THE TWO CONCEPTS OF PROBABILITY

I. THE PROBLEM OF PROBABILITY

The problem of probability may be regarded as the task of finding an adequate definition of the concept of probability that can provide a basis for a theory of probability. This task is not one of defining a new concept but rather of redefining an old one. Thus we have here an instance of that kind of problem—often important in the development of science and mathematics—where a concept already in use is to be made more exact or, rather, is to be replaced by a more exact new concept. Let us call these problems (in an adaptation of the terminology of Kant and Husserl) problems of *explication;* in each case of an explication, we call the old concept, used in a more or less vague way either in every-day language or in an earlier stage of scientific language, the *explicandum;* the new, more exact concept which is proposed to take the place of the old one the *explicatum.* Thus, for instance, the definition of the cardinal number three by Frege and Russell as the class of all triples was meant as an explication; the explicandum was the ordinary meaning of the word 'three' as it appears in every-day life and in science; the concept of the class of all triples (defined not by means of the word 'triple' but with the help of existential quantifiers and the sign of identity) was proposed as an explicatum for the explicandum mentioned.

Using these terms, we may say that the problem of probability is the problem of finding an adequate explication of the word 'probability' in its ordinary meaning, or in one of its meanings if there are several.

II. THE LOGICAL CONCEPTS OF CONFIRMATION

In the preparation for our subsequent discussion of the problem of probability, let us examine some concepts which are connected with the scientific procedure of confirming or disconfirming hypotheses on the basis of results found by observation.

The procedure of confirmation is a complex one consisting of components of different kinds. In the present discussion, we shall be concerned only with what may be called the logical side of confirmation, namely, with certain logical relations between sentences (or propositions expressed by these sentences). Within the procedure of confirmation, these relations are of interest to the scientist, for instance, in the following situation: He intends to examine a certain hypothesis h; he makes many observations of particular events which he regards as relevant for judging the hypothesis h; he formulates this evidence, the results of all observations made, or as many of them as are relevant, in a report e, which is a long sentence.

Then he tries to decide whether and to what degree the hypothesis *h* is confirmed by the observational evidence *e*. It is with this decision alone that we shall be concerned. Once the hypothesis is formulated by *h* and the observational results by *e*, then this question as to whether and how much *h* is confirmed by *e* can be answered merely by a logical analysis of *h* and *e* and their relations. Therefore the question is a logical one. It is not a question of fact in the sense that knowledge of empirical fact is required to find the answer. Although the sentences *h* and *e* under consideration do themselves certainly refer to facts, nevertheless once *h* and *e* are given, the question of confirmation requires only that we are able to understand them, i.e., grasp their meanings, and to discover certain relations which are based upon their meanings. If by semantics[1] we understand the theory of the meanings of expressions, and especially of sentences, in a language then the relations to be studied between *h* and *e* may be regarded as semantical.

The question of confirmation in which we are here interested has just been characterized as a logical question. In order to avoid misunderstanding, a qualification should be made. The question at issue does not belong to deductive but to inductive logic. Both branches of logic have this in common: solutions of their problems do not require factual knowledge but only analysis of meaning. Therefore, both parts of logic (if formulated with respect to sentences rather than to propositions) belong to semantics. This similarity makes it possible to explain the logical character of the relations of confirmation by an analogy with a more familiar relation in deductive logic, viz., the relation of logical consequence or its converse, the relation of L-implication (i.e., logical implication or entailment in distinction to material implication). Let *i* be the sentence 'all men are mortal, and Socrates is a man', and *j* the sentence 'Socrates is mortal'. Both *i* and *j* have factual content. But in order to decide whether *i* L-implies *j*, we need no factual knowledge, we need not know whether *i* is true or false, whether *j* is true or false, whether anybody believes in *i*, and if so, on what basis. All that is required is a logical analysis of the meanings of the two sentences. Analogously, to decide to what degree *h* is confirmed by *e*—a question in logic, but here in inductive, not in deductive, logic—we need not know whether *e* is true or false, whether *h* is true or false, whether anybody believes in *e*, and, if so, whether on the basis of observation or of imagination or of anything else. All we need is a logical analysis of the meanings of the two sentences. For this reason we call our problem the logical or semantical problem of confirmation,

[1] Compare Alfred Tarski, "The Semantic Conception of Truth and the Foundations of Semantics," this journal, vol. IV (1944), pp. 341–376; and R. Carnap, *Introduction to Semantics*, 1942.

in distinction to what might be called the methodological problems of confirmation, e.g., how best to construct and arrange an apparatus for certain experiments in order to test a given hypothesis, how to carry out the experiments, how to observe the results, etc.

We may distinguish three logical concepts of confirmation, concepts which have to do with the logical side only of the problem of confirmation. They are all logical and hence semantical concepts. They apply to two sentences, which we call hypothesis and evidence and which in our example were designated by "h" and "e" respectively. Although the basis is usually an observational report, as in the application sketched above, and the hypothesis a law or a prediction, we shall not restrict our concepts of confirmation to any particular content or form of the two sentences. We distinguish the positive, the comparative, and the metrical concepts of confirmation in the following way.

(i) *The positive concept of confirmation* is that relation between two sentences h and e which is usually expressed by sentences of the following forms:

"h is confirmed by e."

"h is supported by e."

"e gives some (positive) evidence for h."

"e is evidence substantiating (or corroborating) the assumption of h."

