HAROLD JEFFREYS

THEORY OF PROBABILITY

THIRD EDITION

THEORY OF PROBABILITY

BY

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PREFACE

In the present edition I have developed more fully the proof of the consistency of the postulates of the theory, including the product rule and therefore the principle of inverse probability. The convergence principle, otherwise the simplicity postulate for initial probabilities, is shown to satisfy the conditions. The argument amounts to a proof that axioms can be stated that will permit the attachment of a high probability to any precisely stated law given suitable observational data. There is still room for choice in the precise formulation of the convergence principle, but I regard that as an invitation to others to try. I do not claim to have done all that is needed, but I do claim to have done a great deal more than any of my critics have noticed. Where the theory is incomplete the outstanding questions are mostly either of a sort that can make little difference to practical applications, or arise from difficulties in stating the likelihood that would affect any theory.

Some mathematical proofs are given more fully than in previous editions. A proof of the Pitman–Koopman theorem concerning the existence of sufficient statistics is given in an extended form. The related invariance theory of Huzurbazar for initial probabilities is described. The revision of prior probabilities is brought into relation with the theory of types.

Some points in later chapters have been transferred to the first, in the hope that fewer critics will be misled into inferring what is not in the book from not finding it in the first chapter. For instance, the difficulty mentioned on p. 3 has been repeated as inescapable, whereas the greater part of the book is devoted to showing how it can be met in a constructive way; that on p. 119 continues to be stated though it was answered thirty years ago; and arguments based on the assumption of equal probabilities over an infinite class of laws are still given without mention of the convergence principle.

Several writers, even recent ones, have described me as a follower of the late Lord Keynes. Without wishing to disparage Keynes, I must point out that the first two papers by Wrinch and me in the *Philosophical Magazine* of 1919 and 1921 preceded the publication of Keynes's book. What resemblance there is between the present theory and that of Keynes is due to the fact that Broad, Keynes, and my collaborator had all attended the lectures of W. E. Johnson. Keynes's distinctive contribution was the assumption that probabilities are only partially ordered; this contradicts my Axiom 1. I gave reasons for not accepting it in a review of Keynes's book and in the first edition of *Scientific Inference*. Mistakenly thinking that this was no longer necessary I omitted them from the second. Keynes himself withdrew his assumption in his biographical essay on F. P. Ramsey. My own primary inspiration came from Pearson's *Grammar of Science*, a work that is apparently unknown to many present philosophers of science.

On the other hand, the main conclusion, that scientific method depends on considering at the outset the hypothesis that variation of the data is completely random, and modifying it step by step as the data are found to support alternatives, is a complete reversal of the nature of induction as understood by philosophers. Yet so far as I know no philosopher has noticed it.

Adherents of frequency definitions of probability have naturally objected to the whole system. But they carefully avoid mentioning my criticisms of frequency definitions, which any competent mathematician can see to be unanswerable. In this way they contrive to present me as an intruder into a field where everything was already satisfactory. I speak from experience in saying that students have no difficulty in following my system if they have not already spent several years in trying to convince themselves that they understand frequency theories.

Several authors have recently tried to construct theories that can be regarded as compromises between the epistemological one and one that admits intrinsic probabilities only. It seems to me that these are only elaborate ways of shirking the problems. The present formulation is the easiest that can be constructed.

However, there is a decided improvement in the willingness of physicists to estimate the uncertainties of their results properly, and I suppose that I can claim some of the credit for this. There is, however, room for further improvement.

