diversity of semantical criteria, each satisfying the axioms of the calculus in its own way and each having some substantial utility or interest of its own. There may well be other purposes for which it is useful to define probability functions in terms of their formal or mathematical structure. But such a definition

[1] R. Carnap, *Logical Foundations of Probability* (1950).

[2] F. P. Ramsey, *Foundations of Mathematics* (1931), pp. 181 f.

will not serve our present purposes. The mathematical calculus is not a theory of probability in the required sense of 'theory'.

After all, a monocriterial account—a relative frequency theory, say, or a personalist theory—does at least purport to go beyond familiar data. It claims to show, at least in principle, how a value is generated by the probability-function in relation to any pair of arguments of the appropriate type. We are thus put into the position of being able to check the plausibility of the theory in relation to other data than those on which it was founded. But a purely formalist account does not do this. It describes the common structure that, as it happens, certain familiar concepts have. But it makes no contribution towards explaining why they *all* have it. It does not indicate in general terms either a sufficient or a necessary semantical condition for being a concept of the type that will, on investigation, always be found to have that structure. As a symptom of this fault, the formalist account does not enable us to discern any new examples of the structure it describes. It lacks any consequence analogous to that which Bacon[1] and Leibniz[2] long ago saw to be essential for any genuinely explanatory theory in natural science—the prediction of some hitherto unnoticed truth.

3. *The family resemblance theory*

The same weakness also vitiates a very different scheme—semantical rather than syntactical—for presenting a unitary but polycriterial account of probability. J. L. Mackie has recently argued that the term 'probability' has at least five basic senses, that are not mere homonyms. These five senses have grown out from and been nourished by a central root of meaning, he says—'which hovers between the extremes of guarded assertion and good but not conclusive reasons for believing'[3]—and they can all be explained as the result of a series of natural shifts and extensions. 'Probability', on Mackie's view, 'illustrates the thesis of J. S. Mill (recently taken over by Wittgenstein and his followers in their talk about family resemblances) that "names creep on from subject to subject, until all traces of a common meaning sometimes disappear".'[4]

Now, if this were intended as a genuinely historical account

[1] Francis Bacon, *Novum Organum* I, ciii and cvi.

[2] In his letter to Couring of 19 March 1678, published in *Die philosophischen Schriften von G. W. Leibniz*, ed. C. J. Gerhardt (1965), vol. i, p. 196.

[3] J. L. Mackie, *Truth, Probability and Paradox* (1973), p. 188.

[4] Ibid. p. 155, quoting J. S. Mill, *System of Logic*, Bk. I, ch. i, § 5.

of the matter, in the spirit of Mill's remarks, it would need to be accompanied by evidence that Mackie's supposed sequence of semantic shifts and extensions actually took place in the required temporal order. Mackie offers no historical evidence of this kind and, in the light of Hacking's recent researches,[1] it is highly doubtful whether it could be offered. Most of the current ways of talking about probabilities seem to have grown up together.

If, however, the aim of Mackie's account is somehow to make sense of the current situation, irrespective of historical etymology, it suffers from a defect inherent in the family-resemblance approach to conceptual problems. For example, men ride petrol-tankers, horses, and tractors, but not oxen; oxen, horses, and tractors, but not petrol-tankers, are used on farms for pulling things; petrol-tankers, tractors, and oxen stay on the ground while horses sometimes jump; and all petrol-tankers, oxen, and horses have their front and rear means of locomotion approximately the same height, while most tractors do not. But no one supposes it appropriate to mark *this* particular nexus of family resemblance by carrying over the name of one of the four sorts of objects to describe the other three sorts. The fact is that some groups of four or more sorts have common names, and some do not, even when the sorts exhibit family resemblance to one another. So unless the exponent of a family-resemblance approach to probability tells us why *his* supposed nexus of family resemblance generates a common name, in contrast with others that do not, he has not explained anything. But if he does do this, and does it adequately, he has to go beyond a merely family-resemblance account. It will certainly not be adequate to say that a common name is generated when the resemblances are sufficiently close, if the only test of sufficient closeness is the use of a common name. And again a symptom of failure to achieve a genuine explanation is the failure of the theory to imply any further checkable consequences. The family-resemblance account imposes an all-too-familiar kind of gloss on the data we already have, but cannot be relied on to lead us to anything new.

4. *The degree of provability theory*

The problem then is to present a polycriterial account of probability that avoids being purely syntactical, as the family-

[1] I. Hacking, 'Jacques Bernoulli's *Art of Conjecturing*', *British Journal for the Philosophy of Science*, xxii (1971), pp. 209 ff., and 'Equipossibility Theories of Probability', ibid. pp. 339 ff.

resemblance account does, but is also genuinely explanatory, which that account is not. We want to know how such very different criteria of probability are applicable in different contexts, when they are still felt to be, in some important sense, criteria of the same thing. And the clue to a solution of the problem is to be found by bearing in mind that in the Latin ancestry of the European concept of probability there is to be found not only the notion of approvability, on which recent philosophers[1] have tended to concentrate for their etymologies, but also that of provability. So let us see what follows if we regard probability as a generalization on the notion of provability.

This idea is by no means wholly new to philosophy. Even Locke contrasted demonstration with probability as different forms of proof.[2] But, to simplify the situation, it is better to regard demonstrability as a limiting-case of probability rather than as an opposite of it. This too is a familiar idea from the work of de Morgan, Keynes, Waismann, and others.[3] What seems hitherto unexplored, however, is the possibility of founding a polycriterial, rather than a monocriterial, account of probability on this basis.

In any artificial language-system one formula B is said to be provable from another A if and only if there is a primitive or derived syntactic rule that licenses the immediate derivation of B from A. But the kind of provability that concerns us in the present inquiry is not purely syntactic. So, to avoid confusion, let us speak of a (primitive or derived) syntactic proof-rule as being inferentially sound, in an interpreted system S, if and only if the conclusion of any derivation which it licenses is true whenever the premiss or premisses are. Then the hypothesis to be considered is that probability is degree of inferential soundness.