Here e is ordinarily, as in the previous example, an observational report, but may also refer to particular states of affairs not yet known but merely assumed, and may even include assumed laws; h is usually a statement about an unknown state of affairs, e.g., a prediction, or it may be a law or any other hypothesis. It is clear that this concept of confirmation is a relation between two sentences, not a property of one of them. Customary formulations which mention only the hypothesis are obviously elliptical; the basis is tacitly understood. For instance, when a physicist says: "This hypothesis is well confirmed," he means ". . . on the evidence of the observational results known today to physicists."

(ii) *The comparative* (or topological) *concept of confirmation* is uusally expressed in sentences of the following forms (a), (b), (c), or similar ones.

(a) "h is more strongly confirmed (or supported, substantiated, corrobo rated etc.) by e than h' by e'."

Here we have a tetradic relation between four sentences. In general, the two hypotheses h and h' are different from one another, and likewise the two evidences e and e'. Some scientists will perhaps doubt whether a comparison of this most general form is possible, and may, perhaps, restrict the application of the comparative concept only to those situations where two evidences are compared with respect to the same hypothesis (example (b)), or where two hypotheses are examined with respect to one evidence (example (c)). In either case the comparative concept is a triadic relation between three sentences.

(b) "The general theory of relativity is more highly confirmed by the results of laboratory experiments and astronomical observations known today than by those known in 1905."

(c) "The optical phenomena available to physicists in the 19th century were more adequately explained by the wave theory of light than by the corpuscular theory; in other words, they gave stronger support to the former theory than to the latter."

(iii) *The metrical* (or quantitative) *concept of confirmation,* the concept of *degree of confirmation.* Opinion seems divided as to whether or not a concept of this kind ever occurs in the customary talk of scientists, that is to say, whether they ever assign a numerical value to the degree to which a hypothesis is supported by given observational material or whether they use only positive and comparative concepts of confirmation. For the present discussion, we leave this question open; even if the latter were the case, an attempt to find a metrical explicatum for the comparative explicandum would be worth while. (This would be analogous to many other cases of scientific explication, to the introduction, for example, of the metrical explicatum "temperature" for the comparative explicandum 'warmer', or of the metrical explicatum 'I.Q.' for the comparative explicandum 'higher intelligence'.)

III. THE TWO CONCEPTS OF PROBABILITY

The history of the theory of probability is the history of attempts to find an explication for the pre-scientific concept of probability. The number of solutions which have been proposed for this problem in the course of its historical development is rather large. The differences, though sometimes slight, are in many cases considerable. To bring some order into the bewildering multiplicity, several attempts have been made to arrange the many solutions into a few groups. The following is a simple and plausible classification of the various conceptions of probability into three groups[2]: (i) the classical conception, originated by Jacob Bernoulli and Laplace, and represented by their followers in various forms; here, probability is defined as the ratio of the number of favorable cases to the number of all possible cases; (ii) the conception of probability as a certain objective logical relation between propositions (or sentences); the chief representatives of this conception are Keynes[3] and Jeffreys[4]; (iii) the conception of

[2] See Ernest Nagel, *Principles of the Theory of Probability,* (International Encyclopedia of Unified Science, Vol. I, 1939, No. 6).

[3] John Maynard Keynes, *A Treatise on Probability,* 1941.

[4] Harold Jeffreys, *Theory of Probability,* 1939.

probability as relative frequency, developed most completely by von Mises[5] and Reichenbach[6].

In this paper, a discussion of these various conceptions is not intended. While the main point of interest both for the authors and for the readers of the various theories of probability is normally the solutions proposed in those theories, we shall inspect the theories from a different point of view. We shall not ask what solutions the authors offer but rather which problems the solutions are intended to solve; in other words, we shall not ask what explicata are proposed but rather which concepts are taken as explicanda.

This question may appear superfluous, and the fact obvious that the explicandum for every theory of probability is the pre-scientific concept of probability, i.e., the meaning in which the word 'probability' is used in the pre-scientific language. Is the assumption correct, however, that there is only one meaning connected with the word 'probability' in its customary use, or at the least that only one meaning has been chosen by the authors as their explicandum? When we look at the formulations which the authors themselves offer in order to make clear which meanings of 'probability' they intend to take as their explicanda, we find phrases as different as "degree of belief," "degree of reasonable expectation," "degree of possibility," "degree of proximity to certainty," "degree of partial truth," "relative frequency," and many others. This multiplicity of phrases shows that any assumption of a unique explicandum common to all authors is untenable. And we might even be tempted to go to the opposite extreme and to conclude that the authors are dealing not with one but with a dozen or more different concepts. However, I believe that this multiplicity is misleading. It seems to me that the number of explicanda in all the various theories of probability is neither just one nor about a dozen, but in all essential respects—leaving aside slight variations—very few, and chiefly two. In the following discussion we shall use subscripts in order to distinguish these two meanings of the term 'probability' from which most of the various theories of probability start; we are, of course, distinguishing between two explicanda and not between the various explicata offered by these theories, whose number is much greater. The two concepts are: (i) *probability*$_1$ = degree of confirmation; (ii) *probability*$_2$ = relative frequency in the long run. Strictly speaking, there are two groups of concepts, since both for (i) and for (ii) there is a positive, a comparative, and a metrical concept; however, for our discussion, we may leave aside these distinctions.

Let me emphasize again that the distinction made here refers to two

[5] Richard von Mises, *Probability, Statistics, and Truth*, (orig. 1928) 1939.

[6] Hans Reichenbach, *Wahrscheinlichkeitslehre*, 1935.

explicanda, not to two explicata. That there is more than one explicatum is obvious; and indeed, their number is much larger than two. But most investigators in the field of probability apparently believe that all the various theories of probability are intended to solve the same problem and hence that any two theories which differ fundamentally from one another are incompatible. Consequently we find that most representatives of the frequency conception of probability reject all other theories; and, *vice versa*, that the frequency conception is rejected by most of the authors of other theories. These mutual rejections are often formulated in rather strong terms. This whole controversy seems to me futile and unnecessary. The two sides start from different explicanda, and both are right in maintaining the scientific importance of the concepts chosen by them as explicanda—a fact which does not, however, imply that on either side all authors have been equally successful in constructing a satisfactory explicatum. On the other hand, both sides are wrong in most of their polemic assertions against the other side.

A few examples may show how much of the futile controversy between representatives of different conceptions of probability is due to the blindness on both sides with respect to the existence and importance of the probability concept on the other side. We take as examples a prominent contemporary representative of each conception: von Mises, who constructed the first complete theory based on the frequency conception, and Jeffreys, who constructed the most advanced theory based on probability$_1$. Von Mises[7] seems to believe that probability$_2$ is the only basis of the Calculus of Probability. To speak of the probability of the death of a certain individual seems to him meaningless. Any use of the term "probability" in everyday life other than in the statistical sense of probability$_2$ has in his view nothing to do with the Calculus of Probability and cannot take numerical values. That he regards Keynes' conception of probability as thoroughly subjectivistic[8] indicates clearly his misunderstanding.

On the other hand, we find Jeffreys similarly blind in the other direction. Having laid down certain requirements which every theory of probability (and that means for him probability$_1$) should fulfill, he then rejects all frequency theories, that is, theories of probability$_2$, because they do not fulfill his requirements. Thus he says[9]: "No 'objective' definition of probability in terms of actual or possible observations . . . is admissible," because the results of observations are initially unknown and, consequently, we could not know the fundamental principles of the theory and would have no starting point. He even goes so far as to say that "in practice,

[7] *Op. cit.*, First Lecture.
[8] *Op. cit.*, Third Lecture.
[9] *Op. cit.*, p. 11.

no statistician ever uses a frequency definition, but that all use the notion of degree of reasonable belief, usually without ever noticing that they are using it."[10] While von Mises's concern with explicating the empirical concept of probability$_2$ by the limit of relative frequency in an infinite sequence has led him to apply the term "probability" only in cases where such a limit exists, Jeffreys misunderstands his procedure completely and accuses the empiricist von Mises of apriorism: "The existence of the limit is taken as a postulate by von Mises .... The postulate is an *a priori* statement about possible experiments and is in itself objectionable."[11] Thus we find this situation: von Mises and Jeffreys both assert that there is only one concept of probability that is of scientific importance and that can be taken as the basis of the Calculus of Probability. The first maintains that this concept is probability$_2$ and certainly not anything like probability$_1$; the second puts it just the other way round; and neither has anything but ironical remarks for the concept proposed by the other.

When we criticize the theory of probability proposed by an author, we must clearly distinguish between a rejection of his explicatum and a rejection of his explicandum. The second by no means follows from the first. Donald Williams, in his paper in this symposium[12], raises serious objections aginst the frequency theory of proability, especially in von Mises's form. The chief objection is that von Mises's explicatum for probability, viz., the limit of the relative frequency in an infinite sequence of events with a random distribution, is not accessible to empirical confirmation—unless it be supplemented by a theory of inductive probability, a procedure explicitly rejected by von Mises. I think Williams is right in this objection. This, however, means merely that the concept proposed by von Mises is not yet an adequate explicatum. On the other hand, I believe the frequentists are right in the assertion that their explicandum, viz., the statistical concept of probability$_2$, plays an important role in all branches of empirical science and especially in modern physics, and that therefore the task of explicating this concept is of great importance for science.

It would likewise be unjustified to reject the concept of probability$_1$ as an explicandum merely because the attempts so far made at an explication are not yet quite satisfactory. It must be admitted that the classical Laplacean definition is untenable. It defines probability as the ratio of the number of favorable cases to the total number of equipossible cases, where equipossibility is determined by the principle of insufficient reason (or indifference). This definition is in certain cases inapplicable,

[10] *Op. cit.*, p. 300.
[11] *Op. cit.*, p. 304.
[12] "On the Derivation of Probabilities from Frequencies."

in other cases it yields inadequate values, and in some cases it leads even to contradictions, because for any given proposition there are, in general, several ways of analyzing it as a disjunction of other, logically exclusive, propositions.[13] Modern authors, expecially Keynes, Jeffreys, and Hosiasson[14], proceed more cautiously, but at the price of restricting themselves to axiom systems which are rather weak and hence far from constituting an explicit definition. I have made an attempt to formulate an explicit definition of the concept of degree of confirmation (with numerical values) as an explicatum for probability$_1$, and to construct a system of metrical inductive logic based on that definition[15]. No matter whether this first attempt at an explication with the help of the methods of modern logic and in particular those of semantics will turn out to be satisfactory or not, I think there is no reason for doubting that an adequate explication will be developed in time through further attempts.

The distinction between the two concepts which serve as explicanda is often overlooked on both sides. This is primarily due to the unfortunate fact that both concepts are designated by the same familiar, but ambiguous word 'probability'. Although many languages contain two words (e.g., English 'probable' and 'likely', Latin '*probabilis*' and '*verisimilis*', French '*probable*' and '*vraisemblable*'), these words seem in most cases to be used in about the same way or at any rate not to correspond to the two concepts we have distinguished. Some authors (e.g., C. S. Peirce and R. A. Fisher) have suggested utilizing the plurality of available words for the distinction of certain concepts (different from our distinction); however, the proposals were made in an artificial way, without relation to the customary meanings of the words. The same would hold if we were to use the two words for our two concepts; therefore we prefer to use subscripts as indicated above.