H. J.

Cambridge, 1960

PREFACE TO THE FIRST EDITION

THE chief object of this work is to provide a method of drawing inferences from observational data that will be self-consistent and can also be used in practice. Scientific method has grown up without much attention to logical foundations, and at present there is little relation between three main groups of workers. Philosophers, mainly interested in logical principles but not much concerned with specific applications, have mostly followed in the tradition of Bayes and Laplace; but with the brilliant exception of Professor C. D. Broad have not paid much attention to the consequences of adhering to the tradition in detail. Modern statisticians have developed extensive mathematical techniques, but for the most part have rejected the notion of the probability of a hypothesis, and thereby deprived themselves of any way of saying precisely what they mean when they decide between hypotheses. Physicists have been described, by an experimental physicist who has devoted much attention to the matter, as not only indifferent to fundamental analysis but actively hostile to it; and with few exceptions their statistical technique has hardly advanced beyond that of Laplace. In opposition to the statistical school, they and some other scientists are liable to say that a hypothesis is definitely proved by observation, which is certainly a logical fallacy; most statisticians appear to regard observations as a basis for possibly rejecting hypotheses, but in no case for supporting them. The latter attitude, if adopted consistently, would reduce all inductive inference to guesswork; the former, if adopted consistently, would make it impossible ever to alter the hypotheses, however badly they agreed with new evidence. The present attitudes of most physicists and statisticians are diametrically opposed, but lack of a common meeting-ground has, to a very large extent, prevented the opposition from being noticed. Nevertheless, both schools have made great scientific advances, in spite of the fact that their fundamental notions, for one reason or the other, would make such advances impossible if they were consistently maintained.

In the present book I reject the attempt to reduce induction to deduction, which is characteristic of both schools, and maintain that the ordinary common-sense notion of probability is capable of precise and consistent treatment when once an adequate language is provided for it. It leads to the result that a precisely stated hypothesis may attain either a high or a negligible probability as a result of observational data, and therefore to an attitude intermediate between those current in physics and statistics, but in accordance with ordinary thought. Fundamentally the attitude is that of Bayes and Laplace, though it is found necessary to modify their hypotheses before some types of cases not considered by them can be treated, and some steps in the argument have been filled in. For instance, the rule for assessing probabilities given in the first few lines of Laplace's book is Theorem 7, and the principle of inverse probability is Theorem 10. There is, on the whole, a very good agreement with the recommendations made in statistical practice; my objection to current statistical theory is not so much to the way it is used as to the fact that it limits its scope at the outset in such a way that it cannot state the questions asked, or the answers to them, within the language that it provides for itself, and must either appeal to a feature of ordinary language that it has declared to be meaningless, or else produce arguments within its own language that will not bear inspection.

The most beneficial result that I can hope for as a consequence of this work is that more attention will be paid to the precise statement of the alternatives involved in the questions asked. It is sometimes considered a paradox that the answer depends not only on the observations but on the question; it should be a platitude.

The theory is applied to most of the main problems of statistics, and a number of specific applications are given. It is a necessary condition for their inclusion that they shall have interested me. As my object is to produce a general method I have taken examples from a number of subjects, though naturally there are more from physics than from biology and more from geophysics than from atomic physics. It was, as a matter of fact, mostly with a view to geophysical applications that the theory was developed. It is not easy, however, to produce a statistical method that has application to only one subject; though intraclass correlation, for instance, which is a matter of valuable positive discovery in biology, is usually an unmitigated nuisance in physics. It may be felt that many of the applications suggest further questions. That is inevitable. It is usually only when one group of questions has been answered that a further group can be stated in an answerable form at all.

I must offer my warmest thanks to Professor R. A. Fisher and Dr. J. Wishart for their kindness in answering numerous questions from a not very docile pupil, and to Mr. R. B. Braithwaite, who looked over the manuscript and suggested a number of improvements; also to the Clarendon Press for their extreme courtesy at all stages.

H. J.

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FUNDAMENTAL NOTIONS

> They say that Understanding ought to work by the rules of right reason. These rules are, or ought to be, contained in Logic; but the actual science of logic is conversant at present only with things either certain, impossible, or entirely doubtful, none of which (fortunately) we have to reason on. Therefore the true logic for this world is the calculus of Probabilities, which takes account of the magnitude of the probability which is, or ought to be, in a reasonable man's mind.
>
> J. CLERK MAXWELL

1.0. THE fundamental problem of scientific progress, and a fundamental one of everyday life, is that of learning from experience. Knowledge obtained in this way is partly merely description of what we have already observed, but part consists of making inferences from past experience to predict future experience. This part may be called generalization or induction. It is the most important part; events that are merely described and have no apparent relation to others may as well be forgotten, and in fact usually are. The theory of learning in general is the branch of logic known as epistemology. A few illustrations will indicate the scope of induction. A botanist is confident that the plant that grows from a mustard seed will have yellow flowers with four long and two short stamens, and four petals and sepals, and this is inferred from previous instances. The *Nautical Almanac*'s predictions of the positions of the planets, an engineer's estimate of the output of a new dynamo, and an agricultural statistician's advice to a farmer about the utility of a fertilizer are all inferences from past experience. When a musical composer scores a bar he is expecting a definite series of sounds when an orchestra carries out his instructions. In every case the inference rests on past experience that certain relations have been found to hold; and those relations are then applied to new cases that were not part of the original data. The same applies to my expectations about the flavour of my next meal. The process is so habitual that we hardly notice it, and we can hardly exist for a minute without carrying it out. On the rare occasions when anybody mentions it, it is called common sense and left at that.

