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PROBABILITY AS A GUIDE IN LIFE [1]

MANY philosophers are of the opinion—and I agree with them—that the concept of probability is of importance not only from a theoretical point of view but also in practice: it serves as a guide in life. We can never obtain certainty in predictions of future events but at best some measure of probability. We form our expectations of future events in accordance with their probabilities on the basis of the observations made so far, and these expectations influence our decisions.

It seems to me that there are *two* concepts of probability, two different meanings in which the word "probability" is commonly used.[2] Suppose a physicist ascribes the value 0.03 to the probability that the kinetic energy of a molecule in a given body of hydrogen lies within a certain interval. This means that 3 per cent. of the molecules are in such a state of energy. In this case "probability" means as much as "relative frequency in the whole population." On the other hand, if a scientist says that on the basis of such and such observations one hypothesis is more probable than another, he means to say that it is more strongly supported or confirmed by the given evidence. Thus "probability" here means "degree of confirmation." Let us use the term "probability$_1$" for this concept, and "probability$_2$" for the frequency concept. I believe that it is possible to define a *quantitative* concept of probability$_1$ so that it would be possible to make statements of the form "the degree of confirmation of the hypothesis *h* with respect to the evidence *e* is so and so much," specifying a numerical value. Statements of this kind express a purely logical relation between the sentences *h* and *e*, analogous to, though different from, statements of logical implication. While the latter statements con-

[1] This paper (without the footnotes) was read at the Meeting of the American Philosophical Association, Western Division, at Chicago on May 9, 1945.

[2] The distinction of the two concepts and the nature of inductive logic are discussed in "The Two Concepts of Probability," *Philosophy and Phenomenological Research*, Vol. V (1945), pp. 513–532, and "Remarks on Induction and Truth," *ibid.*, Vol. VI (1946), pp. 590–602.

stitute deductive logic, the former statements, in my conception, constitute *inductive logic.*[8]

Since there are those two different concepts of probability, we have to study the question as to which of them can serve as a guide in life. It will be the aim of this paper to show that this holds for the concept of probability$_1$, that is, degree of confirmation. Others deny this, in particular those who want to restrict the theory of probability to probability$_2$, that is, frequency. They believe that only the concept of probability$_2$ can be of help in making practical decisions because only a statement on probability$_2$ says something about the facts of nature. A statement on probability$_1$, on the other hand, is purely logical; if it is true, it is logically true, analytic; it does not say anything about facts. All of us agree in this characterization. But those philosophers draw from it the conclusion that a statement on probability$_1$ can not serve as a basis for our decisions, and hence can not be regarded as a genuine probability statement. It is this objection to the concept of probability$_1$, the denial of its applicability and usefulness for practical purposes, that I want to discuss.

The distinction between the two kinds of probability statements is, I believe, a special case of a more general distinction between two kinds of statements, a distinction which is important for the methodology of empirical science but has not so far been sufficiently analyzed and clarified. This is the distinction between (1) a statement about the actual value of a physical magnitude in a given case, a value which is either unknown to the observer or at least not known exactly, and (2) a statement about the *best estimate* of this value with respect to given evidence, which may, for instance, include the results of some measurements made by the observer. Suppose the observer has measured the length of a given rod three times, with the results, say, 80.0, 80.1, 80.5. Let us assume that the measurements were made under the same conditions. Then there is no reason for regarding any one of the three results as more reliable than any other. Therefore the observer will take as an estimate of the length of the rod the arithmetic mean of the three values, that is, 80.2. He can not assert with certainty that the actual length is 80.2 (not even if this figure is understood as an abbreviated expression for the interval 80.15 to 80.25). The value 80.2 is merely an estimate; that means, it is a guess; not an arbi-

[8] A tentative definition for a quantitative concept of degree of confirmation with respect to a simple language system and a few theorems of inductive logic based on the definition have been given in "On Inductive Logic," *Philosophy of Science*, Vol. 12 (1945), pp. 72–97. References in subsequent footnotes are to this paper.

trary guess but a sensible guess. It is indeed the best guess the observer can make in the present situation, as long as no results of further measurements are available to him. (For the sake of simplicity, I omit here a reference to the standard deviation, which would serve as a measure for the precision or the reliability of the estimated value.) Now let us compare the following two sentences which occur in this example:

(1) The actual length of the rod is 80.2. $l(a) = 80.2$.
(2) The best estimate of the length of the rod with respect to the given evidence e is 80.2. $l'(a,e) = 80.2$.

(The evidence e is here the statement of the three observed values 80.0, 80.1, and 80.5.)

The first of these two sentences is an empirical sentence, one with factual content. (We need not discuss in detail the problem of its exact interpretation; it may be interpreted, for example, as saying that the arithmetic mean of the results of the first n measurements would, with increasing n, converge towards 80.2.) The second sentence, on the other hand, is analytic. It is based upon the definition of "the best estimate." We may assume that this definition is constructed in such a manner that it implies that, for simple cases like the one under discussion, the best estimate is the mean of the observed values. The sentence (2) can not be either confirmed or disconfirmed by any future observations. Even if the results of future measurements tend towards a value considerably different from 80.2, it still remains true that 80.2 is the best estimate *with respect to the evidence* consisting of the three values stated earlier.

Let us suppose that the observer has to make a practical decision concerning the use of a given rod, a decision which depends upon the length of the rod. Then he proceeds, of course, as follows: he acts in certain respects as though he knew that the length was 80.2. Now let us analyze the theoretical basis of this behavior. I do not mean here the psychological question as to the actual process by which the observer arrives at this decision, but rather a rational reconstruction of this process. Which of the two sentences (1) or (2) may serve as a rational basis for the decision? We might be tempted to say that this must be the sentence (1). However, this sentence lies outside of the domain of the observer's knowledge at the present moment, that is to say, this sentence is not highly confirmed on the basis of his evidence.[4] Still less does

[4] The probability$_1$ on the evidence e for the assumption that the actual length is exactly 80.2 is 0; the probability$_1$ for the assumption that the actual length is between 80.15 and 80.25 is considerably lower than ½.

he know whether future observations will highly confirm it and hence suggest its acceptance or highly disconfirm it and hence suggest its rejection. Therefore the observer can not find a theoretical basis for his decision in sentence (1). But he finds it in sentence (2), because this sentence, added to the evidence available, supplies the value 80.2 which determines his decision. On the basis of his observations, he possesses the evidence consisting of the three values 80.0, 80.1, and 80.5. Then the sentence (2) tells him that, with respect to this evidence, the value 80.2 is the best estimate he can make. And this result determines his decision.

Generally speaking, situations of this kind may be characterized as follows. Practical decisions of a man are often dependent upon values of certain magnitudes for the things in his environment. Since he does not know the exact value, he has to base his decision on an estimate. This estimate is given in a statement of the form: "The best estimate for the magnitude in question with respect to such and such observational results is so and so." This statement is purely analytic. Nevertheless it may serve as a basis for the decision. It can not, of course, do so by itself, since it has no factual content; but it may do so in combination with the observational results to which it refers.

Now let us return to the problem of the concept of probability$_1$, that is, degree of confirmation. It seems to me that the situation here is to some extent analogous to that in the example just discussed. Suppose that a sample of eighty persons has been selected at random from the population of Chicago and sixty of these persons have been found to possess a property *M*. This constitutes the present evidence *e*. Let *h* be a singular hypothesis, namely, the prediction that one person taken at random from the non-observed part of the population will be found to have the property *M*. If a definition for the degree of confirmation *c* has been constructed, the value of $c(h,e)$ can be established, that is, of the degree of confirmation for the hypothesis *h* just mentioned with respect to the evidence *e* concerning the sample of eighty individuals. For our discussion the actual value of *c* is irrelevant. It may be remarked incidentally that for plausible definitions of *c*, the value in this case will be equal or near to the relative frequency of *M* in the observed sample, which was $\frac{3}{4}$. To make the example more concrete, let us arbitrarily assume that $c(h,e) = 0.73$.[5] If a

[5] For the present discussion it does not matter whether *c* in this case is equal to the relative frequency observed or only close to it (see *op. cit.*, p. 86). I have chosen here the slightly different value 0.73 in order to make it clear that the estimated value to be discussed in what follows is equal to the value of *c* (here 0.73) and not necessarily to the observed frequency (here $\frac{3}{4}$).

suitable general definition D for the concept of the best estimate in terms of degree of confirmation is laid down, then a theorem T can be proved which says that this estimate in cases of the kind described always has the same value as $c(h,e)$.[6] Applied to our example, T says that the best estimate of the relative frequency of M in the whole population of Chicago with respect to the evidence e has the same value as $c(h,e)$. We have assumed that $c(h,e) = 0.73$. Therefore the best estimate of the relative frequency in the whole population is likewise 0.73, hence close to the relative frequency observed in the sample, which seems plausible. Now let us compare two sentences concerning the present example; they are analogous to the earlier sentences concerning the actual length of a rod and the estimate of its length.

(1) The actual relative frequency of M in the population of Chicago is 0.73.
$fr(M,C) = 0.73$.

(2) a. The degree of confirmation for the singular hypothesis h with respect to the evidence e concerning the observed sample is 0.73.
$c(h,e) = 0.73$.

[6] Various methods of dealing with problems of estimation have been developed in modern mathematical statistics, but the situation is still rather controversial. I believe that a satisfactory general concept of the best estimate must be based on a concept of degree of confirmation. Suppose that a concept of degree of confirmation, say c, has been defined. For the sake of simplicity, let us consider a function (physical magnitude) $f(x)$ whose range of values is finite; let the possible values be $r_1, r_2, \ldots r_n$. Let h_i ($i = 1$ to n) be the hypothesis that $f(x) = r_i$. Then the definition D for $f'(x,e)$, that is, the best estimate of $f(x)$ with respect to the evidence e, is as follows:

$$f'(x,e) = \sum_{i=1}^{n} [r_i \times c(h_i,e)].$$

(This is what is sometimes called the expectation value of $f(x)$; compare H. Jeffreys, *Theory of Probability*, pp. 42f. Most contemporary theories of mathematical statistics give a different but analogous definition for the expectation value, using the frequency concept of probability instead of the concept of degree of confirmation, since the latter does not occur in these theories.) The theorem T mentioned in the text can now be stated as follows. Let e describe a sample with respect to a property M; let h be the hypothesis that an individual not belonging to the sample has the property M; let C be the whole population; let "*fr*" designate relative frequency, and "*fr'*" the best estimate (in the sense of the definition D) of relative frequency; then $fr'(M,C,e) = c(h,e)$. This theorem holds not only for one particular function c chosen as degree of confirmation but for a very comprehensive class of such functions, *viz.*, those which I have called symmetrical c-functions (*op. cit.*, § 5). They include, among others, all those functions which may be considered as adequate concepts of degree of confirmation.

According to the theorem *T*, this sentence is logically equivalent to the following:

b. The best estimate of the relative frequency of *M* in the whole population with respect to the evidence *e* is 0.73. $fr'(M,C,e) = 0.73$.

(Note, by the way, that the theorem *T* which connects (2a) with (2b) throws some light on the peculiar relationship that holds between the two probability concepts mentioned earlier. (2a) is a statement of degree of confirmation, hence a statement of probability$_1$. (2b) is a statement of the estimate of the relative frequency in the whole population, hence a statement of an estimate of probability$_2$. Therefore *T* says that probability$_1$ may be interpreted, at least in certain special cases, as an estimate of probability$_2$.)

Suppose that the observer has to make a practical decision, perhaps of an administrative or legislative nature, a decision depending upon his knowledge concerning the relative frequency of *M* in the population of Chicago. It is clear what he will do; he will act in certain respects as though he knew that the relative frequency was 0.73. But it is perhaps not immediately clear what the theoretical basis for this action is,—in other words, which rational procedure would lead to this action. Should the first or the second of the two sentences be taken as a basis for the decision? The proponents of the frequency conception of probability will perhaps say, the first, because this is a statement about the relative frequency in the whole and hence a probability statement in their sense. However, this first sentence is not known to the observer as long as his knowledge is restricted to the evidence *e* concerning the eighty observed individuals. The first sentence is not even highly confirmed on the basis of the evidence *e*. It is rather the second statement that may serve as a basis for the decision. This statement is known to the observer because it is, in either of the two equivalent formulations (a) and (b), analytic; it follows from the presupposed definition of the degree of confirmation *c* and, for (b), the definition *D* of the best estimate of a function. The statement (2b) is quite analogous to the earlier statement concerning the best estimate of the length of a rod. Here again, the statement about the estimate can not be either confirmed or disconfirmed by any future observations. Even if a complete census of the population of Chicago showed that the actual relative frequency were quite different from 0.73, this would by no means refute the statement that the best estimate *with respect to the evidence e* is 0.73. Here, as in the earlier case, the decision can be based on the given observational evidence *e* and the analytic statement which gives the best estimate with respect to this evidence *e*. It is the value of this estimate or, in other words, the value of degree of confirmation that justifies the decision.

We obtain the same result if we consider the following situation. Suppose that the observer wants to make a bet on the prediction that an arbitrarily chosen individual has the property *M*. This prediction is the hypothesis *h*, to which the statement (2a) ascribes the degree of confirmation 0.73 with respect to the available evidence *e*. Thus on the basis of this statement the observer will decide to give odds of at most 73 to 27 (or, approximately, 3 to 1) on the prediction *h*. (The same decision could also, of course, be based on the statement (2b) concerning the estimate of the relative frequency.)

Our discussions have shown that statements of probability in the logical sense, that is, degree of confirmation, may be used as a guide for practical decisions. To use them in this way does not imply any apriorism, because these purely logical probability statements are not meant to be used in isolation but rather in application to the concrete knowledge situation at the time of the decision. Every decision is based on expectations. To find a rational basis for decisions we must have a rational method for obtaining expectations, and, in particular, estimations. Methods of this kind are used in the customary procedures of inductive thinking, both in everyday life and in science. These customary procedures contain *implicitly* the concept of degree of confirmation. To make this concept and thereby the procedures based upon it *explicit* is the task of inductive logic.[7] In thus helping to provide a clarified rational basis for

[7] I have said in an earlier paper (*op. cit.*, § 16) that it is the task of inductive logic to give a rational reconstruction (or explication) "of a body of generally accepted but more or less vague beliefs," namely, "of inductive thinking as customarily applied in everyday life and in science." Against this view Gustav Bergmann ("Some Comments on Carnap's Logic of Induction," *Philosophy of Science*, Vol. 13, 1946, pp. 71–78, see especially pp. 77f.) says that "the new calculus does not, in the philosophical sense, reconstruct anything. . . . If there is no philosophical problem of induction, then we cannot have any beliefs in this area, no matter how vague or how generally accepted, that are in any sense capable of rational reconstruction." It seems to me, however, that the following two items are historical facts which can not well be denied: (1) There are certain ways of inductive thinking in which all scientists practically agree without stating explicit rules; these ways of thinking are essential to science in the sense that without them scientific inquiry would be impossible. (2) Attempts at a rational reconstruction of these customary ways of inductive thinking have repeatedly been made, for instance, in the classical theory of probability (which may be regarded primarily as a theory of probability$_1$, not of probability$_2$), in the theories by

decisions, inductive logic can serve as a tool not only for theoretical but also for practical purposes.

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WHAT IS REALISM?

THE term "realism" once referred to a systematic mode of thought with distinctive principles clearly opposed to every form of subjectivism and idealism. At present this is no longer the case. Contemporary philosophical usage has allowed this word to fade into a murky cloud of ambiguity in which nothing very clear or distinct can be discerned. This tendency is not peculiar to any one school of contemporary thought, but is common to all schools, even those which have adopted the term in some form to describe their own positions. This paper embodies a few brief suggestions concerning the proper meaning of this term. It is hoped that an effort to clarify and sharpen the meaning of this now obscure epithet may be welcomed not only by friends but by enemies as well, since it is futile to attempt to oppose a mass of equivocations. In the interest of clarity in philosophical debate let us then ask ourselves what we mean by the common term "realism" at the present time.

It suggests to us, of course, the abstractly epistemological theories of the so-called neo-realists and critical realists who, *qua* realists, admittedly refrained from making any metaphysical assertions. But an isolated epistemology of this sort is a mere fragment whose ultimate philosophical bearing remains indeterminate. It is quite possible that such a theory, though decking itself out in a realistic terminology, might ultimately prove to be reconcilable with an idealistic or naturalistic metaphysics. Yet if we pay any

John Maynard Keynes and Harold Jeffreys, and in the methods of estimation developed in modern mathematical statistics. In view of the fact (1), this task of reconstruction is important for the methodology of science. My theory of degree of confirmation is merely a new approach towards the same aim. It does not claim to give a rational *justification* for the customary inductive procedures. When Bergmann says that my theory does not reconstruct anything and that there are no beliefs capable of rational reconstruction, perhaps he did not mean to deny the facts (1) and (2), but intended merely to say that no reconstruction *including a justification* is possible. It seems to me advisable to distinguish clearly between the relatively modest, though still important, task of reconstruction and the deeper-going task of a justification, which is still unsolved and even regarded by many as unsolvable. (On this task and its distinction from the former one, see my remarks at the place mentioned, *op. cit.*, § 16, with reference to Reichenbach's partial solution.)