Probability$_1$, in other words, the logical concept of confirmation in its different forms (positive, comparative, and metrical), has been explained in the preceding section. A brief explanation may here be given of probability$_2$, merely to make clear its distinction from probability$_1$. A typical example of the use of this concept is the following statement: "The prob-

[13] Williams' indications (*op. cit.*, pp. 450 and 469) to the effect that he intends to maintain Laplace's definition even in a simplified form and without the principle of indifference are rather puzzling. We have to wait for the full formulation of his solution, which his present paper does not yet give (*op. cit.*, p. 481), in order to see how it overcomes the well-known difficulties of Laplace's definition.

[14] Janina Hosiasson-Lindenbaum, "On Confirmation," *Journal of Symbolic Logic* Vol. V (1940), pp. 133-148.

[15] A book exhibiting this system is in preparation. The present paper is a modified version of a chapter of the book. The definition is explained and some of the theorems of my system of inductive logic are summarized in the paper "On Inductive Logic," which will appear in *Philisophy of Science*, Vol. XII, 1945.

ability$_2$ of casting an ace with this die is 1/6." Statements of this form refer to two properties (or classes) of events: (i) the reference property M_1, here the property of being a throw with this die; (ii) the specific property M_2, here the property of being a throw with any die resulting in an ace. The statement says that the probability$_2$ of M_2 with respect to M_1 is 1/6. The statement is tested by statistical investigations. A sufficiently long series of, say, n throws of the die in question is made, and the number m of these throws which yield an ace is counted. If the relative frequency m/n of aces in this series is sufficiently close to 1/6, the statement is regarded as confirmed. Thus, the other way round, the statement is understood as predicting that the relative frequency of aces thrown with this die in a sufficiently long series will be about 1/6. This formulation is admittedly inexact; but it intends no more than to indicate the meaning of 'probability$_2$' as an explicandum. To make this concept exact is the task of the explication; our discussion concerns only the two explicanda.

IV. THE LOGICAL NATURE OF THE TWO PROBABILITY CONCEPTS

On the basis of the preceding explanations, let us now characterize the two probability concepts, not with respect to what they mean but merely with respect to their logical nature, more specifically, with respect to the kind of entities to which they are applied and the logical nature of the simplest sentences in which they are used. (Since the pre-scientific use of both concepts is often too vague and incomplete, e.g., because of the omission of the second argument (viz., the evidence or the reference class), we take here into consideration the more careful use by authors on probability. However, we shall be more concerned with their general discussions than with the details of their constructed systems.) For the sake of simplicity, let us consider the two concepts in their metrical forms only. They may be taken also in their comparative and in their positive forms (as explained for probability$_1$, i.e., confirmation, in section II, and these other forms would show analogous differences. Probability$_1$ and probability$_2$, taken as metrical concepts, have the following characteristics in common: each of them is a function of two arguments; their values are real numbers belonging to the interval 0 to 1 (according to the customary convention, which we follow here). Their characteristic differences are as follows:

1. *Probability*$_1$ (degree of confirmation).

(a) The *two arguments* are variously described as events (in the literal sense, see below), states of affairs, circumstances, and the like. Therefore each argument is expressible by a declarative sentence and hence is, in our terminology, a proposition. Another alternative consists in taking as arguments the sentences expressing the propositions, describing the events, etc. If we choose this alternative, probability$_1$ is a semantical concept (as in section II). (Fundamentally it makes no great difference whether propositions or sentences are taken as arguments; but the second method has certain technical advantages, and therefore we use it for our discussion.)

(b) A simple *statement* of probability$_1$, i.e., one attributing to two given arguments a particular number as value of probability$_1$, is either L-true (logically true, analytic) or L-false (logically false, logically self-contradictory), hence in any case L-determinate, not factual (synthetic). Therefore, a statement of this kind is to be established by logical analysis alone, as has been explained earlier (section II). It is independent of the contingency of facts because it does not say anything about facts (although the two arguments do in general refer to facts).

2. *Probability*$_2$ (relative frequency).

(a) The *two arguments* are properties, kinds, classes, usually of events or things. [As an alternative, the predicate expressions designating the properties might be taken as arguments; then the concept would become a semantical one. In the present case, however, in distinction to (1), there does not seem to be any advantage in this method. On the contrary, it appears to be more convenient to have the probability$_2$ statements in the object language instead of the metalanguage; and it seems that all authors who deal with probability$_2$ choose this form.]

(b) A simple *statement* of probability$_2$ is factual and empirical, it says something about the facts of nature, and hence must be based upon empirical procedure, the observation of relevant facts. From these simple statements the theorems of a mathematical theory of probability$_2$ must be clearly distinguished. The latter do not state a particular value of probability$_2$ but say something about connections between probability$_2$ values in a general way, usually in a conditional form (for example: "if the values of such and such probabilities$_2$ are q_1 and q_2, then the value of a probability$_2$ related to the original ones in a certain way is such and such a function, say, product or sum, of q_1 and q_2"). These theorems are not factual but L-true (analytic). Thus a theory of probability$_2$, e.g., the system constructed by von Mises or that by Reichenbach, is not of an empirical but of a logico-mathematical nature; it is a branch of mathematics, like arithmetic, fundamentally different from any branch of empirical science, e.g., physics.