Now such inference is not covered by logic, as the word is ordinarily understood. Traditional or deductive logic admits only three attitudes to any proposition: definite proof, disproof, or blank ignorance. But no number of previous instances of a rule will provide a deductive proof that the rule will hold in a new instance. There is always the formal possibility of an exception.

Deductive logic and its close associate, pure mathematics, have been developed to an enormous extent, and in a thoroughly systematic way —indeed several ways. Scientific method, on the other hand, has grown up more or less haphazard, techniques being developed to deal with problems as they arose, without much attempt to unify them, except so far as most of the theoretical side involved the use of pure mathematics, the teaching of which required attention to the nature of some sort of proof. Unfortunately the mathematical proof is deductive, and induction in the scientific sense is simply unintelligible to the pure mathematician—as such; in his unofficial capacity he may be able to do it very well. Consequently little attention has been paid to the nature of induction, and apart from actual mathematical technique the relation between science and mathematics has done little to develop a connected account of the characteristic scientific mode of reasoning. Many works exist claiming to give such an account, and there are some highly useful ones dealing with methods of treating observations that have been found useful in the past and may be found useful again. But when they try to deal with the underlying general theory they suffer from all the faults that modern pure mathematics has been trying to get rid of: self-contradictions, circular arguments, postulates used without being stated, and postulates stated without being used. Running through the whole is the tendency to claim that scientific method can be reduced in some way to deductive logic, which is the most fundamental fallacy of all: it can be done only by rejecting its chief feature, induction.

The principal field of application of deductive logic is pure mathematics, which pure mathematicians recognize quite frankly as dealing with the working out of the consequences of stated rules with no reference to whether there is anything in the world that satisfies those rules. Its propositions are of the form 'If p is true, then q is true', irrespective of whether we can find any actual instance where p is true. The mathematical proposition is the *whole* proposition, 'If p is true, then q is true', which may be true even if p is in fact always false. In applied mathematics, as usually taught, general rules are asserted as applicable to the external world, and the consequences are developed logically by the technique of pure mathematics. If we inquire what reason there is to suppose the general rules true, the usual answer is simply that they are known from experience. However, this use of the word 'experience' covers a confusion. The rules are inferred from past experience, and then applied to future experience, which is not the same thing. There is no guarantee whatever in deductive logic that a rule that has held in all previous instances will not break down in the next instance or in all future instances. Indeed there are an infinite number of rules that have held in all previous cases and cannot possibly all hold in future ones. For instance, consider a body falling freely under gravity. It would be asserted that the distance at time t below a fixed level is given by a formula of the type

$$s = a+ut+\tfrac{1}{2}gt^2. \tag{1}$$

This might be asserted from observations of s at a series of instants $t_1, t_2,..., t_n$. That is, our previous experience asserts the proposition that a, u, and g exist such that

$$s_r = a+ut_r+\tfrac{1}{2}gt_r^2 \tag{2}$$

for all values of r from 1 to n. But the law (1) is asserted for *all* values of t. But consider the law

