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Remarks on Probability

by RUDOLF CARNAP

UNIVERSITY OF CALIFORNIA AT LOS ANGELES

THE purpose of this paper is, first, to give some brief indications of the development of the field of inductive logic during the last decade, and of the present situation as it appears to me. Secondly, I shall specify some particular points on which my views have changed since I wrote *The Logical Foundations of Probability* (1950). (I shall refer to this book as simply "the book"; references to sections or pages without indication of title are likewise meant to refer to this book.) The major features of my theory, as explained in the book, are still maintained today. This holds for both the basic philosophical conception of the nature of logical probability, explained in the first half of the book, and the formal system constructed in the second half.

AUTHOR'S NOTE: This paper is a slightly modified version of the preface to the second edition of my *The Logical Foundations of Probability* (Chicago: University of Chicago Press, 1962). In this edition, the text of the first edition of 1950 is reprinted without change, except for a number of small corrections. This paper is printed with the kind permission of the University of Chicago Press.

Only a small part of the results of the work I have done in the meantime, in collaboration with my friends, especially John G. Kemeny and Richard C. Jeffrey, has been published so far. I have abandoned my original plan of writing a companion volume to the book, as announced in the preface to the first edition. There is now such a rapid development and change in this field that a comprehensive book, trying to describe the present situation, would probably be outdated before its appearance. Therefore we are planning instead the publication of a series of small volumes with the tentative title *Studies in Probability and Inductive Logic*, each volume containing several articles, some expository, others in the nature of technical research reports.

Among the future topics announced in the original preface was the construction of a parametric system of inductive methods, i.e., c-functions and corresponding estimate functions, called the lambda-system. I gave an exposition of this system in *The Continuum of Inductive Methods* [8; see the Bibliography]. This monograph contains also a discussion of estimate functions for relative frequency, points out some serious disadvantages of certain estimate functions widely used in mathematical statistics, and proposes new functions avoiding these disadvantages.

The axiom system of inductive logic given in *Continuum* has since been further developed. A more comprehensive form of it will appear in "Replies and Systematic Expositions" [12, §26] and in the Carnap-Stegmüller volume [13, Anhang B]. The system is still in the process of change and growth.

My conception of logical probability (called "probability," in the book) has some basic features in common with those of other authors, e.g., John Maynard Keynes, Frank P. Ramsey, Harold Jeffreys, Bruno De Finetti, B. O. Koopman, Georg Henrik von Wright, I. J. Good, and Leonard J. Savage, to mention only the names more widely known. All these conceptions share the following features. They are different from the frequency conception ("probability$_2$" in the book). They emphasize the relativity of probability with respect to the evidence. (For this reason, some of the authors call their conception "subjective"; however, this term does not seem quite appropriate for logical probability; see pp. 43f, 239f.) Further, the numerical probability of an unknown possible event can be regarded as a fair betting quotient. And finally, if logical relations (e.g., logical implication or incompatibility) hold between given propositions, then their probabilities must, according to these conceptions, satisfy certain conditions (usually laid down by axioms) in order to assure the rationality of the beliefs and the actions, e.g., bets, based upon these probabilities. I have the impression that the number of those who think and work in the direction indicated is increasing. This is certainly the case among philosophers. But it seems that also among those who work in mathematical statistics more and more begin to regard the customary exclusive use of the frequency concept of probability as unsatisfactory and are searching for another concept.

Almost every author in this field, including myself, worked at the beginning practically alone, following his own particular line. But by now there is more mutual influence. Certainly I and my friends have learned much from other authors, both in the purely mathematical theory of probability and in the methodology of its application. Often a certain approach to a problem seemed to us the best or at least acceptable at a certain time, but a few years later we saw that it had to be abandoned or modified. The change required was sometimes brought about by a clarification of the basic ideas, sometimes by the discovery of a new approach to a particular problem, sometimes by newly proven concrete mathematical results. Thus there is rapid change and, we hope, progress in this field.