It is very important to distinguish clearly between *kinds of events* (war, birth, death, throw of a die, throw of this die, throw of this die yielding an ace, etc.) and *events* (Caesar's death, the throw of this die made yesterday at 10 A.M., the series of all throws of this die past and future). This distinction is doubly important for discussions on probability, because one of the characteristic differences between the two concepts is this: the first concept refers sometimes to two events, the second to two kinds of events (see 1(a) and 2(a)). Many authors of probability use the word 'event' (or the corresponding words 'Ereignis' and 'évènement') when they mean to speak, not about events, but about kinds of events. This usage is of long standing in the literature on probability, but it is very unfortunate. It has only served to reinforce the customary neglect of the fundamental difference between the two probability concepts which arose originally out of the ambiguous use of the word 'probability', and thereby to increase the general confusion in discussions on probability. The authors who use the term 'event' when they mean kinds of events get into trouble, of course, whenever they want to speak about specific events. The traditional solution is to say 'the happenings (or occurrences) of a certain event' instead of 'the events of a certain kind'; sometimes the events are referred to by the term 'single events'. But this phrase is rather misleading; the important difference between events and kinds of events is not the same as the inessential difference between single events (the first throw I made today with this die) and multiple or compound events (the series of all throws made with this die). Keynes, if I interpret him correctly, has noticed the ambiguity of the term 'event'. He says[16] that the customary use of phrases like 'the happening of events' is "vague and unambiguous," which I suppose to be a misprint for "vague and ambiguous"; but he does not specify the ambiguity. He proposes to dispense altogether with the term 'event' and to use instead the term 'proposition'. Subsequent authors dealing with probability$_1$, like Jeffreys, for example, have followed him in this use.

Many authors have made a distinction between two (or sometimes more) kinds of probability, or between two meanings of the word 'probability'. Some of these distinctions are quite different from the distinction made here between probability$_1$ and probability$_2$. For instance, a distinction is sometimes made between mathematical probability and philosophical probability; their characteristic difference appears to be that the first has numerical values, the second not. However, this difference seems hardly essential; we find a concept with numerical values and one without, in other words, both a metrical and a comparative concept on either side of our distinction between the two fundamentally different meanings of 'probability'. Another distinction has been made between subjective and objective probability. However, I believe that practically all authors

[16] *Op. cit.*, p. 5.

really have an objective concept of probability in mind, and that the appearance of subjectivist conceptions is in most cases caused only by occasional unfortunate formulations; this will soon be discussed.

Other distinctions which have been made are more or less similar to our distinction between probability$_1$ and probability$_2$. For instance, Ramsey[17] says: "... the general difference of opinion between statisticians who for the most part adopt the frequency theory of probability and logicians who mostly reject it renders it likely that the two schools are really discussing different things, and that the word 'probability' is used by logicians in one sense and by statisticians in another."

It seems that many authors have taken either probability$_1$ or probability$_2$ as their explicandum. I believe moreover that practically all authors on probability have intended one of these two concepts as their explicandum, despite the fact that their various explanations appear to refer to a number of quite different concepts.

For one group of authors, the question of their explicandum is easily answered. In the case of all those who support a frequency theory of probability, i.e., who define their explicata in terms of relative frequency (as a limit or in some other way), there can be no doubt that their explicandum is probability$_2$. Their formulations are, in general, presented in clear and unambiguous terms. Often they state explicitly that their explicandum is relative frequency. And even in the cases where this is not done, the discussion of their explicata leaves no doubt as to what is meant as explicandum.

This, however, covers only one of the various conceptions, i.e., explicata proposed, and only one of the many different explanations of explicanda which have been given and of which some examples were mentioned earlier. It seems clear that the other explanations do not refer to the statistical, empirical concept of relative frequency; and I believe that practically all of them, in spite of their apparent dissimilarity, are intended to refer to probability$_1$. Unfortunately, many of the phrases used are more misleading than helpful in our efforts to find out what their authors actually meant as explicandum. There is, in particular, one point on which many authors in discussions on probability$_1$, or on logical problems in general, commit a certain typical confusion or adopt incautiously other authors' formulations which are infected by this confusion. I am referring to what is sometimes called psychologism in logic.

Many authors in their general remarks about the nature of (deductive) logic say that it has to do with ways and forms of thinking or, in more cautious formulations, with forms of correct or rational thinking. In spite of these subjectivistic formulations, we find that in practice these

[17] F. P. Ramsey, *The Foundations of Mathematics*, 1931; see p. 157.

authors use an objectivistic method in solving any particular logical problem. For instance, in order to find out whether a certain conclusion follows from given premises, they do not in fact make psychological experiments about the thinking habits of people but rather analyze the given sentences and show their conceptual relations. In inductive logic or, in other words, the theory of probability$_1$, we often find a similar psychologism. Some authors, from Laplace and other representatives of the classical theory of probability down to contemporary authors like Keynes and Jeffreys, use subjectivistic formulations when trying to explain what they take as their explicandum; they say that it is probability in the sense of degree of belief or, if they are somewhat more cautious, degree of reasonable or justified belief. However, an analysis of the work of these authors comes to quite different results if we pay more attention to the methods the authors actually use in solving problems of probability than to the general remarks in which they try to characterize their own aims and methods. Such an analysis, which cannot be carried out within this paper, shows that most and perhaps all of these authors use objectivistic rather than subjectivistic methods. They do not try to measure degrees of belief by actual, psychological experiments, but rather carry out a logical analysis of the concepts and propositions involved. It appears, therefore, that the psychologism in inductive logic is, just like that in deductive logic, merely a superficial feature of certain marginal formulations, while the core of the theories remains thoroughly objectivistic. And, further, it seems to me that for most of those authors who do not maintain a frequency theory, from the classical period to our time, the objective concept which they take as their explicandum is probability$_1$, i.e., degree of confirmation.