$$s = a+ut+\tfrac{1}{2}gt^2+f(t)(t-t_1)(t-t_2)...(t-t_n), \tag{3}$$

where $f(t)$ may be any function whatever that is not infinite at any of $t_1, t_2,..., t_n$, and a, u, and g have the same values as in (1). There are an infinite number of such functions. Every form of (3) will satisfy the set of relations (2), and therefore every one has held in all previous cases. But if we consider any other instant t_{n+1} (which might be either within or outside the range of time between the first and last of the original observations) it will be possible to choose $f(t_{n+1})$ in such a way as to give s as found from (3) any value whatever at time t_{n+1}. Further, there will be an infinite number of forms of $f(t)$ that would give the same value of $f(t_{n+1})$, and there are an infinite number that would give different values. If we observe s at time t_{n+1}, we can choose $f(t_{n+1})$ to give agreement with it, but an infinite number of forms of $f(t)$ consistent with this value would be consistent with any arbitrary value of s at a further moment t_{n+2}. That is, even if all the observed values agree with (1) exactly, deductive logic can say nothing whatever about the value of s at any other time. An infinite number of laws agree with previous experience, and an infinite number that have agreed with previous experience will inevitably be wrong in the next instance. What the applied mathematician does, in fact, is to select one form out of this infinity; and his reason for doing so has nothing whatever to do with traditional logic. He chooses the simplest. This is actually an understatement of the case; because in general the observations will not agree with (1) exactly, a polynomial of n terms can still be found that will agree exactly with the observed values at times $t_1,\ldots, t_n$, and yet the form (1) may still be asserted. Similar considerations apply to any quantitative law. The further significance of this matter must be reserved till we come to significance tests. We need notice at the moment only that the choice of the simplest law that fits the facts is an essential part of procedure in applied mathematics, and cannot be justified by the methods of deductive logic. It is, however, rarely stated, and when it is stated it is usually in a manner suggesting that it is something to be ashamed of. We may recall the words of Brutus.

> But 'tis a common proof
> That lowliness is young ambition's ladder,
> Whereto the climber upwards turns his face;
> But when he once attains the upmost round,
> He then unto the ladder turns his back,
> Looks in the clouds, scorning the base degrees
> By which he did ascend.

It is asserted, for instance, that the choice of the simplest law is purely a matter of economy of description or thought, and has nothing to do with any reason for believing the law. No reason in deductive logic, certainly; but the question is, Does deductive logic contain the whole of reason? It does give economy of description of past experience, but is it unreasonable to be interested in future experience? Do we make predictions merely because those predictions are the easiest to make? Does the Nautical Almanac Office laboriously work out the positions of the planets by means of a complicated set of tables based on the law of gravitation and previous observations, merely for convenience, when it might much more easily guess them? Do sailors trust the safety of their ships to the accuracy of these predictions for the same reason? Does a town install a new tramway system, with expensive plant and much preliminary consultation with engineers, with no more reason to suppose that the trams will move than that the laws of electromagnetic induction are a saving of trouble? I do not believe for a moment that anybody will answer any of these questions in the affirmative; but an affirmative answer is implied by the assertion that is still frequently made, that the choice of the simplest law is merely a matter of convention. I say, on the contrary, that the simplest law is chosen because it is the most likely to give correct predictions; that the choice is based on a reasonable degree of belief; and that the fact that deductive logic provides no explanation of the choice of the simplest law is an absolute proof that deductive logic is grossly inadequate to cover scientific and practical requirements. It is sometimes said, again, that the trust in the simple law is a peculiarity of human psychology; a different type of being might behave differently. Well, I see no point whatever in discussing at length whether the human mind is any use; it is not a perfect reasoning instrument, but it is the only one we have. Deductive logic itself could never be known without the human mind. If anybody rejects the human mind and then holds that he is constructing valid arguments, he is *contradicting himself*; *if he holds that human* minds other than his own are useless, and then hopes to convince them by argument, he is again contradicting himself. A critic is himself using inductive inference when he expects his words to convey the same meaning to his audience as they do to himself, since the meanings of words are learned first by noting the correspondence between things and the sounds uttered by other people, and then applied in new instances. On the face of it, it would appear that a general statement that something accepted by the bulk of mankind is intrinsically nonsense requires much more to support it than a mere declaration.

Many attempts have been made, while accepting induction, to claim that it can be reduced in some way to deduction. Bertrand Russell has remarked that induction is either disguised deduction or a mere method of making plausible guesses.† In the former sense we must look for some general principle, which states a set of possible alternatives; then observations are used to show that all but one of these are wrong, and the survivor is held to be deductively demonstrated. Such an attitude has been widely advocated. On it I quote Professor C. D. Broad.‡

The usual view of the logic books seems to be that inductive arguments are really syllogisms with propositions summing up the relevant observations as minors, and a common major consisting of some universal proposition about nature. If this were true it ought to be easy enough to find the missing major, and the singular obscurity in which it is enshrouded would be quite inexplicable. It is reverently referred to by inductive logicians as the *Uniformity of Nature*; but, as it is either never stated at all or stated in such terms that it could not

† *Principles of Mathematics*, p. 360. He said, at the Aristotelian Society summer meeting in 1938, that this remark has been too much quoted. I therefore offer apologies for quoting it again. He has also remarked that the inductive philosophers of central Africa formerly held the view that all men were black. My comment would be that the deductive ones, if there were any, did not hold that there were any men, black, white, or yellow.

