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PREFACE TO THE SECOND EDITION

The first edition of this book has been out of print for some time. In this second edition the original text is reprinted without changes, except for a number of small corrections.[1] I have added a Supplementary Bibliography, listing the more important publications in the field which have appeared since 1950.

The purpose of this new preface 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. Second, I shall specify some particular points in which my views have changed since I wrote this book. But the major features of my theory, as explained in this 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.

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 this 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and change in this field that a comprehensive book, trying to describe the present situation, would probably be outdated before its appearance.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 [Continuum] (see the Supplementary Bibliography). This monograph contains also a discussion of estimate functions for relative frequency, points out some serious disadvantages of certain

[1] I give here a list of those places where actual errors or serious misprints have been corrected. References in parentheses refer to corrections which were already made in the second impression (1951) and in the British edition: 12/$\bar{5}$ (i.e., page 12, line 5 from bottom); (66/6 f.); (77/20); (81, T6/1) (i.e., page 81, Theorem T19-6, line 1); 99, T90/1; (99, T11g); (101, T2a); (124/10); 158/6–8 and 15–18; 166/1f.; 229/$\overline{11}$; 312/15; (318/10); 321, T5a/2; 325, T6j/1; (334, T1d/7); 361/$\overline{13}$; 362, T1/1; 409/5; (441/11); 507/$\bar{4}$; 533/3f.; 542/11; 543/$\bar{2}$; 544/24.

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 was published in [Replies] § 26 and in Carnap-Stegmüller [Wahrsch.], Anhang B. The system is still in the process of change and growth.

My conception of logical probability (called 'probability$_1$' in this 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 this 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. 43–44, 239–40].) 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 proved 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.[2]

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

A. The meaning of logical probability (probability$_1$) 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 $2/3$' can best be characterized by explaining their use, in combination with the concept of utility, in the rule for the determination of rational decisions (§ 51A, 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 this 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$_1$ (§ 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

[2] I have explained this view in the last paragraphs of [Aim].

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 comparison of two hypotheses h and h' with respect to the same evidence i. For each of these concepts a formulation in terms of $\mathfrak{c}$ is given in the last column; $\mathfrak{c}$ is to be understood as probability$_1$ 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.	$\mathfrak{c}(h, e) > b$, where b is a fixed number.
I 2. (a) h on e is *firmer* than h' on e'.	$\mathfrak{c}(h, e) > \mathfrak{c}(h', e')$.
(b) h is firmer on e than on e'.	$\mathfrak{c}(h, e) > \mathfrak{c}(h, e')$.
(c) h is firmer than h', on e.	$\mathfrak{c}(h, e) > \mathfrak{c}(h', e)$.
I 3. The (degree of) *firmness* of h on e is u.	$\mathfrak{c}(h, e) = u$.

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 $\mathfrak{c}$. This can be done by different functions (compare the different relevance functions discussed in chap. vii). For the present survey let us take the simplest function of this kind, the difference; we define: $D(h, i) =_{Df} \mathfrak{c}(h, i) - \mathfrak{c}(h, t)$.

II 1. h is *made firmer* by i.	$D(h, e) > 0$; hence: $\mathfrak{c}(h, i) > \mathfrak{c}(h, t)$.
II 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: $\mathfrak{c}(h, i) > \mathfrak{c}(h, i')$.
(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 '$\mathfrak{c}(h, e \,.\, i) > \mathfrak{c}(h, e)$' and thus be the same as the general concept of positive relevance, D65-1a.)

[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 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 '$\mathfrak{c}$' (which was always meant in the sense of probability$_1$). Thus we find (by formula (4), p. 464) that the classificatory concept was meant in the sense of II 1; and similarly (by formula (2), p. 431) that the comparative concept was meant in the sense of I 2a. (In formula (2) I took '$\geqq$' rather than '$>$' for reasons of technical convenience; this difference is irrelevant for our present discussion.) Since the quantitative concept was always meant as I 3, my triple of concepts consisted of II 1, I 2, and I 3. Thus 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, Warmer, 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