I hold the view, in common with some, but not all, of the authors mentioned, that the concept of logical probability may serve as the basis for the construction of a system of inductive logic, understood as the logical theory of all inductive reasoning. Moreover, in contrast to the customary view that the outcome of a process of inductive reasoning about a hypothesis h on the basis of given evidence e consists in the acceptance (or the rejection, or the temporary suspension) of h, I believe that the outcome should rather be the finding of the numerical value of the probability of h on e. Although a judgment about h, e.g., a possible result of a planned experiment, is usually not formulated explicitly as a probability statement, I think a statement of this kind is implicitly involved. This means that a rational reconstruction of the thoughts and decisions of an investigator could best be made in the framework of a probability logic. It seems to me, furthermore, that the indicated conception of the form of inductive reasoning makes it possible to give a satisfactory answer to Hume's objection.[1]

In the following I shall explain some special points in which my views have changed since the time when I wrote the book.

A. The meaning of logical probability (probability,) was informally explained in §41 in several ways: (a) as the degree to which a hypothesis h is confirmed or supported by the evidence e, (b) as a fair betting quotient, and (c) as an estimate of relative frequency. Even at that time I regarded (a) as less satisfactory than (b) or (c); today I would avoid formulations of the kind (a) because of their ambiguity (see points B and C below). Although the concept of logical probability in the sense here intended is a purely logical concept, I think that the meaning of statements like "the probability of h with respect to e is ⅔" can best be characterized by explaining their use, in combination with the concept of utility, in the rule for the determination of rational decisions (§51 A, rule R_5). The explanation of probability as a betting quotient is a simplified special case of this rule.

B. Two triples of concepts. In the book I distinguished three kinds of scientific concepts (§4) : classificatory, comparative, and quantitative concepts; e.g., (1) "x is warm," (2) "x is warmer than y," (3) "the temperature of x is u" ("T (x) = u"). If the quantitative concept T is available, (1) and (2) may be formulated as follows: (1) "T (x) > b," where b is a fixed number chosen as the lower boundary for "warm"; (2) "T(x) > T(y)."

But I specified only one triple of concepts connected with probability, (§8). At present it seems to me more appropriate to set up two triples of concepts, I and II. The concepts of I are concerned with the question how probable the hypothesis h is on the basis of the evidence e. The concepts of II relate to the question as to whether and how much the probability of h is increased when new evidence i is acquired (in addition to the prior evidence which, for simplicity, we shall take here as tautological). Let us say (for the present discussion only) "h is firm" for "h is probable," and "h is made firmer" for "h is made more probable"; then we may call the concepts of I "concepts of firmness," and those of II "concepts of the increase in firmness." I shall now specify, in each of the triples I and II, (1) the classificatory concept, (2) (a) the general comparative concept, and (3) the quantitative concept; under (2) I add two special cases, because they are used more frequently than the general concept, viz., (b) the comparison of two additional evidences i and i′ for the same hypothesis h, and (c) the comparisons of two hypotheses h and h′ with respect to the same evidence i. For each of these concepts a formulation in terms of c is given in the last column; c is to be understood as probability, in the sense explained above under A. Thus these formulas will indicate clearly what is meant by each of the listed concepts.

I. The three concepts of firmness:

I 1. h is *firm* (on the basis of) e.	c(h,e) > b, where b is a fixed number.
I 2. (a) h on e is *firmer* than h′ on e′.	c(h,e) > c(h′,e′).
(b) h is firmer on e than on e′.	c(h,e) > c(h,e′).
(c) h is firmer than h′ on e.	c(h,e) > c(h′,e).
I 3. The (degree of) *firmness* of h on e is u.	c(h,e) = u.

II. The three concepts of increase in firmness. For the sake of simplicity, we shall consider here only the *initial* increase in firmness, i.e., the case that the prior evidence is tautological. The exact interpretation of these concepts depends upon the way in which we measure the increase in firmness, i.e., the increase of c. This can be done by different functions (compare the different relevance functions discussed in Chapter VII). For the present survey let us take the simplest function of this kind, the difference; we define: $D(h,i) =_{Df} c(h,i) — c(h,t)$.

II 1. h is *made firmer* by i.	$D(h,e) > 0$; hence: $c(h,i) > c(h,t)$.
11 2. (a) h is *made firmer* by i *more* than h′ by i′.	$D(h,i) > D(h',i')$.
(b) h is made firmer by i more than by i′	$D(h,i) > D(h,i')$; hence: $c(h,i) > c(h,i')$.
(c) h is made firmer by i more than h′.	$D(h,i) > D(h',i)$.
II 3. The (amount of) *increase in firmness* of h by i is u.	$D(h,i) = u$.