V. EMPIRICISM AND THE LOGICAL CONCEPT OF PROBABILITY

Many empiricist authors have rejected the logical concept of probability$_1$ as distinguished from probability$_2$ because they believe that its use violates the principle of empiricism and that, therefore, probability$_2$ is the only concept admissible for empiricism and hence for science. We shall now examine some of the reasons given for this view.

The concept of probability$_1$ is applied also in cases in which the hypothesis h is a prediction concerning a particular "single event," e.g., the prediction that it will rain tomorrow or that the next throw of this die will yield an ace. Some philosophers believe that an application of this kind violates the principle of verifiability (or confirmability). They might say, for example: "How can the statement 'the probability of rain tomorrow on the evidence of the given meteorological observations is one-fifth' be verified? We shall observe either rain or not-rain tomorrow, but we shall not observe anything that can verify the value one-fifth." This objection, however, is based on a misconception concerning the nature of the probability$_1$ statement. This statement does not ascribe the probability$_1$ value 1/5 to tomorrow's rain but rather to a certain logical relation between the prediction of rain and the meteorological report. Since the relation is logical, the statement is, if true, L-true; therefore it is not in need of verification by observation of tomorrow's weather or of any other facts.

It must be admitted that earlier authors on probability have sometimes made inferences which are inadmissible from the point of view of empiricism. They calculated the value of a logical probability and then inferred from it a frequency, hence making an inadvertent transition from probability$_1$ to probability$_2$. Their reasoning might be somewhat like this: "On the basis of the symmetry of this die the probability of an ace is 1/6; therefore, one-sixth of the throws of this die will result in an ace." Later authors have correctly criticized inferences of this kind. It is clear that from a probability$_1$ statement a statement on frequency can never be inferred, because the former is purely logical while the latter is factual. Thus the source of the mistake was the confusion of probability$_1$ with probability$_2$. The use of probability$_1$ statements cannot in itself violate the principle of empiricism so long as we remain aware of the fact that those statements are purely logical and hence do not allow the derivation of factual conclusions.

The situation with respect to both objections just discussed may be clarified by a comparison with deductive logic. Let h be the sentence 'there will be rain tomorrow' and j the sentence 'there will be rain and wind tomorrow'. Suppose somebody makes the statement in deductive logic: "h follows logically from j." Certainly nobody will accuse him of apriorism either for making the statement or for claiming that for its verification no factual knowledge is required. The statement "the probability$_1$ of h on the evidence e is 1/5" has the same general character as the former statement; therefore it cannot violate empiricism any more than the first. Both statements express a purely logical relation between two sentences. The difference between the two statements is merely this: while the first states a complete logical implication, the second states only, so to speak, a partial logical implication; hence, while the first belongs to deductive logic, the second belongs to inductive logic. Generally speaking, the assertion of purely logical sentences, whether in deductive or in inductive logic, can never violate empiricism; if they are false, they violate the rules of logic. The principle of empiricism can be violated only by the assertion of a factual (synthetic) sentence without a sufficient empirical foundation, or by the thesis of apriorism when it contends that for knowledge with respect to certain factual sentences no empirical foundation is required.

According to Reichenbach's view[18], the concept of logical probability or weight, in order to be in accord with empiricism, must be identified with the statistical concept of probability. If we formulate his view with the help of our terms with subscripts, it says that probability$_1$ is identical with probability$_2$, or, rather, with a special kind of application of it. He argues for this "identity conception" against any "disparity conception," like the one presented in this paper, which regards the two uses of 'probability' as essentially different. Reichenbach tries to prove the identity conception by showing how the concept which we call probability$_1$, even when applied to a "single event," leads back to a relative frequency. I agree that in certain cases there is a close relationship between probability$_1$ and relative frequency. The decisive question is, however, the nature of this relationship. Let us consider a simple example. Let the evidence e say that among 30 observed things with the property M_1 20 have been found to have the property M_2, and hence that the relative frequency of M_2 with respect to M_1 in the observed sample is 2/3; let e say, in addition, that a certain individual b not belonging to the sample is M_1. Let h be the prediction that b is M_2. If the degree of confirmation c is defined in a suitable way as an explicatum for probability$_1$, $c(h,e)$ will be equal or close to 2/3; let us assume for the sake of simplicity that $c = 2/3$.[19] However, the fact that, in this case, the value of c or probability$_1$ is equal to a certain relative frequency by no means implies that probability$_1$ is here the same as probability$_2$; these two concepts remain fundamentally different even in this case. This becomes clear by the following considerations (i) to (iv).

(i) The c-statement '$c(h,e) = 2/3$' does not itself state a relative frequency although the value of c which it states is calculated on the basis of a known relative frequency and, under our assumptions, is in this case exactly equal to it. A temperature is sometimes determined by the volume of a certain body of mercury and is, under certain conditions, equal to it; this, however, does not mean that temperature and volume are the same concept. The c-statement, being a purely logical statement, cannot possibly state a relative frequency for two empirical properties like M_1 and M_2. Such a relative frequency can be stated only by a factual sentence; in the example, it is stated by a part of the factual sentence e. The c-statement

[18] Hans Reichenbach, *Experience and Prediction*, 1938, see §§ 32–34.

