Until very recently, the foregoing criticisms would have related to the whole art of crustal interpretation, and it is clear that the authors appreciate many of the limitations of their approach. However, the "plus-minus" method of Hagedoorn and the "time-term" method of Willmore, Scheidegger and Bancroft have now eliminated the necessity of forcing refraction data to fit plane-stratified structures, and the introduction of tape-recording has permitted the techniques of filtering and cross-correlation to be applied to the recognition of arrivals. It is a pity that such a substantial effort should have been carried to completion at the end of the long plateau in the development of crustal studies, but at least the work will serve to illustrate the extreme complexity of the data, and the importance of pressing for further refinements of interpretation.

P. L. WILLMORE

Theory of Probability

Harold Jeffreys

(Third edition, 447 + ix pp., Oxford Univ. Press, 84*s*.)

In order to appreciate the historical significance of Sir Harold Jeffreys's work on probability it is necessary to understand that the word "probability" has been used in various senses by various writers. There is not space here to discuss these senses in detail, but a few distinctions can be briefly indicated by merely naming them. (A fuller, but still brief, account is presented in *Science*, **129**, 1959, p. 443.) The most important distinction is that between *physical*, material, or intrinsic probabilities, chances, or propensities on the one hand, and non-physical or *intuitive* probabilities on the other. This distinction was clearly made by Poisson in 1837. Intuitive probabilities can be subdivided into *credibilities* = logical probabilities = unique rational degrees of belief or intensities of conviction on the one hand, and, on the other hand subjective or personal degrees of belief to which some canons of consistency, honesty and maturity have been applied, in which case they are called *subjective* or personal probabilities.

From about 1925 to 1950, nearly all professional statisticians made official use only of physical probabilities. But during much the same period, there were others, such as Keynes, Ramsey, de Finetti and Jeffreys, who argued that no description of the use of physical probability could be given, except in terms of intuitive probability, and that it is impossible to make direct use of a definition in terms of limiting frequencies of "successes". For, as Keynes, said "In the long run we shall all be dead". Ramsey and de Finetti were subjectivists, whereas Keynes and Jeffreys argued in favour of credibilities. The basic distinction between the philosophies of Keynes and Jeffreys was that Keynes regarded probabilities as "partially ordered", whereas Jeffreys assumes unique numerical credibilities to exit, with the aim of avoiding vagueness. He has by no means succeeded in attaining this aim, the reason being, in the reviewer's opinion, that some degree of vagueness is inevitable in the foundations of reasoning, of which the theory of probability is the main part. For an example of the troubles see the top of page 188, dealing with the initial distribution of a correlation coefficient.

In the Preface of the second and third editions of his book, Jeffreys states that Keynes withdrew his assumption of partial ordering in his biographical essay on F. P. Ramsey. This is a mistake. What Keynes withdrew was his belief in credibilities. (See J. M. Keynes, *Essays in Biography*, 1933, 1951, "F. P. Ramsey, 1903–1930", pp. 239–252, esp. p. 243.) In the reviewer's opinion, each person must indeed make implicit or explicit use of partially ordered subjective probabilities, as the basis of his reasoning if any.

In order to apply a calculus of *credibilities* to statistics, it is necessary in each application to make an assumption concerning initial probabilities, the classical form of which is a Bayes–Laplace postulate of equidistribution, and which Jeffreys modifies in various ways. It is most instructive to see how, when selecting a rule for initial probabilities, Jeffreys arrives at his estimates by means of subjective judgments. That a man who makes a judgment is a famous personality does not change the fact that the judgment is personal.

A very familar objection to Bayes postulates, and to their modifications, is that a transformation of the parameter space can lead to a different postulate, and hence to an inconsistency. Jeffreys made a valiant attempt to overcome this difficulty by means of an invariance theory (1946), in which initial probability density is taken as proportional to the square root of the determinant of the information matrix. But he is not himself entirely satisfied with the theory, and is forced to use some *ad hoc* arguments when deciding whether or not to use the theory.

In a thoroughgoing subjective theory there is no obligation to make use of Bayes postulates, so that the above difficulty does not arise. It might indeed be a psychological law that we often behave as if we divide up the parameter space into a minimal number of non-overlapping regions, in each of which the points are indiscernible by means of the apparatus at our disposal, and then attach equal probabilities to each region. (The number of regions is always finite in practice.) Such behaviour would be consistent in the sense that it would not be affected by a transformation of the parameter space.