Since we took t as the prior evidence, these are concepts of initial increase in firmness. We see that the classificatory concept II 1 is the same as initial positive relevance (D65-2a). (The general concepts of relevance would be relative to a variable prior evidence e. In this case the concept II 1 would mean "$c(h,e \cdot i) > c(h,e)$" and thus be the same as the general concept of positive relevance, D65-la.)

(Note, incidentally, that for the special case 2b of the comparative concept with one hypothesis, the concept II 2b coincides with I 2b (this holds likewise if we take a variable prior evidence e instead of t). But this result depends upon the choice of the function by which we measure the amount of increase in firmness; it holds also for the quotient, but not generally for other functions.)

The triple of concepts (1), (2a), and (3), both under I and under II, are analogous to the triple "warm," "warmer," and "temperature." We see this easily when we compare the formulas with 'T' given for the latter concepts at the beginning of B, with the formulas given here under I and II, respectively.

I gave a detailed discussion of the classificatory concept in §86, and of the comparative concept in the first sections of Chapter VII. (Later, under D and E I shall return to the problems discussed in these two sections.) If we wish to ascertain what I actually meant by these concepts, we should look, not at the paraphrases in words (which were sometimes misleading, as we shall presently find, under C), but rather at the given corresponding formulas with "c" (which was always meant in the sense of probability,). Thus the formula "$c(h,i) > c(h,t)$" (p. 464) indicates that the classificatory concept was meant in the sense of II 1; and similarly the formula "$c(h,e)$ $c(h',e')$" (p. 431, with "$\geq$" rather than ">" for reasons of technical convenience) shows that the comparative concept was meant in the sense of I 2a. Since the quantitative concept was always meant as I 3, my triple of concepts consisted of II 1, I 2, and I 3. Thus I recognize now that my three concepts, though each of them is an interesting concept, did not fit together in the way I had intended, i.e., as analogues to "warm," "wanner," and "temperature," respectively. If I 1 is taken instead of II 1, we have a fitting triple of the kind I. It is curious to see that in my discussion of Hempel's investigations I considered I 1 as an alternative form of the classificatory concept, but explained my reasons for preferring II 1 (see under D below), which is indeed the more interesting concept of the two.

C. *Terminological questions*. When I examine today, from the point of view of the distinction between concepts of firmness and concepts of increase in firmness, the paraphrases and informal explanations I gave in the book for various concepts, I realize that they are often ambiguous and may sometimes even be misleading. For example, the comparative concept was meant (as I mentioned above) in the sense of I 2, thus as a comparison of firmness. However, my formulations ". . . more strongly confirmed (or supported . . . corroborated, etc.) . . ." (p. 22, (ii) (a)) may rather suggest a comparison of increase in firmness in the sense of II 2.

In view of the fact that the verb "to confirm" is ambiguous and has perhaps the connotation of "making firmer" even more often than that of "making firm," it may seem advisable to use expressions of the form "e is confirming evidence for h" or "h is confirmed by e," if at all, only in the sense of II 1 (as I did in §86), and not in that of I 1.

I am in doubt what to propose for the concept I 3. It seems to me feasible, in spite of the ambiguity of "to confirm," to keep the term "degree of conformation" as a technical term for I 3, as I did throughout the book. If we do, we have to keep in mind that "degree of confirmation" means, not the amount of increase in firmness, but the degree of firmness that the hypothesis has on its present basis (after being made either firmer or less firm by the additional confirming or disconfirming evidence).[2]

Another possibility would be to take the good old term "probability" also as a technical term (not only, in the form of "probability_1," as a non-technical term for the explicandum, as I did in the book). I would certainly have liked from the beginning to follow Keynes and Jeffreys in using this term also as a technical term for the explicatum. But I decided with regret not to do so, because in the literature of mathematical statistics, which had grown in the last decades to enormous size, the term "probability" is almost exclusively used in the different sense of probability_2, the frequency concept. Although I regarded this use as an illegitimate usurpation, since I believe the classical authors meant mostly not probability_2, but something like prob-

$ability_1$, (§12B), it seemed to me then inadvisable to use the term “probability” in a sense deviating from that prevalent in statistics. Today the situation looks different. As mentioned at the beginning of this paper, there are now a number of authors whose concepts of probability are similar to $probability_1$. They usually emphasize at the beginning the difference between their concept of probability and the frequency concept, sometimes by adjoining to the word “probability” a qualifying adjective like “subjective,” “personal,” or “intuitive.” But then they use in the body of their work mostly the simple term “probability.” I think I would prefer to do the same if I should decide to give up the term “degree of confirmation.”

Under B above, I explained that my three concepts did not form a triple of the kind intended; here under C I pointed out that my informal explanations in words were often not appropriate. I wish to emphasize that these two points do not touch the content of my system itself, consisting of the formal definitions and theorems .[3]

D. *The classificatory concept of confirmation* was discussed in §86. There I paraphrased this concept thus: “i is confirming evidence for h” (p. 463, the second form). In this case the formulation is appropriate, because I had in mind, not I 1, but II 1; accordingly I gave as the corresponding formula with “c”: “c(h,i) > c(h,t)” (p. 464, formula (4)), as in II 1 above. Later, in my discussion of Hempel’s investigation, I surmised (p. 475) that his original explicandum was the same as mine (i.e., II 1), for example, when he referred to “data favorable for h” or said that i “is strengthening h.” But then I pointed out that at some other places the kind of arguments he gave made it likely that he had inadvertently shifted to another explicandum, viz., “the degree of confirmation of h on i is greater than r, where r is a fixed value, perhaps 0 or ½” (p. 475), which is I 1. Thus, if my assumption about Hempel’s two explicanda is correct, there was a lack of distinction between I and II, possibly influenced, as in my own case, by the ambiguity of the word “confirmation.”

The special aim of my investigation of the classificatory concept in §86 was to find a definition using no quantitative concepts like c, but only L-concepts. I gave a definition of this kind for a concept C′ (p. 465, formula (8)). However, I did not accept C′ as an explicatum, because I found that this concept was too narrow; I showed this by two counterexamples (p. 466). I am today still of the opinion (expressed in the last paragraph of §86) that to find a nonquantitative explicatum is chiefly of interest for those who are skeptical about the possibility of a quantitative explicatum of $probability_1$. Within quantitative inductive logic, we have a detailed theory of relevance (Chapter VI), which contains the concept II 1 as positive relevance, defined quantitatively. As to the concept I 1, it may be useful for everyday communication, e.g., "it is probable that it will rain tomorrow," but its usefulness for scientific work is hardly higher than that of the concept "warm" in physics.

E. *The comparative concept*, understood in the sense of I 2, was investigated in Chapter VII. In this case likewise I searched for an explication in nonquantitative terms. I proposed a definition of this kind (D81-1). But Bar-Hillel showed [2] that my explicatum was too narrow. He used the same two counterexamples which I had used in §86 against C′. To my earlier remark (p. 467) that it seemed doubtful whether a simple definition based upon L-terms could be found for the *classificatory* concept, I added later [10, p. 318] that in the case of the comparative concept the reasons for doubt are even stronger. At any rate, such a definition would have to go far beyond simple L-relations among the four sentences involved and refer also to the internal structure of the sentences. A definition of this kind would presumably not be simple. The task of finding such a definition does not seem very important today, when we recognize that a quantitative theory can be developed.

Aside from the question of an explicit, nonquantitative definition, the comparative concept may be of interest as a primitive concept in an axiom system. Many authors on probability (in a nonfrequency sense) begin with a system of comparative axioms. This procedure has advantages in view of the fact that on the basis of our intuitions we often find it easier to make a comparative judgment than a quantitative one. I have proposed to add to the usual comparative axioms some new ones, among them the axiom of symmetry with respect to individual constants and the axiom of instantial relevance [see 10, p. 316].

F. *The requirement of logical independence of atomic sentences* (§18B) has been abolished. Kemeny [15] and Bar-Hillel [1] pointed out, independently of each other, that this requirement would exclude any primitive two-place predicate that, in virtue of its meaning, possesses some structural property. For example, if the primitive predicate "W" designates the relation Warmer and thus is asymmetric by its meaning, then the atomic sentences "Wab" and "Wba" are incompatible and hence any state-description containing both would not represent a possible case. However, we can admit as primitive predicates those of the kind just indicated and also one-place predicates with meaning relations holding among them (e.g., predicates designating different colors and hence being incompatible), if we apply the following procedure, which was first proposed by Kemeny. We require that all such meaning relations and structural properties are expressed by special postulates, which I call meaning postulates or A-postulates [9; cf. Kemeny, 16, 17]. Then we define as admissible state-descriptions those in which all A-postulates hold. The analytic (or A-true) sentences, i.e., those which are true in virtue of meanings alone, are defined as those holding in all admissible state-descriptions. In inductive logic we take into account only the admissible state-descriptions. We assign the m-value 1 not only to all L-true sentences (T57-1d) but to all A-true sentences.

C. *The requirement of completeness* for the set of primitive predicates (§18B) has been abolished. Special axioms are adopted which assure the invariance of c-values with respect to an extension of the language by the addition of either new individual constants or new families of primitive predicates.

Invariance with respect to an addition of a new family did not hold in my original system But this invariance becomes possible by a modification of the treatment of the primitive predicates. I shall now indicate this modification; for the sake of simplicity I shall refer to one-place predicates only.

The primitive predicates are classified into families (this procedure was indicated in §18C, but not applied in the book). For example, there may be a family of colors, another family of shapes, and the like. We make sure, either by A-postulates or by a suitable special form of the state-descriptions, that in any admissible state-description, for each individual, one and only one of the predicates of any given family holds. Chapters IV through IX remain mostly unchanged. But at those places (mostly in §107A) where the language systems L^P and the Q-predicates are dealt with, the following changes have to be made. The explanations and results at those places, and furthermore in the Appendix concerning the function c* and in the monograph [8] concerning the lambda-system, are now to be understood as restricted to cases involving predicates of any one family only. The number p of independent primitive predicates is to be disregarded, and the number k of Q-predicates is to be understood as the number of predicates of the family in question. (For typographical reasons I write here "p" and "k" instead of the corresponding Greek letters used in the book.)

As an example, consider any formula containing "p" or "k" or both, e.g., a formula in §110 of the book about c*, say (6) or (7), or a formula in *Continuum* about a c-function of the lambda-system, e.g. (11-4). Let us apply this formula to the case p = 3, hence k = 8. The result was originally interpreted as referring to the following situation: we have three independent primitive predicates, say "P_1, ," "P_2," and "P_3," and hence eight Q-predicates (or, in the terminology of §18C, which I now prefer: we have three families containing two predicates each; the first family contains "P_1" and its negation; the second and third are analogous). Today I would interpret the formula differently. It is applicable to the described situation as an approximation only (since it neglects the analogy influence); but it holds exactly for another situation, viz., one family of eight primitive predicates, for example, for eight different colors exhausting the color universe. In order to obtain exact c-values for the former situation, we need a method for three families, a method different from all those discussed in the book or in *Continuum*. Together with Kemeny I have developed a general method for an arbitrary number n of families $F^1 \ldots F^n$, where the family F^m (m = 1, . . ., n) contains any number k_m of primitive predicates. The formula for two families is given and explained in Carnap-Stegmüller [13, Anhang B VIII].

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NOTES

[1] I have explained this view in the last paragraphs of "The Aim of Inductive Logic" [11].

[2] I used the term "degree of confirmation" first for a pragmatical concept referring to a person at a given time [3 (1936), §3; 4 (1939)], and later for the corresponding semantical concept. It seems clear from the informal explanations that, even at that time, the concept was intended as a measure of certainty, not of the increase in certainty; thus I said [4, p. 222]: "The outcome of such a procedure of testing an hypothesis is either a confirmation or an infirmation of that hypothesis, or, rather, either an increase or a decrease of its degree of confirmation." The term "degree of confirmation" was perhaps first suggested to me by Karl Popper's term "Bewährungsgrad" [18 (1935), §§81f]. But it seems that at that time I was not quite clear about the sense of Popper's concept, or about that of my own. I gave the first clear exposition of my concept in 1945 [5, 6]. I explained the distinction between probability and probability$_2$—as in the book—and I said that I mean by the term "degree of confirmation" the logical concept of probability$_1$, thus the concept I 3 in the above schema. Popper's later publications showed that what he had in mind was not I 3, or II 3 either, but still another concept. Nowadays Popper uses for this concept the term "degree of corroboration" instead of "degree of confirmation." Thus there is no longer a collision between our terms.