[1] E.g. William Kneale, *Probability and Induction* (1949), p. 20.

[2] John Locke, *An Essay Concerning Human Understanding*, Bk. IV, ch. xv, § 1 *ad init.*: 'As demonstration is the showing the agreement or disagreement of two ideas, by the intervention of one or more proofs, which have a constant, immutable, and visible connexion one with another; so probability is nothing but the appearance of such an agreement or disagreement, by the intervention of proofs, whose connexion is not constant and immutable, or at least is not perceived to be so, but is, or appears, for the most part to be so, and is enough to induce the mind to judge the proposition to be true or false, rather than the contrary.'

[3] Cf. A. de Morgan, *Formal Logic, or the Calculus of Inference, Necessary and Probable* (1847); J. M. Keynes, *A Treatise on Probability* (1921), pp. 133 ff.; and F. Waismann, 'Logische Analyse des Wahrscheinlichkeitsbegriffs', *Erkenntnis*, i (1930), pp. 228 ff.

To grade the probability of B on A is to talk qualitatively, comparatively, ordinally, or quantitatively about the degree of inferential soundness of a primitive or derived rule that would entitle one to infer B immediately from A. And, just as demonstrative provability, when philosophically reconstructed, is always relative to the primitive derivation-rules of some particular deductive system, so too degree of probability is always relative to some particular criterion.

What makes this account inherently polycriterial is that, just as there are several different ways in which deductive systems, regarded as interpreted logistic systems, may be distinguished from one another in accordance with the type of provability regulated by their respective derivation-rules, so too different criteria generate correspondingly different types of gradation for inferential soundness. Indeed it turns out that all the main differences between commonly discussed criteria of probability are revealed in this way, provided that attention is not confined to the rather limited variety of derivation-rules that is normally at issue in metamathematical proof-theory.

To categorize the type of provability regulated by the primitive derivation-rules of a deductive system, where these rules are inferentially sound, at least three important questions need to be asked. All three are questions that are also familiar in many other contexts of philosophical analysis. Briefly stated, the questions are: Is a typical statement about such provability general or singular? Is its truth necessary or contingent? Is it extensional or non-extensional?

A typical statement about provability in a deductive system S is general if, as commonly in mathematical logic, the primitive derivation-rules of S set up proof-schemas, legitimating the inference from any formula or formulas of a certain kind to a corresponding formula of another kind. Proofs may then be conducted wholesale in these terms, with individual formulas never specified. On the other hand, a typical statement about provability in S is singular if each primitive derivation-rule legitimates an inference only from one specific sentence to another, as from 'There will never be a nuclear war' to 'London will never be destroyed by hydrogen bombs'.[1]

[1] I shall say nothing here in regard to deductive systems that have both singular and general rules, or in regard to systems that have rules which are singular in regard to their premisses and general in regard to their conclusions or vice versa. The probability-functions that correspond to such mixed systems seem to be as little used as they are studied.

Familiar examples of necessarily true statements about provability are those for which the conditionalizations are truths of logic, say, or arithmetic: the derivation-rules are then deductive in the logician's or philosopher's rather specialized sense of 'deduction'. But a statement about provability is contingent if it relates to derivation-rules which are read off from some physical theory, like the rules of classical mechanics or special relativity which are needed to prove—or to 'deduce', in the everyday sense of that word—from astronomical data that the next eclipse of the sun visible in England will be on 11 August 1999.

Finally, a statement about provability is extensional if it remains true whenever co-extensive terms are substituted for one another in the formulas about which it speaks. But such a statement is non-extensional if it does not always remain true under these circumstances, as is often held to be the case in the logic of indirect discourse.

Accordingly, on the proposed hypothesis (that probability is a generalization of provability), a corresponding three questions may be asked as a step towards disambiguating the comparative sentence-schema of natural language '. . . is more probable than - · - · - · on the assumption - - -', or towards determining the variety of meanings available for a functor p[· · ·, - - -] = n that maps probabilities on to numbers.

Should the blanks of the comparative sentence-schema be filled by expressions signifying membership of this or that class, as when formulas that may constitute the premisses (or the conclusions) of a particular proof-rule are classed together if and only if they supply different subjects to the same specified predicable?[1] Or should these blanks be filled by expressions designating this or that fully determinate proposition, as when the premisses and conclusion of a proof are fully specified. Secondly, should the completed sentences, if true, be taken to formulate necessary truths of logic or arithmetic, or should they be taken instead to formulate contingent truths about the world? And, thirdly, should the completed sentences be regarded as preserving their truth-values whenever terms that have identical extensions replace one another therein, or not?

Analogously, are the fillers for the argument-places in a rationally reconstructed probability-functor to be conceived of

[1] In using the term 'predicable' here, rather than 'predicate', I follow the example of P. T. Geach, *Reference and Generality* (1962), p. 23 f., in another context.

as self-naming predicables, or as self-naming sentences? Is an accepted equation of the form p[· · ·,- - -] $=n$ to be regarded as necessarily, or contingently, true? And is the left-hand side of such an equation as extensional as the right-hand side?

Thus whether we regard ourselves as being concerned with *discovering* the meanings of natural-language sentences about probability, or with *inventing* meanings for the formulas of a formal system, at least three of the most important questions that can be asked about demonstrative provability can also be asked about probabilities. Moreover these three binary dimensions of categorization provide a matrix within which all the familiarly advocated criteria of probability can be accommodated in distinct pigeon-holes.

Some examples will clarify these claims and help to substantiate them.

5. *Proof-criteria that are general, necessary and extensional*

Suppose first that statements about probability are to correspond to expressions that signify *general* rules of demonstrative inference, concerned with any formulas of a certain kind. The probability-functor will therefore be assigned self-naming predicables as fillers for its argument-places. Suppose too that the resultant equations are *extensional* and, if true, *necessarily* true. Here, where we are dealing with predicables and construing them extensionally, we shall be able to grade the inferential soundness of the derivation-rule at issue in terms of its success-rate. For example, the probability of a number's being prime, if greater than 1 and less than 10 may be said—informally—to be 0·5, because out of 8 such numbers just 4 are primes. But this is tantamount to stating a rule that entitles us to infer a sentence with 'is a prime' as predicate, from a sentence with 'is greater than 1 and less than 10' as predicate, whenever both sentences have the same subject, and claiming that the rule has a 50 per cent reliability in any context to which it is applicable. And it is applicable wherever no other premiss is relevant to inferring that conclusion: i.e. the numeral that is the subject of both premiss and conclusion in each case is supposed to be picked at random. Similarly the probability of any one outcome in a six-outcome game of chance is $\frac{1}{6}$, because a rule which says

> From an outcome's being either A, B, C, D, E, or F infer its being A

has a $\frac{1}{6}$ reliability wherever it applies. And again such a rule applies wherever no other premiss is relevant to inferring that conclusion: e.g. it applies to random throws of a well-balanced cubic die on to a flat surface. This kind of rule correlates two sets of equally specific, mutually exclusive and jointly exhaustive outcome-types, one of which is the set described in the premiss and the other of which is the intersection between this set and the set described in the conclusion. The soundness of the rule is assessed *a priori* by calculating the ratio that the size (i.e. cardinality) of the latter intersection bears to the size of the set with which it is correlated.

This kind of probability-statement has often been taken to presuppose a principle of indifference between alternative outcomes. But that principle creates notorious difficulties, not least about why it is needed for some kinds of probabilities and not for others. And if one recognizes what kind of gradation of inferential soundness is being made by these probability-statements, one can see the problem in its proper perspective. We do not need a special principle of indifference here but only a proviso about other relevant premisses; and that proviso is no different from the one which needs to be made whenever a necessarily sound rule of inference is applied. There is a familiar rule that 2 and 2 make 4, but the rule applies to counting the apples in a bucket only if there is no hole in the bottom.

Thus the limitations of this kind of criterion of probability are quite apparent. The conception of probability as an *a priori* calculable ratio between one set of mutually exclusive outcomes and another applies well to set-ups of assumed randomness, like games of chance. Absence of empirical evidence that one outcome (out of the game's set of equally specific, mutually exclusive and jointly exhaustive outcomes) is more probable than another is just what characterizes a game as a game of pure chance. But in science, as has been justifiably remarked, one does not obtain knowledge out of ignorance. So this classical criterion of probability has no application to findings in the natural or social sciences. Moreover, according to this criterion, what in effect probability-functions map on to numbers are ordered pairs of *sets*. So the criterion assigns probability-values collectively, not distributively, and it cannot guarantee application of the collective value to individual outcomes. There is a 0·5 probability that any number you choose which is greater than 1 and less than 10 will be a prime, but there is certainly not a 0·5 probability that the number 4 is a prime. There is a 0·5 probability of landing heads for any coins-toss. But there is certainly not a 0.5 probability that yesterday's toss of tails was actually heads.

6. *Proof-criteria that are general, contingent and extensional*

Suppose next that we consider the probabilistic analogue for rules of demonstrative inference that are general, extensional and *contingent*. Here again the most obvious criterion of gradation for demonstrative soundness is by the ratio of one set-membership size to another.[1] The reliability of the rule

> From a man's being a lorry-driver, infer his survival till age seventy

may be assessed empirically by estimating, from samples appropriate to some chosen confidence-level, the ratio of the number of lorry-drivers surviving till seventy to the total number of lorry-drivers. But we might also, instead of talking about ratios, seek to grade the reliability of this rule, as Reichenbach's work suggests,[2] by the truth-value in a multi-valued logic of a certain combination of propositional sequences, like the proof of a multiple conclusion from a multiplicity of premisses. We should then have a mode of gradation for inferential soundness that was not only contingent and extensional but also concerned with singular, rather than general, inference-rules, in that it applied to (sequences of) specific propositions rather than to predicables.

The conception of probability as an empirically estimatable relative frequency, or as the limit of this frequency in a long run of randomly chosen samples, has the advantage of indicating a precise and inter-personally objective source of quantitative

[1] An objector might be inclined to argue that wherever probabilities are defined over sets the resultant equations are necessarily true, because, whatever two sets are named, those two sets have the same size-ratio in each possible world. However, the argument seems too powerful to be valid, for it applies to numbers as well as to sets and thus generates necessary truth for, say, the statement that the ratio of the number of men's colleges to the number of women's colleges in Oxford is 18 to 5. My remarks in the text must be construed as being intended in a sense of 'necessary truth' in which this statement is not necessarily true while it is necessarily true that the ratio of the number of primes to the total number of integers between 1 and 10 is 1 to 2. My sense is therefore one in which the necessity of a statement's truth depends not only on the sets, numbers, or other things it refers to but also on its mode of reference to them, so that some true statements of identity may be contingent. It would be out of place to say more about this currently much-discussed topic in the present context.

[2] Op. cit., 1949 ed., pp. 387 ff.

determination in very many types of statistical problem. But again probability-values are being assigned collectively, not distributively. So this conception, if strictly construed, has the disadvantage of not indicating any simple, direct, and trouble-free method for assessing the probabilities of such individual events. If we speak of the probability that John Smith, *qua* lorry driver, will survive till seventy, we are not speaking about John Smith specifically, but only about any randomly picked lorry-driver, and we are still not speaking specifically about John Smith if we get nearer to identifying the intersection of relevant sets to which he belongs and speak of him *qua* fifty-year-old, British, diabetic, father of four children, living next to an asbestos factory, son of a suicide, and so on.[1] Also, since this conception of probability applies to predicables, not to complete sentences, it does not indicate any obviously plausible way to assess the strength of scientific hypotheses in relation to experimental evidence. Anyone who seeks in it a foundation for inductive logic is handicapped from the start. Nor does the conception of a probability as a truth-value solve any epistemological problem that cannot be solved by conception of it as a ratio, since the precise truth-value to be assigned has still to be assessed by consideration of the relevant ratio, whether this be a relative frequency in a statistical sample or a proportion of favourable alternatives in a game of chance. It is possible that the conception of probability as a truth-value in an appropriate multi-valued logic has the intellectual advantage of linking probability theory with logic. But it does not help to indicate how any such probabilities may be ascertained, except in the limiting cases of 0 and 1.

7. *Proof-criteria that are general, contingent and non-extensional*

Let us now examine the probabilistic analogue for rules of demonstrative inference that are general, contingent, and non-extensional, like statements of causal laws. We have to consider what happens when the probability-functor has (self-naming) predicables to fill its argument-places but the resultant equations are contingent and non-extensional. Since co-extensive predicables may not now be substituted for one another, we have

[1] So in Reichenbach's interpretation a statement about the probability of a single case has no meaning of its own. It represents 'an elliptic mode of speech' and, 'in order to acquire meaning, the statement must be translated into a statement about a frequency in a sequence of repeated occurrences' (ibid. pp. 376–7).

to suppose that such a probability-function maps ordered pairs of properties, attributes, or characteristics, not ordered pairs of sets, on to numbers. So the soundess or reliability of a rule of inference is then graded by the strength of the contingent, physical connection between two characteristics, as perhaps with the rule that would entitle us to infer from a thing's being a radium atom to its disintegrating within twenty-four hours. Indeed, to treat a radium atom's probability of disintegration within twenty-four hours as a so-called 'propensity' of the atom is in effect to grade the soundness of such a rule of inference by the strength of this physical connection. Or rather, *if* a worthwhile distinction is to be drawn between interpreting the mathematical calculus of chance as a theory of relative frequencies and interpreting it as a theory of propensities, *then* an essential, though curiously seldom noticed, feature of difference between the two interpretations is that the former makes probability-equations extensional and the latter makes them non-extensional. But in both cases the same method of assessment is available. The probability may be estimated from relative frequencies in appropriate samples.

This conception of a probability as a measurable physical connection between two characteristics—a 'propensity', as it is sometimes called—has the advantage of allowing probabilities to be predicated distributively of individual objects in well-understood environments, like radium atoms in experimental situations. If a particular object has the one characteristic, it may be said to be subject to the weakish connection linking that characteristic with the other. But, if this conception is ever justifiably applicable, it can be so only when some actual or possible scientific theory is supposed, as perhaps ideally in atomic physics,[1] to predict or explain the precise strength of the particular connection involved, quite apart from sample-based estimates of that strength. Since the connection is then regarded as not being purely accidental, the terms signifying what are taken to be connected may with good reason be understood non-extensionally, like the terms of a statement asserting a causal law. The connection lies between one property, attribute, natural kind[2] or other characteristic, and another, not

[1] For a criticism of Popper's analysis of quantum-theoretical probabilities as propensities cf. P. Suppes, 'Popper's analysis of Probability in Quantum Mechanics', in *The Philosophy of Karl Popper*, ed. P. A. Schlipp (1974), pp. 760 ff.

[2] Admittedly W. V. Quine, *Ontological Relativity and Other Essays* (1969),

between two sets.[1] But where no theoretical explanation of the precise strength and nature of an empirically estimated probability is supposed obtainable, as in the case of the probability that a fifty-year-old lorry-driver—anyone you please—will die within the next twenty years, there is no reason not to conceive the probability extensionally as the ratio of one set's member's to another's. Similarly, where set-ups of assumed randomness are concerned, as in games of chance, it is appropriate to conceive a probability as an *a priori* calculable ratio between one set of equally specific, mutually exclusive and jointly exhaustive possibilities and another rather than as the empirically measurable strength of a physical connection. If we do start to measure the physical connection between a coin's being tossed and its landing heads, we are in effect checking how far the assumption that we are dealing with a game of chance is appropriate.

8. *Proof-criteria that are singular and either necessary or contingent*

I am trying to show how the hypothesis that probability is degree of inferential soundness is bound to generate a polycriterial account of probability in accordance with the different criteria of gradation that are appropriate to grading the soundness of familiarly different kinds of inference-rule. And I have so far confined myself almost entirely to general inference-rules, whether these be necessary or contingent, extensional or non-extensional. I shall turn now to singular inference-rules and to the analogous probability-functions, which take fully formed, self-naming sentences as fillers of their argument-places.

If such an inference-rule is necessary, rather than contingent,

pp. 131 ff., has attempted to reduce the concept of a natural kind to the wholly extensional concept of a set of objects that match each other in respect of their parts, like the molecules of a particular chemical element. But this makes the logical analysis of a concept depend on the infinite divisibility of matter. If matter is not infinitely divisible, there must be some level in the study of natural kinds (perhaps sub-atomic physics) at which objects cannot be classified together in terms of their matching parts.

[1] Philosophers have disputed whether such a propensity-type probability 'belongs to' an individual atom, say, or to an experimental set-up. On the proposed analysis it belongs to both, though in different ways. On the one hand it connects a certain type of experimental set-up with a certain type of event. On the other hand, each instance of such a set-up is thereby subjected to the connection. From this point of view to dispute whether a propensity belongs primarily to a set-up or to its instances is as philosophically pointless as disputing whether we should say that being penetrated by a bullet causes a heart to stop beating, or rather that the bullet in the individual victim's heart causes it to stop beating.

the analogous gradation of inferential soundness will have entailment as the upper limit of the relation between premiss and conclusion and contradiction as the lower limit, and whatever intermediate relations obtain will also obtain necessarily. For example, Carnap's programme for inductive logic supposed that these conditions were satisfied by the confirmation-relation between a statement of experimental evidence, *e*, and a scientific hypothesis, *h*. According to Carnap the probability of *h* on *e* is to vary with the extent to which the range of *e* is contained within the range of *h*, where the range of a sentence *s* in a language-system L is the class of those state-descriptions in L in which *s* holds true. Different range-measures then provide different methods of assessment for probabilities formulated in the appropriate artificial language-system. But there is a strong constraint on the application of such necessarily true statements of probability, similar to that already noticed in the case of other necessarily true evaluations of demonstrative soundness. The price to be paid for deriving practical benefit from a necessary truth is that conditions must be just right for its application, whether it be applied in evaluating the probability of a tossed coin's landing heads or in summing the apples in a barrel. And, as Carnap himself pointed out, in using the inference-rule that corresponds to one of his confirmation-functions we have to be sure that *e* states all the available evidence. The equation $c(h, e) = r$ entitles us to hold that the proof of *h* from *e* has degree *r* of inferential soundness only so far as *e* states all the available evidence.

The conception of a probability as a logical relation between propositions, corresponding to a singular rule of demonstrative inference, has the advantage of not precluding the association of probabilities with individual events. It achieves this by allowing the assignment of probabilities to the propositions reporting these events. But even if such a conception can be adapted, as is notoriously difficult, to languages of richer structure than monadic first-order predicate calculus, it seems inevitably to confront its users with the need to make some evidentially unsupported decision, like the choice of a preferred Carnapian range-measure out of an infinity of available ones. Such a decision might sometimes be a matter of considered policy as regards the degree to which prior probabilities should be allowed to influence our calculations. But there seems no way in which the decision can be appraised for truth or falsity in the light of empirically discoverable facts. So, as a philosophical reconstruction of how people actually reason with one another, the logical theory of probability is applicable only where some such *a priori* decision may legitimately be imputed.

It is natural to wonder therefore, so far as the probability-functor is to take fully formed self-naming sentences as fillers of its argument-places, whether we should not do better to conceive statements about probability as being capable only of contingent, not necessary, truth. In such a conception probability is not a logical operation on propositions, but perhaps a psychological or epistemological one. Statements about probability are analogous to singular rules of inference that are empirically validated, and the task for a criterion of probability is to provide a gradation for the inferential soundness of such rules. And one familiar way of doing this is to grade how strongly a rational man does, or should, believe in the truth of the conclusion when the truth of the premiss is given him. The inferential soundness of a rule deriving B from A might thus be calibrated in terms of the lowest odds at which a man who distributes all his wagers coherently might bet on B if given A, where at least a necessary condition[1] for coherence is that the bettor should not distribute his bets in such a way as to ensure an over-all loss of money.

Conception of probability as degree of belief escapes difficulties like the arbitrariness of choosing a particular range-measure in Carnapian inductive logic. By grading probabilities in terms of acceptable betting-odds within a coherent betting policy, the so-called subjectivist or personalist approach can plausibly claim that all sufficiently informed and identically motivated rational men would agree about such probabilities.[2] But,

[1] On various possible conceptions of coherence here cf. H. E. Kyburg, Jr., and H. E. Smokler (eds.), *Studies in Subjective Probability* (1964), editorial introduction p. 11. There are problems about the impact of a rational man's interests on the betting-odds he will accept. Some of these problems are resolved by C. A. B. Smith, 'Consistency in Statistical Inferences and Decision', *Journal of the Royal Statistical Society* (Series B), xxiii (1961), pp. 1–37. But a non-coherent betting policy might be quite rational for a non-acquisitive man: cf. the remarks of G. A. Barnard, ibid., pp. 25–6.

[2] Of course, a proof that invokes a contingent rule of inference can always be transformed into one that invokes a necessary rule, if the contingent rule is transformed into the major premiss of the new proof. Analogously a contingently true probability-statement can, in principle, always be reconstructed as a necessarily true one in which the facts warranting the contingent truth are included in the evidence for the new probability. I.e. the contingent probability p[B, A] warranted by C, becomes the logical probability p[B, A & C]. But, if the ways in which the content of A and the content of C affect the

presumably because it is not easy to become so rational, well-informed and conventionally motivated and because, for reasons already mentioned, betting on open-ended generalizations is hardly an appropriate activity for rational men, few researchers in the natural or social sciences have in fact adopted this personalist approach.

9. *Probability-statements as evaluations of inferential soundness*

I have only been able to sketch the situation very summarily and incompletely. But what I have said so far should suffice to show that each of the familiarly advocated criteria of probability fits neatly into place within the matrix generated by three primary questions about deductive systems and their proof-rules. If we view probability-statements as evaluations of inferential soundness, we are led naturally to recognize their inherent capacity for heterogeneity; and since different kinds of deductive system are appropriate to different tasks we can understand why such widely differing criteria of probability have actually been put forward. A monocriterial account of probability is as viciously Procrustean as a monocriterial account of goodness which would identify the criteria of a good pen, say, with those of a good gardener.

A polycriterial account of probability is thus to be seen as no more requiring a family-resemblance analysis than does a polycriterial account of goodness. The criteria of a good pen can be distinguished quite uncontroversially from the criteria of a good gardener. But not only is the term 'good' not a mere accidental homonym in these contexts, as Aristotle long ago remarked:[1] we do not have to give a family-resemblance, successive-shift-of-meaning account of it either. There is an underlying nuclear meaning of the word which enables us to use it in judging the value of indefinitely many different types of thing. In this meaning it is no more a homonym, or a polyseme, than is 'value' itself. So, too, like many other terms of appraisal or

value of p[B, A & C] are highly disparate, this mode of construction serves no useful purpose. There may also be a problem about substitutivity. On both the logical and the personalist accounts probability-statements are non-extensional, in the sense that co-extensive predicables are not always inter-substitutable *salva veritate* in such statements. But, whereas logically equivalent expressions would normally be inter-substitutable therein on a logical account, they would not be inter-substitutable on a personalist account unless the rationality of a rational bettor is taken to include logical omniscience.

[1] *Nicomachean Ethics* 1096b 26–7.

evaluation—like 'good', 'valid', legitimate', 'beautiful', etc.—the term 'probable' turns out to have, quite compatibly with its single nuclear meaning, a wide variety of criteria of gradation which may be regarded as determining the existence of a correspondingly wide variety of different concepts of probability.

But whatever the nuclear meaning of such an evaluative term is (and sometimes a philosphical characterization of it is rather difficult) this nuclear meaning would certainly be useless without some appropriate criterion to complement it in any particular context of use. And therein lies a substantial difference between the nuclear meaning of such an evaluative term and any original root of meaning for a descriptive term, like 'game', say, from which various other meanings may have developed by a process of family resemblance. The latter is self-sufficient in a way that the former is not.

It seems reasonable to accept, then, that the vocabulary of probability is, at bottom, the proper terminology for grading and evaluating the soundness of proof-rules. And, if one accepts this, one can see not only why, because there are different kinds of proof-rule, there must also be correspondingly different criteria of probability, but also why there was an opportunity for some philosophers[1] to be excessively impressed by the use of the vocabulary of probability in the utterance of guarded assertions. Evaluative terms like 'good' and 'probable' lend themselves very readily, as predicates in simple categorical sentences, to the performance of rather characteristic types of speech-act, such as commendation and guarded assertion, respectively. But that the terms have to be assigned meanings independently of these performances becomes clear when we consider their use in the antecedents of conditional sentences, and in other more complex contexts, that exclude speech-acts of this type.[2]

Nor does the guarded-assertion theory help us much to see how, at its heart, probability is a relation. By appealing to such familiar types of utterance in ordinary speech as 'It is probable that Peter will come to the party' where reference to the

[1] E.g. S. E. Toulmin, 'Probability', *Proceedings of the Aristotelian Society*, supp. vol. xxiv (1950), pp. 27 ff.; J. R. Lucas, *The Concept of Probability* (1970), pp. 1 ff., and J. L. Mackie, op. cit., pp. 158 f.

[2] M. Dummett, Frege: *Philosophy of Language* (1973), pp. 327 ff., has recently defended the applicability of sentential operators to sentences already containing force-indicators in certain special cases. But I shall not examine his arguments here because they do not have the generality that would be required to sustain a 'guarded assertion' theory of probability.

evidence has been omitted because it is unnecessary, the guarded-assertion theory invokes an ellipse as a paradigm, and directs attention to the rhetorical, rather than to the logical, structure of discourse. Here as elsewhere[1] the surface forms of ordinary language are a particularly unrevealing guide to the philosophical student of probability. More specifically, what I have in mind is that any analysis for the term 'probable', that is to allow interpretation of the mathematical calculus of chance as a logic of probability, must at least elucidate a certain well-known fact. This is that the following three expressions do not necessarily have the same truth-value for particular A, B and n, viz.: $p[B, A] = n$, $p[A \rightarrow B] = n$, and $A \rightarrow p[B] = n$.[2] And the guarded-assertion theory is inherently incapable, on its own, of elucidating this fact, because all three expressions can function equally well as forms of guarded assertion where the truth of A is known or assumed.

But the proposed analysis of probability as degree of inferential soundness achieves the required elucidation quite simply. For according to that analysis an expression of the form of $p[B, A] = n$ grades the soundness of inferring B from A, and

[1] For instance, the normal German word for probability, *wahrscheinlichkeit* has no etymological connection with the notion of provability. Though *probabel* and *probabilität* are occasionally used today, *wahrscheinlich* goes back into the seventeenth century. But the long-standing tendency to treat *warscheinlich* and *scheinbar* as opposites does indicate that in its semantics *wahrscheinlich*, like probable, has an underlying concern with interpretation of evidence. It is noteworthy also that even in constructing the mathematical calculus of probability as a purely formal system it is less restrictive to take the dyadic functor as primitive and define the monadic functor in terms of it, than to adopt the reverse procedure; cf. K. R. Popper, 'Replies to my Critics', in *The Philosophy of Karl Popper*, ed. P. A. Schlipp (1974), p. 1132, and Tom Settle, 'Induction and Probability Unfused', ibid., p. 733.

[2] Three false equivalences are at issue here:

$$\text{(i)}\ (A)(B)(n)(p[B, A] = n \leftrightarrow p[A \rightarrow B] = n).$$

Whereas elementary logic gives us $p[A \rightarrow B] = p[-B \rightarrow -A]$, it is demonstrable that $p[B, A]$ is not in every case equal to $p[-A, -B]$: cf. L. Jonathan Cohen, *The Implications of Induction* (1970), p. 113.

$$\text{(ii)}\ (A)(B)(n)(p[B, A] = n \leftrightarrow (A \rightarrow p[B] = n)).$$

Consider any case in which $p[B, A] > p[B]$ and A is contingently true.

$$\text{(iii)}\ (A)(B)(n)(p[A \rightarrow B] = n \leftrightarrow (A \rightarrow p[B] = n)).$$

Consider any case in which A logically implies B, A is contingently true, and $p[B] < 1$, or in which A is necessarily false and $n < 1$. Because of these non-equivalences the term 'conditional probability' is an unfortunate name for $p[B, A]$: 'dyadic probability' is less misleading.

this must be different, in principle, from grading the soundness of inferring A $\rightarrow$ B since, notoriously, there are deductive systems in which one particular formula may be demonstrable from another even though the truth-functional conditional linking the two is not demonstrable. Similarly, since according to the analysis what are commonly called 'prior' probabilities—and would better be called 'monadic' ones—correspond to inferences from the null class of assumptions, an expression of the form A $\rightarrow$ p[B] $= n$ tells us what level of soundness to assign, if what A says is true, to the derivation of B from no assumptions at all. But the expression does not imply that B must be derivable from A with just this level of soundness—which is quite another issue and dealt with instead by p[B, A] $= n$. Nor does it tell us, unconditionally, that the truth-functional conditional linking A and B must be derivable with this level of soundness from no assumptions—which is again an obviously different issue and dealt with by p[A $\rightarrow$ B] $= n$.

