Comment

Stephen Senn

I have always felt very guilty about Harold Jeffreys's *Theory of Probability* (referred to as *ToP*, hereafter). I take seriously George Barnard's injunction (Barnard, 1996) to have some familiarity with the four great systems of inference. I also consider it a duty and generally find it a pleasure to read the classics, but I find Jeffreys much harder going than Fisher, Neyman and Pearson fils or De Finetti. So I was intrigued to learn that Christian Robert and colleagues had produced an extensive chapter by chapter commentary on Jeffreys, honored to be invited to comment but apprehensive at the task.

Reading Robert et al.'s insightful commentary has sent me back to Jeffreys. Like them, what I am familiar with is the third edition (as corrected in 1966) and I have a rather battered copy with pages heavily annotated in pencil. My habit is to put a marginal vertical line against important passages that merit attention and a question mark where I don't understand. There are lots of both in my copy of Jeffreys.

The commentary by Roberts et al. is a tour de force. Only statisticians with complete familiarity with Bayesian methods and a deep understanding of its many forms could have produced it. It in no way detracts from my admiration for what the authors have achieved to have to admit that my opinion of Jeffreys is unchanged. *ToP* is full of brilliant insights and I return from it convinced that the man was a genius. However, I also think that to any outsider, the theory outlined *as a whole* will appear to be a bit of a mess.

As a small example of one of these insights, consider the discussion of "Artificial Randomization" in Section 4.9, not really covered by Robert et al. Among many interesting points, Jeffreys notes that if a 5×5 Latin Square in agriculture is analyzed using the methods proposed by Fisher, then the row and column totals have eight degrees of freedom assigned to them and hence that the polynomial equivalent is a quartic in the row and the column positions but with no cross-product terms, which would be a very strange function.

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However, perhaps the most important insight in *ToP* concerns the necessity for a prejudice in favour of simpler theories if one wishes to try and rescue the Laplacian proposal of insufficient reason. I was once told by Peter Freeman that when he and Dennis Lindley interviewed Harold Jeffreys and asked him what he considered his greatest scientific achievement, they were stunned when he replied that it was the invention of the significance test. Thus Chapter V of *ToP* (reviewed by Roberts et al. in Section 6) is the one he regarded as being the most important.

There is a very interesting passage in a letter of Jeffreys to Fisher of 1 March 1934. [This correspondence forms pages 149–161 of Henry Bennett's edited correspondence of Fisher (Bennett, 1990) but is also available on the web in facsimile at the very useful site maintained by the University of Adelaide at http://digital.library.adelaide.edu.au/coll/special//fisher/.] The letter is part of a series initiated by the fact that papers of theirs that were due to appear in the *Proceedings of the Royal Society*. (Later in this correspondence, on 10 April, Jeffreys raises Newman's tramcar problem to which Robert et al. refer in Section 5.3.) John Aldrich (Aldrich, 2005), in an article I strongly recommend to any interested in Harold Jeffreys, has identified this period as being crucial to the statistical education of Jeffreys who was, it seems, long unaware that the biologists had something to teach the physicists.

Fisher and Jeffreys had been invited to take account of each other's submissions and were discussing what modification each should make (if any) to accommodate the other's position. The exchange is interesting because Jeffreys proves himself to be a fair match for Fisher and it is a tribute to the respect that Fisher clearly had for him that despite the fact that Jeffreys is occasionally rather cheeky to Fisher (suggesting, e.g., that if Fisher had chosen to justify likelihood in terms of work by Jeffreys and Dorothy Wrinch he would have been on strong ground), Fisher, who was sometimes irascible in correspondence, never loses his temper and even later proposes to moderate in the published commentary the terms in which he describes Jeffreys's theory.

The passage on page 3 of the letter of 1 March reads: