Rejoinder

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1. ROBIN L. PLACKETT

Professor Plackett has made a number of distinguished contributions to the history of statistics, and I am grateful for the many interesting questions and issues that he raises in his commentary. Many of these touch on the evolution of Fisher's own thoughts about inference and provide a useful complement to my paper. I was pleased to see that several of the points Professor Plackett makes are consistent with and indeed support the thesis of my paper.

That thesis, in brief, was that: (1) After the criticisms of Boole, Venn and Chrystal, inverse probability and the Bayesian approach, although controversial, remained intellectually respectable until the 1920's. (2) During the 1920's and 1930's these methods fell into disrepute due to the efforts of Fisher and Neyman.

As Professor Plackett notes, Bayesian methods were still part of the curriculum of University College London in 1921; it was instead Rothamsted, Fisher's own institution, that first began to turn out a stream of statisticians opposed to such methods after Fisher's arrival there in 1919; and it was Fisher's textbook *Statistical Methods for Research Workers*, first published in 1925, which played a decisive part in discrediting Bayesian methods during the next quarter century.

Let me turn to a detailed comment on the issues Professor Plackett raises.

1. *The impact of Mendelian genetics on Fisher's view of statistical inference.* I think Professor Plackett makes an important observation when he suggests that genetics may have been of crucial importance in molding Fisher's view of the nature of probability. Fisher was one of those rare individuals who made major contributions to two different fields—statistics and genetics—and it is remarkable how evenly his output was divided between the two areas. Fisher cites genetics in *SMSI* as an area where objective prior probabilities are available, and the last chapter of *SMRW* uses the problem of estimating a linkage parameter to illustrate the key elements in Fisher's theory of statistical inference.

2. *Did Karl Pearson abandon uniform priors?* This is an interesting question, but I do not think it a crucial one. *Laplace* did not advocate the use of uniform priors in all instances (see Stigler, 1986, pages 135–136) and, as noted in the paper, the use of non-uniform priors was discussed by many people during the 19th century. Fisher's attack was not solely on the use of uniform priors, but on the entire Bayesian approach.

I agree with Professor Plackett that the correlation coefficient incident involving Fisher and Pearson is a crucial episode in the history of the subject. It was in large part responsible for the later break between the two, and much of Fisher's subsequent work can be viewed as an attack—implicitly or explicitly—on the Pearsonian edifice: the controversy over the degrees of freedom for chi-squared; the inefficiency of the method of moments vs. maximum likelihood; the subjectivity of Bayesian methods vs. the objectivity of fiducial probability. In his 1917 paper with Soper et al. on the correlation coefficient, a confused Pearson had criticized Fisher for using an inappropriate prior (actually Fisher hadn't used Bayesian methods at all!); it is surely no accident that in his first paper on fiducial inference, Fisher used by way of illustration the bivariate correlation coefficient, rather than the simpler univariate t-statistic.

3. *The role of "Student."* Plackett dismisses Gosset's early excursus into Bayesianism as being at a time when he was under the influence of Pearson, but in many ways that's the point: if you studied statistics at a research level in Britain shortly after the turn of the century, you studied under Pearson and were likely to emerge with a Bayesian perspective. Admittedly Gosset made little actual use of Bayesian statistics in his own work, but then again, on a *practical* level few people after Laplace did. This brings us to another important question discussed by Professor Plackett.

4. *How widely were Bayesian methods employed?* In thinking about this issue, an important distinction needs to be made. During the 19th century, Bayesian methods functioned primarily as a conceptual framework for thinking about the inferential problem, rather than as a working tool in everyday statistical practice. This was partly because most of the common statistical methods then employed could be derived from, and were often thought of as, large-sample approximations to Bayesian solutions employing flat priors.

There are, however, important exceptions to this general rule regarding the largely theoretical role of inverse probability. Poisson's use of Bayesian methods in his analysis of judicial decision-making is of course familiar; a lesser known, but equally interesting example involves the Tübingen pathologist Carl