‡ *Mind*, **29**, 1920, 11.

possibly do what is required of it, it appears to be the inductive equivalent of Mrs. Gamp's mysterious friend, and might be more appropriately named Major Harris.

It is in fact easy to prove that this whole way of looking at inductive arguments is mistaken. On this view they are all syllogisms with a common major. Now their minors are propositions summing up the relevant observations. If the observations have been carefully made the minors are practically certain. Hence, if this theory were true, the conclusions of all inductive arguments in which the observations were equally carefully made would be equally probable. For what could vary the probabilities? Not the major, which is common to all of them. Not the minors, which by hypothesis are equally certain. Not the mode of reasoning, which is syllogistic in each case. But the result is preposterous, and is enough to refute the theory which leads to it.

Attempts have been made to supply the missing major by several modern physicists, notably Sir Arthur Eddington and Professor E. A. Milne. But their general principles and their results differ even within the very limited field of knowledge where they have been applied. How is a person with less penetration to know which is right, if any? Only by comparing the results with observation; and then his reason for believing the survivor to be likely to give the right results in future is inductive. I am not denying that one of them may have got the right results. But I reject the statement that any of them can be said to be certainly right as a matter of pure logic, independently of experience; and I gravely doubt whether any of them could have been thought of at all had the authors been unaware of the vast amount of previous work that had led to the establishment by inductive methods of the laws that they set out to explain. These attempts, though they appear to avoid Broad's objection, do so only within a limited range, and it is doubtful whether such an attempt is worth making if it can at best achieve a partial success, when induction can cover the whole field without supposing that special rules hold in certain subjects.

I should maintain (with N. R. Campbell, who says† that a physicist would be more likely to interchange the two terms in Russell's statement) that a great deal of what passes for deduction is really disguised induction, and that even some of the postulates of *Principia Mathematica* are adopted on inductive grounds (which, incidentally, are false).

Karl Pearson‡ writes as follows:

Now this is the peculiarity of scientific method, that when once it has become a habit of mind, that mind converts all facts whatsoever into science. The field of science is unlimited; its material is endless, every group of natural phenomena,

† *Physics, The Elements*, 1920, p. 9.

‡ *The Grammar of Science*, 1892. Page 16 of Everyman edition, 1938.

every phase of social life, every stage of past or present development is material for science. *The unity of all science consists alone in its method, not in its material.* The man who classifies facts of any kind whatever, who sees their mutual relation and describes their sequences, is applying the scientific method and is a man of science. The facts may belong to the past history of mankind, to the social statistics of our great cities, to the atmosphere of the most distant stars, to the digestive organs of a worm, or to the life of a scarcely visible bacillus. It is not the facts themselves which form science, but the methods by which they are dealt with.

Here, in a few sentences, Pearson sets our problem. The italics are his. He makes a clear distinction between method and material. No matter what the subject-matter, the fundamental principles of the method must be the same. There must be a uniform standard of validity for all hypotheses, irrespective of the subject. Different laws may hold in different subjects, but they must be tested by the same criteria; otherwise we have no guarantee that our decisions will be those warranted by the data and not merely the result of inadequate analysis or of believing what we want to believe. An adequate theory of induction must satisfy two conditions. First, it must provide a general method; secondly, the principles of the method must not of themselves say anything about the world. If the rules are not general, we shall have different standards of validity in different subjects, or different standards for one's own hypotheses and somebody else's. If the rules of themselves say anything about the world, they will make empirical statements independently of observational evidence, and thereby limit the scope of what we can find out by observation. If there are such limits, they must be inferred from observation; we must not assert them in advance.