[3] Popper was the first to criticize the two points mentioned. However, he combined these correct observations with a number of other comments based upon misunderstandings and mistakes; he even asserted that my system itself contained a contradiction. Bar-Hillel, Kemeny, and Jeffrey stated their agreement with Popper's criticism in the two points, but rejected his assertion of a contradiction. Bar-Hillel pointed out Popper's mistakes clearly and in detail. (See a series of discussion notes by Popper (partly reprinted in his [19]) and Bar-Hillel in the *British Journal for the Philosophy of Science*, 5:143-49 (1954) 6:155-63 (1955), and 7:244-56 (1956), and a brief note of mine, same journal, 7:243f (1956); furthermore, see reviews by Kemeny in *Journal of Symbolic Logic*, 20:304 (1955), and Jeffrey in *Econometrica*, 28:925 (1960). Compare also my [12, §31].) My agreement with Popper's criticism in the two points above does, of course, in no way affect my views on the nature and function of inductive logic.

BIBLIOGRAPHY

1. Bar-Hillel, Y. "A Note on State Descriptions," *Philosophical Studies*, 2:72-75 (1951) .
2._____. "A Note on Comparative Inductive Logic," *British Journal for the Philosophy of Science*, 3:308-10 (1953).
3. Carnap, R. "Testability and Meaning," *Philosophy of Science*, 3:419-71 (1936), 4:1-38 (1937). Separately printed, New Haven, Conn.: Whitlock's, 1954.
4._____. "Science and Analysis of Language," *Journal of Unified Science*, 9:221-26

(1940). (Fifth International Congress for the Unity of Science, Cambridge, Mass., 1939.)
5._____. "On Inductive Logic," *Philosophy of Science*, 12:72-97 (1945).
6._____. "The Two Concepts of Probability," *Philosophy and Phenomenological Research*, 5:513-32 (1945). Reprinted in H. Feigl and W. Sellars, eds., *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts, 1949.
7._____.*The Logical Foundations of Probability,* Chicago: University of Chicago Press, 1950.
8._____. *The Continuum of Inductive Methods*. Chicago: University of Chicago Press, 1952.
9._____. "Meaning Postulates," *Philosophical Studies*, 3:65-73 (1952). Reprinted in *Meaning and Necessity*, 2nd edition. Chicago: University of Chicago Press, 1956.
10.____. "On the Comparative Concept of Confirmation," *British Journal for the Philosophy of Science*, 3:311-18 (19 5 3).
11.____. "The Aim of Inductive Logic," in *Proceedings of the International Congress for Logic and Methodology of Science* (Stanford, 1960). Stanford, Calif.: Stanford University Press, 1962. Pp. 113-28.
12.____. "Replies and Systematic Expositions," to appear in Paul A. Schilpp, ed., *The Philosophy of Rudolf Carnap*. Forthcoming.
13. Carnap, R., and W. Stegmüller. *Inductive Logik and Wahrscheinlichkeit*. Wien, 1959.
14. Good, I. J. *Probability and the Weighing of Evidence*. London: C. Griffin; New York: Hafner, 1950.
15. Kemeny, John G. "Carnap on Probability" (review of [7]), *Review of Metaphysics*, 5:145-56 (1951).
16.____. "Extensions of the Methods of Inductive Logic," *Philosophical Studies*, 3:38-42 (1952).
17.____. "A Logical Measure Function," *Journal of Symbolic Logic*, 18:289-308 (1953).
18. Popper, K. *Logik der Forschung*. Wien: J. Springer,1935.
19.____. *The Logic of Scientific Discovery* (translation of [18]). London: Hutchinson,1959.
20. Savage, L. J. *The Foundations of Statistics*. New York: Wiley,1954.