In my opinion Jeffreys is one of the best living unorthodox philosophers of science. His arguments are often not as rigorous nor as lucid as they ought to be; but he has the great merit of having a practical and mathematical background, without which it is almost impossible to write usefully about the philosophy of probability, and he is witty and stimulating. Consequently his book is of greater importance for the philosophy of science, and obviously of greater immediate practical importance, than nearly all the books on probability written by professional philosophers *lumped together*. His was the first book which attempted to formulate a coherent theory of modern statistics, based on credibilities, and the first edition was thus a landmark in statistics. The new edition is not very different from the second one (1948). It is unfortunate that Jeffreys makes no attempt to deal with the philosophical developments of the last twelve years, being apparently satisfied to brush aside the theory of partially-ordered subjective probabilities with his incorrect prefatory remark concerning Keynes's recantation. Pioneers often ignore the work of those who have stood on their shoulders.

As an example of a lack of rigour, Jeffreys argues (page 29) that the following assumption is only a convention: "If, given p [a proposition], q and q' are exclusive, then the number [probability] assigned on data q to 'q or q'' is the sum of those attached to q and to q'." An examination of the argument shows that the following postulate still remains: For some function, f, $f(P(q \text{ or } q'|p) = f(P(q|p)) + f(P(q'|p))$.

It has in fact been proved (R. T. Cox, *Amer. J. Phys.*, **14**, 1–13, 1946; I. J. Good, *Probability and the Weighing of Evidence*, London, 1950, pp. 105–106; J. Aczél, *Vorlesungen über Funktionalgleichungen und Ihre Anwendungen*, Basel & Stuttgart, 1961, 219–223) that this postulate follows from much more primitive assumptions. But in any case Koopman showed in 1940 that it is virtually impossible not to accept something like the usual axioms for intuitive probability, on the assumption that intuitive probability has a meaning at all. Even this assumption is not required in the behavioural approach used for example by F. P. Ramsey, L. J. Savage, and C. A. B. Smith.

Again, on page 43, there is a very interesting argument which is intended to show that an inductive inference can approach certainty; formally that $P(p_n|p_1 \dots p_{n-1}) \to 1$, where $p_1, p_2, \dots$ are the results of experiments, all of which were "successful". But it seems somewhat inconsistent to deny the validity of a limiting definition for physical probabilities, and then to use a limiting argument for the justification of induction. The conclusion is that "repeated verifications of the consequences of a hypothesis will make it practically certain that the next consequence of it will be verified." In practice (after a very long but of course finite sequence of successes) this would not be true if the new consequences were known to be of an entirely different character from the previous ones. This important gloss seems to have been overlooked by Jeffreys, and I think most good professional philosophers of science would not have overlooked it. And yet the argument is worth more for the philosophy of science than some whole books on the subject.

On page 128, there is another argument concerning scientific induction. The hypothesis is considered that all animals with feathers have beaks. It is again argued that the probability will approach 1 as the number of successes, without failures, increases. The argument and the conclusion are both undermined by the fact that the distribution of essentially distinct species of feathered animals might be very skew, like a Willis, Pareto, or Zipf distribution, or even worse, so that there would always remain species that had not been sampled until about half of the entire population of feathered animals had been examined. (If the rth commonest species had relative frequency, in the population, of $2^{-(r+1)}$, then the number of species represented in the sample would be only about $\log n$, where n is the size of the sample, and by the same token the number of genera might be only about $\log\log n$, and so not large enough for an application of scientific induction. The geometrical distribution is assumed here merely for the sake of argument.) It seems to me that a general law which does not itself refer to probabilities will not usually tend to become certain, however often it is verified. If the law could be qualified by saying that it is to be interpreted as applying only to experiments and observations of a similar nature to those already made, then it might well tend to certainty, but it is not easy to make this kind of qualification precise.

Although most of this review refers to philosophical matters, it seems worth while to repeat that the book contains a large number of detailed applications to mathematical statistics. But Jeffreys never forgets for long that every technique has both its foundations and its applications. As one would expect, some of the applications are to geophysics, and it is not surprising to find twelve index references to earthquakes, but there is no impression that the Earth has been dragged in.

One can forgive Jeffreys for the lack of reference to recent writers, since the hunting up of references can be very time-consuming, and his time can be put to better uses in advancing science. But it should not be forgotten that he has probably been influenced by these writers. For example, perhaps his interest in the possible application of the theory of types to probability might have stemmed from some comments of the reviewer in the above-mentioned book, and in *J. Roy. Stat. Soc. B.* **14**, 107–114, 1952. We all find it easiest to remember our own work. Moreover, if Jeffreys had tried to bring the book up-to-date, rather than to revise it in part, we might have had to wait much longer for the present edition.

I. J. Good