10. *The connection between deductive completeness and mathematical probability*

I remarked earlier that a good explanatory theory, in philosophy as in natural science, should always predict some hitherto unnoticed type of truth. And I used this point as one argument against both formalist and family-resemblance theories of probability. Such theories merely put a gloss on what we know already about the polycriterial nature of probability and do not lead directly to the discovery of some hitherto unacknowledged criterion. It is clearly incumbent on me, therefore, to show that my own analysis of probability in terms of inferential soundness does lead to such a discovery; and the remainder of my lecture, which is all too brief for the purpose, will be devoted to this end.

The familiar criteria of probability were pigeon-holed within a matrix that was generated by three binary dimensions of categorization. The three questions 'General or singular?', 'Necessary or contingent?' and 'Extensional or non-extensional?'—questions that arise naturally in any categorization of inference-rules—were applied to gradations of inferential soundness and were found to generate enough distinctions to accommodate all the philosophically familiar types of criteria in separate pigeon-holes. It follows that, if further questions of this sort can usefully be asked, some or all of the pigeon-holes may be subdivided and we may have not only a fuller characterization of the familiar criteria, but also an indication of some philosophically unfamiliar ones. If both of the latter results are obtainable, we shall have substantial confirmation of the present analysis.

Well, there is certainly another important question that arises about any deductive system in relation to its inference-rules: is it complete or not? A deductive system is said to be complete as to provability if and only if, for every well-formed formula B, either B is provable or if B is added as an axiom the system becomes inconsistent. So in a system that is both consistent and complete, and includes negation, any closed formula B is provable from the axioms A if and only if not-B is not provable. Now, if probability is to be conceived as degree of inferential soundness, the demonstrative provability of B from A is a limiting-case of probability, where p[B, A] = 1. Hence, if this limiting-case is an instance of provability in a complete system, we should also have p[not-B, A] = 0—at least where A and B are closed formulas; and, in general, the probability of B on A should be expected to vary inversely with the probability of not-B on A. For we cannot, in a complete system, assert anything about the non-probability or non-provability of B except in terms that imply asserting something about the probability or provability of not-B. What emerges is the familiar complementational principle for negation: p[B, A] = 1 − p[not-B, A]. Completeness, as a property of certain deductive systems, may thus be viewed as a limiting-case of probabilistic complementationality.

In an incomplete deductive system, on the other hand, even where all the well-formed formulas are closed, there must be at least one well-formed formula such that neither it nor its negation is provable. That is to say, when provability is described in terms of probability, there must be an A and a B such that both p[B, A] = 0 and also p[not-B, A] = 0. It follows that where criteria of probability are invoked that are analogous to the primitive derivation-rules for an incomplete deductive system the familiar complementational principle for negation cannot apply. If we ever wish to reason in terms of such probabilities, our statements must have some other logical structure than that articulated by the familiar mathematical calculus of chance.

11. *The grading of probabilities by evidential weight*

A probability that is analogous to demonstrability in an incomplete system has much in common with what Keynes long ago referred to as the 'weight' of evidence.[1] Though the absence of a required axiom precludes us perhaps from having a demonstrative proof of B, it may nevertheless be conceived to be compatible with admitting some degree of provability for B since so many of the axioms we need for proving B are there. 'B is *almost* demonstrable', we might say. Similarly Keynes said that 'one argument has more *weight* than another if it is based on a greater amount of relevant evidence.... It has a greater amount of *probability* than another if the balance in its favour, of what evidence there is, is greater than the balance in favour of the argument with which we compare it.' Weight, Keynes was convinced, cannot be analysed in terms of mathematical probability. An equation of the form p[B, A] $= n$ is not more likely to be right, he said, if of higher weight, since such an equation states the relation between A and B with equal accuracy in either case. 'Nor is an argument of high weight one in which the probable error is small; for a small probable error only means that magnitudes in the neighbourhood of the most probable magnitude have a relatively high probability, and an increase of evidence does not necessarily involve an increase in these probabilities.' Keynes did not feel sure, so he said, that the theory of what he called evidential weight had much practical significance. But perhaps that was because he seems to have envisaged it wrongly, like Peirce before him,[2] as a mere auxiliary to the theory of mathematical probability, and not as an independent criterion of probability in its own right. Correspondingly the measures developed by statisticians for grappling, in effect, with what Keynes took to be the problem of evidential weight—measures like Fisher's significance levels or Neyman's confidence intervals—are designed to operate on, or make statements about, estimates of statistical magnitudes like mathematical probabilities. They do not constitute criteria for the inferability of B from A that judge by nothing but the relevant 'weight' of A.

What happens if we do judge inferability in this way? We obtain a positive gradation of inferability for B only if the evidence is, on balance, in favour of B, and the level of this gradation then depends just on the amount of the evidence. Only if the evidence were, on balance, in favour of not-B, would we instead, by grading the amount of relevant evidence we

[1] J. M. Keynes, *A Treatise on Probability* (1921), pp. 71 ff.