We must notice at the outset that induction is more general than deduction. The answers given by the latter are limited to a simple 'yes', 'no', or 'it doesn't follow'. Inductive logic must split up the last alternative, which is of no interest to deductive logic, into a number of others, and say which of them it is most reasonable to believe on the evidence available. Complete proof and disproof are merely the extreme cases. Any inductive inference involves in its very nature the possibility that the alternative chosen as the most likely may in fact be wrong Exceptions are always possible, and if a theory does not provide for them it will be claiming to be deductive when it cannot be. On account of this extra generality, induction must involve postulates not included in deduction. Our problem is to state these postulates. It is important to notice that they cannot be proved by deductive logic. If they could, induction would be reduced to deduction, which is impossible. Equally they are not empirical generalizations; for induction would be needed to make them and the argument would be circular. We must in fact distinguish the general rules of the theory from the empirical content. The general rules are *a priori* propositions, accepted independently of experience, and making by themselves no statement about experience. Induction is the application of the rules to observational data.

Our object, in short, is not to prove induction; it is to tidy it up. Even among professional statisticians there are considerable differences about the best way of treating the same problem, and, I think, all statisticians would reject some methods habitual in some branches of physics. The question is whether we can construct a general method, the acceptance of which would avoid these differences or at least reduce them.

1.1. The test of the general rules, then, is not any sort of proof. This is no objection because the primitive propositions of deductive logic cannot be proved either. All that can be done is to state a set of hypotheses, as plausible as possible, and see where they lead us. The fullest development of deductive logic and of the foundations of mathematics is that of *Principia Mathematica*, which starts with a number of primitive propositions taken as axioms; if the conclusions are accepted, that is because we are willing to accept the axioms, not because the latter are proved. The same applies, or used to apply, to Euclid. We must not hope to prove our primitive propositions when this is the position in pure mathematics itself. But we have rules to guide us in stating them, largely suggested by the procedure of logicians and pure mathematicians.

1. All hypotheses used must be explicitly stated, and the conclusions must follow from the hypotheses.

2. The theory must be self-consistent; that is, it must not be possible to derive contradictory conclusions from the postulates and any given set of observational data.

3. Any rule given must be applicable in practice. A definition is useless unless the thing defined can be recognized in terms of the definition when it occurs. The existence of a thing or the estimate of a quantity must not involve an impossible experiment.

4. The theory must provide explicitly for the possibility that inferences made by it may turn out to be wrong. A law may contain

adjustable parameters, which may be wrongly estimated, or the law itself may be afterwards found to need modification. It is a fact that revision of scientific laws has often been found necessary in order to take account of new information—the relativity and quantum theories providing conspicuous instances—and there is no conclusive reason to suppose that any of our present laws are final. But we do accept inductive inference in some sense; we have a certain amount of confidence that it will be right in any particular case, though this confidence does not amount to logical certainty.

5. The theory must not deny any empirical proposition *a priori*; any precisely stated empirical proposition must be formally capable of being accepted, in the sense of the last rule, given a moderate amount of relevant evidence.

These five rules are essential. The first two impose on inductive logic criteria already required in pure mathematics. The third and fifth enforce the distinction between *a priori* and empirical propositions; if an existence depends on an inapplicable definition we must either find an applicable one, treat the existence as an empirical proposition requiring test, or abandon it. The fourth states the distinction between induction and deduction. The fifth makes Pearson's distinction between material and method explicit, and involves the definite rejection of attempts to derive empirically verifiable propositions from general principles adopted independently of experience.

The following rules also serve as useful guides.

6. The number of postulates should be reduced to a minimum. This is done for deductive logic in *Principia*, though many theorems proved there appear to be as obvious intuitively as the postulates. The motive for not accepting other obvious propositions as postulates is partly aesthetic. But in the theory of scientific method it is still more important, because if we choose the postulates so as to cover the subject with the minimum number of postulates we thereby minimize the number of acts of apparently arbitrary choice. Most works on the subject state more principles than I do, use far more than they state, and fail to touch many important problems. So far as their assumptions are valid they are consequences of mine; the present theory aims at removing irrelevancies.

7. While we do not regard the human mind as a perfect reasoner, we must accept it as a useful one and the only one available. The theory need not represent actual thought-processes in detail, but should agree with them in outline. We are not limited to considering only the