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Harold Jeffreys and R. A. Fisher

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Introduction

This note traces the differences between Harold Jeffreys (1891-1989) and Ronald Fisher (1890-1962) over probability and statistical inference. Fisher was once the central figure in statistics but Jeffreys was only ever on the fringe. His statistical work, summarised in *Theory of Probability* (1939, -48, -60), seemed misconceived both to Fisher, who thought he had destroyed its Bayesian basis long before, and to younger statisticians for whom Fisher was himself a dinosaur.

Jeffreys and Fisher had much in common. They were applied mathematicians whose research, in physics and genetics respectively, combined mathematical theory and empirical analysis. Their education was similar. Fisher entered Cambridge University as a mathematics undergraduate in 1909 and Jeffreys followed a year later. As an undergraduate, Fisher read Karl Pearson on biometry and found connections with what he was learning about the combination of observations. Jeffreys also learnt about combining observations but he used the methods for many years before contributing to the theory. Jeffreys also read Pearson and adopted his view that laws are not established with certainty but can have a high degree of probability on the data. He developed and applied this view in philosophy of science first and then in statistics.

Jeffreys wrote about scientific method with Dorothy Wrinch (1894-1976), who had links to the Cambridge logicians, including Bertrand Russell and W. E. Johnson. Their first paper “On Some Aspects of the Theory of Probability” (1919) presented the logicians’ notion of probability as a degree of reasonable belief to a scientific audience. The paper also attacked the limiting frequency notion of probability. Wrinch and Jeffreys investigated the problem of sampling from an urn to determine its composition, but only to get insight into the process of accumulating evidence for scientific laws.

Jeffreys’s career was not built on philosophy but on his work on the solar system and the earth. Empirical seismology gradually became his main field and this stimulated him to do research on combining observations. Fisher, meanwhile, was making a career at Rothamsted agricultural station. He maintained his old interests in biometry/genetics/evolution and in statistical theory while creating the analysis of variance and the design of experiments. His *Statistical Methods for Research Workers* (1925) became the statistics book of the era. Fisher’s notion of probability was based on relative frequency and he rejected Bayesian arguments unless there was a prior based on frequency data. He identified the (unsound) Bayesian argument with the use of a uniform prior, noting (1922, p. 325) that the inference depends on the parametrisation chosen.

The exchange 1933-34

In 1933 Jeffreys and Fisher came in sight of one another and both fired. They knew enough of each other's work to know it was futile: Jeffreys had exploded Fisher's notion of probability and Fisher had refuted the Bayesian principle. Given this, there is nothing surprising in Lane's (1980, p. 159) reflection on the exchange, "Nothing was settled Neither scientist seems to have convinced the other of anything."

Jeffreys was looking for better ways of analysing seismological data but he was also revisiting the Wrinch-Jeffreys topics. Among the new material in his *Scientific Inference* (1931) was a treatment of the fundamental problem in the combination of observations: inference to the true value, x , when the errors are normally distributed. The known precision case, which had been treated by Gauss before he went "Gauss-Markov", was in the textbooks but the unknown precision case was not. Jeffreys thought a uniform prior appropriate for x but he saw "no special reason for measuring the precision in terms of h [the established measure in error theory] rather than $\sigma [= \frac{1}{h\sqrt{2}}]$." He recognised the problem of the non-invariance of inference to reparametrisation and disarmed it by making the prior proportional to $1/h$. This met Fisher's objection of 1922, though Jeffreys did not mention this. Nor did Fisher pay much attention to Jeffreys's use of invariant priors, even after Jeffreys developed the idea further in 1946.

In a paper extending the argument to regression, Jeffreys (1932) put the case for $1/h$ in an "alternative form": instead of asking, what distribution describes our prior knowledge, we ask what distribution is consistent with facts otherwise known about the posterior probability on certain types of data. Jeffreys (1932, p. 48) showed that only $1/h$ generates a predictive density embodying the following "fact":

Two measures are made: what is the probability that the third observation will lie between them? The answer is easily seen to be one-third. For the law says nothing about the order of occurrence of errors of different amounts, and therefore the middle one is equally likely to be the first, second, or third made (provided, of course, that we know nothing about the probable range of error already.)

