genius of Keynes was applied elsewhere, and the solitary Edgeworth was at the end of his long career.

(4) Textbooks on probability by the mathematicians Whitworth, Burnside and Coolidge gave examples on the probabilities of causes suitable only for the examination room. In view of the title of *SMSI*, there are better reasons for inspecting how inverse probability was treated in textbooks on statistics, or on topics that are statistical in nature. A short list for the period between 1880 and 1930 might include the following books, detailed references for which are scarcely necessary: M. Merriman (1884); A. L. Bowley (1901); G. U. Yule (1911); D. Brunt (1917); E. T. Whittaker and G. Robinson (1924); R. A. Fisher (1925); H. L. Rietz, (1927). The choice of *Statistical Methods for Research Workers* seems appropriate. This book made a fundamental break with tradition (Yates, 1951), and successive editions tolled the death knell of inverse probability for all to hear. The mathematician Neyman seems to have been rather hard of hearing. But Harold Jeffreys firmly rejected the claim and he carried the banner of Bayes and Laplace until the next generation was ready to take over.

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Comment

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In drafting these comments I have had the advantage of seeing Robin Plackett's, with which I broadly agree. Matters are indeed complex.

I beg to differ from Zabell when he writes that in 1930 "Fisher and Neyman *simultaneously* (my stress, G. A. B.) administered a nearly lethal blow to Bayesian statistics, one from which it was not to recover until the publication ... of Savage's *Foundations of Statistics* in 1954." Neyman's continued interest in Bayesian methods in 1929, correctly noted by Zabell, is hardly consistent with his having shared in giving them a near lethal blow the following year. But Fisher's rejection of inverse *probability*, in the sense used here, is already quite clear in the paper of 1912 to which Zabell refers. The most important difference between 'probability' and Fisher's 'likelihood' as a measure of credibility of statistical hypotheses is that 'likelihood' does not obey the addition laws—as Fisher was wont to say, "the likelihood of H or H'" is, like "the height of Peter or Paul," meaningless unless it is specified which of the two is meant. In the final paragraph of his 1912 paper, Fisher specifically says that what he has been calling "probability" is not to be understood as capable of summation over a set of alternative hypotheses. True, he does not put forward the term 'likelihood' until 1921, but the difference of concept is already there in 1912.

Fisher clearly persuaded Egon Pearson, who in turn eventually persuaded Neyman to abandon Bayesian methods, though, unlike Pearson *fils*, Neyman never accepted likelihood as a valid measure of credibility distinct from probability. Neyman's view of the Neyman–Pearson theory had strong "decision" aspects, while Pearson's view was always more flexible.

But "eclipse" does not seem appropriate to describe the state of a theory which, through the 1930's and later continued to have the support, not only of Jeffreys, but also of such other leading users of statistics as Haldane and Gini. In 1940 Deming caused to be published a reprint of Bayes' paper of 1763, and in his introduction E. C. Molina makes it clear that Bayes' ideas continued to demand attention. Frank Yates' contribution to discussion of a paper of mine in 1946 shows Fisher's most distinguished co-worker in statistics agreeing with a Bayesian approach to problems of a certain type. When Maurice Frechet organized a discussion on statistical inference for the 1949 Paris International Congress on the History and Philosophy of Science, it was natural for him to invite

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