[2] C. S. Peirce, *Collected Papers*, ed. C. Hartshorne and P. Weiss, vol. ii (1932), p. 421.

have, obtain a positive gradation of inferability for not-B. So, when we obtain a positive gradation of inferability for a proposition from consistent evidence, we obtain none for its negation. In other words, where A is consistent, if p[B, A] > o, then p[not-B, A] = o. But the converse does not hold—the negation principle is non-complementational—since A might be indecisive or wholly irrelevant in regard to B. The evidence might neither favour B on balance, nor not-B. We should then have both p[B, A] = o and p[not-B, A] = o. And this is like saying that in an incomplete deductive system it does not hold that, if not-B is not provable from A, then B is so provable, since it might be the case that neither B nor not-B is provable.

Of course, some philosophers may wish to defend Keynes's failure to accept evidential weight as a criterion of probability, just on the ground that incomplete deductive systems cannot accord with the complementational principle for negation. Such a philosopher would hold it safe to conceive probability as degree of inferential soundness only so far as complete systems are concerned. Since an extension of the analogy to incomplete systems breaks the guarantee of conformity to familiar principles of the mathematical calculus of chance, he prefers to restrict the inferential-soundness analysis in a way that will maintain a guarantee of that conformity. But this philosophical position has three substantial demerits.

First, it has an inelegant lopsidedness. Why should there not be gradations of inferential soundness for incomplete systems as well as for complete ones, and, if so, what better term can there be for these gradations than 'probabilities'? Certainly none of the other three binary dimensions of categorization for provability did anything to restrict the scope of the semantical connection between probability and inferential soundness. So what is there about the completeness-incompleteness distinction that justifies its doing this? One can hardly say that incomplete deductive systems are very rare or always unimportant.

Secondly, if probabilistic analogues for incomplete systems are rejected just because their structures do not conform to the mathematical calculus of chance, the life-blood of probability is being identified with a purely formal structure rather than with a semantical feature. Yet, as we have already seen, a formalist account of probability cannot provide an explanatory theory of the required sort—a theory that will *explain* the diversity of identically named criteria which are actually in use. Just as a syntactic homogeneity that was semantically unelucidated did not suffice to explain the semantic fact that different criteria are all equally named criteria of probability, so too a merely syntactic heterogeneity does not suffice to invalidate a semantical argument for identity of nomenclature. Indeed the old example of non-Euclidean geometry, and the more recent one of non-Zermelian set-theory, should warn anyone against being too quick to deny the possibility of what we might call a non-Pascalian calculus of probability.

Thirdly, in everyday life we very often have to form beliefs in a context of incomplete information. We need something better then than a concept of probability which is conclusively applicable to our inferences only when we have total relevant evidence. What may well serve our purposes instead is a concept that grades how much of the relevant evidence we do have. For example, given that the circumstantial evidence before the court does on balance favour the prosecution, how does a member of the jury judge the probability of the accused's having struck the fatal blow? Surely he must ask himself how large a portion of the relevant facts have been determined: did the bloodstains on the accused's clothing belong to the victim's blood-group? does the accused wear size ten boots? has his alleged alibi been refuted? and so on.

It will be part of a much larger undertaking to show how, in Anglo-American lawcourts, the only legitimate way for juries to evaluate admissible proofs of fact is in terms of this kind of probability,[1] which has a non-complementational negation-principle; to show how one important concept of this kind is closely dependent on, though by no means identical with, a concept of inductive support that belongs originally to the tradition of Bacon, Herschel, and Mill; and to show also how that concept can resolve certain familiar paradoxes about probability, such as the difficulty of picking on a level of probability which can constitute a threshold of rational acceptance or justifiable belief. Roughly, the probability of B on A will turn out, in many such contexts, to equal the (ordinal) grade of inductive support that exists, according to Baconian principles, for the proposition 'A

[1] I do not refer here to mathematical probabilities that may form part of expert evidence (e.g. the statistical probability of a worker's developing asbestosis in an asbestos factory), but to the probability of a particular conclusion on the testimonial and other facts before the court. It should be remembered that in civil, as distinct from criminal cases, the normal standard of proof required is 'proof on the balance of probability', not proof beyond reasonable doubt.

is a cause, or a sign, of B'.[1] But I can say no more about this now. On the present occasion I have been trying to show only that a theory of probability can be both polycriterial and genuinely explanatory if it works out the implications of regarding probability as that mode of gradation of which demonstrative provability is a limiting case.[2]

[1] Cf. L. Jonathan Cohen, *The Implication of Induction* (1970), pp. 35 ff. This criterion of probability resembles that of personalist probability in being analogous to inference-rules that are singular, contingent, and non-extensional, but an important difference between the two criteria—a difference in their negation principles—stems from the issue about completeness.

[2] It should be noted that, when one treats a given mode of gradation *M* as a generalization of a certain quality *Q*, or *Q* as a limiting-case of *M*, one does not imply that every feature of *Q* is generalized in *M*. For example, it is no objection to treating the concept of temperature as a generalization on the concepts of hot and cold that, for any person's hand at a particular time, there is some intermediate degree of temperature—the same as that of the hand—which does not have a distinctive feel. Similarly, as an example from the present issue, it is normally the case that, if C is provable from A (without the help of the principle that contradictions imply anything whatever), then C is provable from A and B (without the help of the principle that contradictions imply anything whatever); and so we have correspondingly, in the mathematical calculus of probability, the principle that, if $p[C, A] = 1$ and $p[A,B] > 0$, then $p[C, A \,\&\, B] = 1$. But this principle does not generalize into the principle that, for any n, if $p[C, A] = n$ and $p[A, B] > 0$, then $p[C, A \,\&\, B] = n$. Part of the fall-out from any successful proposal to treat *M* (e.g. probability) as a generalization of *Q* (e.g. provability) is a clarification of which features belong to *Q* in virtue of its being merely a quality, as it were, or a limiting-case, and which features belong to *Q* independently of this status.