Fisher (1933, p. 344) ridiculed the argument on the (expected) ground, "That there should be a method of evolving such a piece of information by mathematical reasoning only ... would be in all respects remarkable ..." but he also thought there was a mathematical mistake. The discussion is further tangled by Fisher's desire to describe his new (from 1930) fiducial argument and to distinguish it from Jeffreys's argument.

Simultaneously Jeffreys (1933, p. 86) was firing at Fisher: "The whole reason for attaching any importance to Fisher's 'likelihood' is that it is proportional to the posterior probability given by Laplace's theory, and it has no meaning outside the original sample except in terms of that theory." Fisher's criticism of his work on $1/h$ brought an immediate reply. Jeffreys (1933a) explained he was not trying to "evolve a piece of information" in Fisher's sense! Under

fire he became bolder. *Scientific Inference* had not fully endorsed the $1/h$ prior for there was a modification to make the integral unity, to conform to the “convention” that “1 is the constant to be attached to certainty.” Now, however, impropriety became a virtue and Jeffreys (1933a, p. 531) gave new arguments to support “*the fact ... that my distribution is the only distribution of prior probability that is consistent with complete ignorance of the value of h.*”

Jeffreys (1933a, p. 532) also made a general point about the averaging in Fisher’s argument.

This procedure [of integrating with respect to all values of the observed measures] involves a fundamental confusion, which pervades the whole of his statistical work and deprives it of all meaning. The essential distinction in the problem of inference is the distinction between what we know and what we are trying to find out: between the data and the proposition on the data we are trying to find out. ... To integrate with respect to them and average a function of them over the range of integration is an absolutely meaningless process.

This seems to be the first time Jeffreys noted in print the practice of averaging over the sample space. He returned to its meaninglessness when he criticised prob-values and conventional significance testing in the *Theory of Probability*.

The engagement (1934) ended with the protagonists producing a paper summarising his position. The parties separated with their authority intact and convictions reinforced. The only criticism that moved Jeffreys moved him to a more extreme position.

Reconciliation—of a kind

The encounter with Fisher *did* have an effect on Jeffreys. He changed his mind about *garbage in, garbage out* and studied statistical theory, re-doing it so it made sense. The outcome, *Theory of Probability* (1939), could have not been foreseen in 1931 or -33. Added to the philosophy, which went back to the collaboration with Wrinch, and to his work with data was the application of Bayesian methods to a range of problems comparable to that in Fisher’s *Statistical Methods* with a Bayesian take on such Fisherian items as sufficiency, information and the fiducial argument.

This passage (p. 324) captures the book’s tone towards Fisher:

I have in fact been struck repeatedly in my own work, after being led on general principles to a solution of a problem, to find that Fisher has already grasped the essentials by some brilliant piece of common sense, and that his results would be either identical with mine or would differ only in cases where we should both be very doubtful.

The book could have been subtitled *Fisher done properly*.

Fisher did not budge from *garbage in, garbage out* and there is little to suggest that he ever paid detailed attention to what Jeffreys wrote. Yet some of

the points Jeffreys made against him reappeared when he criticised Neyman and his followers. Fisher's caricature of them as mathematicians with no experience of science could not be applied to Jeffreys.

In 1934 the journal brought Fisher and Jeffreys together to arrange the end of hostilities. Thereafter they enjoyed good personal relations. (Some of their letters are in Bennett (1990)). Fisher tended to personalise intellectual differences but Jeffreys was a special case. There was no question of his betraying Fisher (and truth) because he *never* knew any better; nor was his work a threat because in Fisher's life-time very few statisticians took it seriously—it was an intellectual curiosity. When Fisher's fiducial argument came under attack from Bartlett (representing the next generation) and Neyman (a rival leader) Jeffreys's support was welcome. Yet Jeffreys only gave it because he believed that the fiducial argument *properly stated* was equivalent to his own argument. When the missing steps are supplied “the arguments are much longer than those got by introducing the prior probability to express previous ignorance at the start.” (1939, p. 312).

Literature

This account is based on Aldrich (2003). Other works include a book on Jeffreys and Fisher by David Howie, a historian of science, and an article on the controversy by David Lane. For further information see my website *Harold Jeffreys as a Statistician*, which also has the urls of literature available on the web. For Fisher see my website *A Guide to R. A. Fisher*.

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