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Comment

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I was taught by Harold Jeffreys, having attended his postgraduate lectures at Cambridge in the academic year 1946–1947, and also knew him when I joined the Faculty there. I thought I appreciated the *Theory of Probability* rather well, so was astonished to read this splendid paper, which so successfully sheds new light on the book by placing it in the context of recent developments.

Jeffreys's—he preferred that form of the possessive—main aim in writing the Theory, his term for TP, was to provide tools for scientists, like himself, more famous then for his geophysics, to use in the observational data they encountered. In the preface to the second edition, he criticizes reviewers of the first for the fact that "no mention was made of the fact that the book contained useful methods of treatment of several problems of practical importance." It is primarily a text on operational statistics. This is most strikingly seen in his development of significance tests, producing results that are distinct from those of Fisher, who was also at Cambridge, though the distinction was not apparent to either of them then. Cambridge was then, as it still is, a true university in the sense that you would regularly meet people outside your own, often narrow, discipline, in college activities. In this atmosphere, Jeffreys was much influenced by a group of philosophers including W. E. Johnson, C. D. Broad and J. M. Keynes, and, as a result, thought seriously about the scientific method, where he was also influenced by Karl Pearson's *Grammar of Science.* (In my view, the best thing KP ever wrote.) It is this atmosphere of data collection in astronomy, combined with the philosophy of science, that produced the Theory; an atmosphere in which mathematics is an essential tool, but only a tool. His attitude to mathematics is best seen in the magisterial book he wrote with his wife, *Methods of Mathematical Physics* (Jeffreys and Swirles, 1946). In light of these considerations, it is clear that his respect for mathematical rigor, while high, did not occupy a dominant position; it was the application that mattered. Robert and his colleagues are right to criticize Jeffreys's attitude to improper distributions but, if uniform over the whole real line gave a sensible posterior, that was good enough for him. He did notice the difficulties with several variances.

There is one point in the Theory where, in my view, he makes an error that he might have recognized. It occurs in equation (1) in Section 3.10 when, in modern terms, he integrates over the sample space to produce the invariants needed for his objective priors. In retrospect, it is surprising that he did this, especially when, elsewhere in the Theory, he condemns the use of integration over the tails of distributions, so incorporating results that did not occur, in the common, non-Bayesian form of a significance test. As a result of the integration in equation (1) the invariant prior can depend on the experiment to be performed; that is, the sample space to be used. Thus the invariant prior for a chance θ would differ according to whether you were going to use direct, or inverse, binomial sampling. Chance θ was, for Jeffreys, a representation of a real thing and ignorance of it should not depend on how it was to be studied. I did not appreciate this issue until Birnbaum introduced me to the likelihood principle.

This error, in a sense, arises from a disputed philosophical view of the nature of science. Jeffreys, like many scientists, both then and now, regarded the scientific method as objective; indeed objectivity was held to be one of, if not the principal, advantages of science over other ways of understanding the world. It was his search for objectivity, in the form of a definition of ignorance, that led him to violate the likelihood principle, which he had recognized rather informally in the condemnation of tails mentioned above. It is obvious now, and should have been at the time of the first edition in 1946, that there are subjective elements in the scientific method as when, in the early stages of an investigation, scientists disagree because of the limited data available. It is only with the accumulation of more evidence that agreement is reached and apparent objectivity obtained. Statistical methods, as Haldane pointed out, are most valuable with modest amounts of data. Jeffreys's error left the way for de Finetti and Savage to lay the foundations for Bayesian ideas in a coherent way.

Let me turn from errors to his triumphs, and the great concepts that he introduced. One of these is his Chapter 1 in which he states, and produces a "proof" that uncertainties, always present with modest amounts of data, must obey the basic rules of probability. It is not,