[19] According to Reichenbach's inductive logic, in the case described $c = 2/3$. According to my inductive logic, c is close to but not exactly equal to 2/3. My reason for regarding a value of the latter kind as more adequate has been briefly indicated in the paper mentioned above "On Inductive Logic," § 10. For our present discussion, we may leave aside this question.

does not imply either e or the part of e just mentioned; it rather speaks about e, stating a logical relation between e and h. It seems to me that Reichenbach does not realize this fact sufficiently clearly. He feels, correctly, that the c-value 2/3 stated in the c-statement is in some way based upon our empirical knowledge of the observed relative frequency. This leads him to the conception, which I regard as incorrect, that the c-statement must be interpreted as stating the relative frequency and hence as being itself a factual, empirical statement. In my conception, the factual content concerning the observed relative frequency must be ascribed, not to the c-statement, but to the evidence e referred to in the c-statement.

(ii) The relative frequency 2/3, which is stated in e and on which the value of c is based, is not at all a probability$_2$. The probability$_2$ of M_2 with respect to M_1 is the relative frequency of M_2 with respect to M_1 in the whole sequence of relevant events. The relative frequency stated by e, on the other hand, is the relative frequency observed within the given sample. It is true that our estimate of the value of probability$_2$ will be based on the observed relative frequency in the sample. However, observations of several samples may yield different values for the observed relative frequency. Therefore we cannot identify observed relative frequency with probability$_2$, since the latter has only one value, which is unknown. (I am using here the customary realistic language as it is used in everyday life and in science; this use does not imply acceptance of realism as a metaphysical thesis but only of what Feigl calls "empirical realism."[20])

(iii) As mentioned, an estimate of the probability$_2$, the relative frequency in the whole sequence, is based upon the observed relative frequency in the sample. I think that, in a sense, the statement '$c(h,e) = 2/3$' itself may be interpreted as stating such an estimate; it says the same as: "The best estimate on the evidence e of the probability$_2$ of M_2 with respect to M_1 is 2/3." If somebody should like to call this a frequency interpretation of probability$_1$, I should raise no objection. It need, however, be noticed clearly that this interpretation identifies probability$_1$ not with probability$_2$ but with the best estimate of probability$_2$ on the evidence e; and this is something quite different. The best estimate may have different values for different evidences; probability$_2$ has only one value. A statement of the best estimate on a given evidence is purely logical; a statement of probability$_2$ is empirical. The reformulation of the statement on probability$_1$ or c in terms of the best estimate of probability$_2$ may be helpful in showing the close connection between the two probability concepts. This formulation must, however, not be regarded as eliminating

[20] Herbert Feigl, "Logical Empiricism," in *Twentieth Century Philosophy*, ed. D. Runes, 1943, pp. 373–416; see pp. 390 f.

probability_1. The latter concept is still implicitly contained in the phrase "the best estimate," which means nothing else but "the most probable estimate," that is, "the estimate with the highest probability_1." Generally speaking, any estimation of the value of a physical magnitude (length, temperature, probability_2, etc.) on the evidence of certain observations or measurements is an inductive procedure and hence necessarily involves probability_1, either in its metrical or in its comparative form.

(iv) The fundamental difference between probability_1 and probability_2 may be further elucidated by analyzing the sense of the customary references to *unknown probabilities*. As we have seen under (ii), the value of a certain probability_2 may be unknown to us at a certain time in the sense that we do not possess sufficient factual information for its calculation. On the other hand, the value of a probability_1 for two given sentences cannot be unknown in the same sense. (It may, of course, be unknown in the sense that a certain logico-mathematical procedure has not yet been accomplished, that is, in the same sense in which we say that the solution of a certain arithmetical problem is at present unknown to us.) In this respect also, a confusion of the two concepts of probability has sometimes been made in formulations of the classical theory. This theory deals, on the whole, with probability_1; and the principle of indifference, one of the cornerstones of the theory, is indeed valid to a certain limited extent for this concept. However, this principle is absurd for probability_2, as has often been pointed out. Yet the classical authors sometimes refer to unknown probabilities or to the probability (or chance) of certain probability values, e.g., in formulations of Bayes' theorem. This would not be admissible for probability_1, and I believe that here the authors inadvertently go over to probability_2. Since a probability_2 value is a physical property like a temperature, we may very well inquire into the probability_1, on a given evidence, of a certain probability_2 (as in the earlier example, at the end of (iii)). However, a question about the probability_1 of a probability_1 statement has no more point than a question about the probability_1 of the statement that $2 + 2 = 4$ or that $2 + 2 = 5$, because a probability_1 statement is, like an arithmetical statement, either L-true or L-false; therefore its probability_1, with respect to any evidence, is either 1 or 0.

VI. PROBABILITY AND TRUTH

It is important to distinguish clearly between a concept characterizing a thing independently of the state of our knowledge (e.g., the concept 'hard') and the related concept characterizing our state of knowledge with respect to the thing (e.g., the concept 'known to be hard'). It is true that a person will, as a rule, attribute the predicate 'hard' to a thing *b* only if he knows it to be hard, hence only if he is prepared to attribute to it also the predicate 'known to be hard'. Nevertheless, the sentences '*b* is hard' and '*b* is known to be hard' are obviously far from meaning the same. One point of difference becomes evident when we look at the sentences in their complete form; the second sentence, in distinction to the first (if we regard hardness as a permanent property), must be supplemented by references to a person and a time point: '*b* is known to *X* at the time *t* to be hard'. The distinction between the two sentences becomes more conspicuous if they occur within certain larger contexts. For example, the difference between the sentences '*b* is not hard' and '*b* is not known to *X* at the time *t* to be hard' is clear from the fact that we can easily imagine a situation where we would be prepared to assert the second but not the first.

The distinction just explained may appear as obvious beyond any need of emphasis. However, a distinction of the same general form, where 'true' is substituted for 'hard', is nevertheless often neglected by philosophers. A person will, in general, attribute the predicate 'true' to a given sentence (or proposition) only if he knows it to be true, hence only if he is prepared to attribute to it also the predicate 'known to be true' or 'established as true' or 'verified'. Nevertheless 'true' and 'verified (by the person *X* at the time *t*)' mean quite different things; and so do 'false' and 'falsified' (in the sense of 'known to be false', 'established as false'). A given sentence is often neither verified nor falsified; nevertheless it is either true or false, whether anybody knows it or not. (Some empiricists shy away from the latter formulation because they believe it to involve an anti-empiricist absolutism. This, however, is not the case. Empiricism admits as meaningful any statement about unknown fact and hence also about unknown truth, provided only the fact or the truth is know*able*, or confirmable.) In this way an inadvertent confusion of 'true' and 'verified' may lead to doubts about the validity of the principle of excluded middle. The question of whether and to what extent a confusion of this kind has actually contributed to the origin of some contemporary philosophical doctrines rejecting that principle is hard to decide and will not be investigated here.

A statement like 'this thing is made of iron' can never be verified in the strictest sense, i.e., definitively established as true so that no possibility remains of refuting it by future experience. The statement can only be more or less confirmed. If it is highly confirmed, that is to say, if strong evidence for it is found, then it is often said to be verified; but this is a weakened, non-absolutistic sense of the term. I think it is fair to say that most philosophers, and at least all empiricists, agree today that the concept 'verified' in its strict sense is not applicable to statements about physical things. Some philosophers, however, go further; they say that, because we can never reach absolutely certain knowledge about things, we ought to abandon the concept of truth. It seems to me that this view is due again to an unconscious confusion of 'true' and 'verified'.[21] Some of these philosophers say that, in order to avoid absolutism, we should not ask whether a given statement is true but only whether it has been confirmed, corroborated, or accepted by a certain person at a certain time.[22] Others think that 'true' should be abandoned in favor of 'highly confirmed' or 'highly probable'. Reichenbach[23] has been led by considerations of this kind to the view that the values of probability (the logical concept of probability$_1$) ought to take the place of the two truth-values, truth and falsity, of ordinary logic, or, in other words, that probability logic is a multivalued logic superseding the customary two-valued logic. I agree with Reichenbach that here a concept referring to an absolute and unobtainable maximum should be replaced by a concept referring to a high degree in a continuous scale. However, what is superseded by 'highly probable' or 'confirmed to a high degree' is the concept 'confirmed to the maximum degree' or 'verified', and not the concept 'true'.

Values of probability$_1$ are fundamentally different from truth-values. Therefore inductive logic, although it introduces the continuous scale of probability$_1$ values, remains like deductive logic two-valued. While it is true that to the multiplicity of probability$_1$ values in inductive logic only a dichotomy corresponds in deductive logic, nevertheless, this dichotomy is not between truth and falsity of a sentence but between L-implication and non-L-implication for two sentences. If, to take our previous example, $c(h,e) = 2/3$, then h is still either true or false and does not have an intermediate truth-value of 2/3.

It has been the chief purpose of this paper to explain and discuss the two concepts of probability in their role as explicanda for theories of probability. I think that in the present situation clarification of the explicanda is the most urgent task. When every author has not only a clear understanding of his own explicandum but also some insight into the existence, the importance, and the meaning of the explicandum on the other side, then it will be possible for each side to concentrate entirely on

[21] I have given earlier warnings against this confusion in "Wahrheit und Bewährung," *Actes du Congrès International de Philosophic Scientifique*, Paris, 1936, Vol. IV, pp. 1–6; and in *Introduction to Semantics*, p. 28.

[22] See, e.g., Otto Neurath, "Universal Jargon and Terminology," *Proceedings Aristotelian Society*, 1940–1941, pp. 127–148, see esp. pp. 138 f.

[23] *Op. cit.* (*Experience*), §§ 22, 35.

the positive task of constructing an explication and a theory of the chosen explicatum without wasting energy in futile polemics against the explicandum of the other side.

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EXTRACTO

Las diversas teorías de la probabilidad tienen por fin substituir el concepto pre-científico de probabilidad por un concepto exacto y científico. Sin embargo, hay *dos* sentidos del término "probabilidad" empleados generalmente; ambos están conectados el uno con el otro, aunque son fundamentalmente diferentes. Primero, hay el concepto lógico de probabilidad, o sea el grado de confirmación de una hipótesis sobre la base de las pruebas aducidas. La proposición que aplica este concepto no se basa en la observación de los hechos, sino en el análisis lógico. Segundo, hay el concepto estadístico de probabilidad, o sea la relativa frecuencia. La proposición que emplea este concepto es sintética y empírica. Ambos conceptos son importantes para la ciencia. El primero constituye la base de la lógica inductiva. El segundo es útil en las investigaciones estadísticas. Muchos autores que se ocupan de sistematizar uno de estos dos conceptos, no se percatan de la importancia y aun de la existencia del otro concepto. Cuando el uno y el otro sean claramente reconocidos, se evitarán muchas controversias inútiles.