

Review of Statistical Inference and Analysis: Selected Correspondence of R. A. Fisher (Edited by J. H. Bennett)

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This volume of Fisher's letters, together with a companion volume also edited by J. H. Bennett (containing his letters on natural selection, heredity and eugenics), completed the corpus of Fisher's major writings just in time for the centenary year of his birth. The companion volume prints the paper on "The Centenary of Darwinism" read by Fisher in Adelaide in 1959. Fisher begins:

The great advantage of celebrations of Centenaries lies in the opportunity they afford to consolidate what has been learnt in a century, and to fix in orderly relation to each other, and to the whole, the diverse movements, some fruitful, some abortive, which confuse the history of current events. A century affords an opportunity of taking a bird's eye view, and of eliminating unjust and erroneous opinions more speedily than would happen in the absence of such a periodic stocktaking.

Fisher's public style, of which this is a fair specimen, is very rich, so that one sometimes needs to read and reread to grasp his full meaning. The style in his Collected Papers is freer, although it can still be somewhat convoluted. He is much more relaxed in these letters, which therefore form an almost indispensable adjunct to the rest of his works. Professor Bennett has classified the letters under the headings Statistical Inference, Statistical Theory and Method, History of Statistics, Teaching of Statistics, History and Philosophy of Science, and Scientists and Scientific Research. Within each heading, the letters are ordered alphabetically by the correspondent's name. The correspondence with

a single individual is made easy to follow by the index.

One point that must be kept in mind when reading this book can be seen from the fact that Frank Yates, Fisher's closest collaborator by far, has only 10 letters (some of the most important ones in the book), whereas there are 43 letters under my own name. This is because, for many years, Yates and Fisher lived near each other, whereas I was never in this position. On the other hand, I did see quite a lot of Fisher when he was President of the Royal Statistical Society, and at other times, so that many of the questions raised in the correspondence were settled in conversation.

One sequence of the letters between us has made me see an unforgettable incident in a new perspective. It occurred at the Centenary meeting of the International Statistical Institute (ISI) held in Brussels in September 1958. At the introductory reception, Fisher, his daughter Joan, George Box and I were together, and the discussion turned to Bayes's famous 1763 paper. Fisher and I differed over what then seemed to me a minor point of interpretation. He thereupon became most abusive, accusing me of threatening to set back the subject for years to come. I could only say that I would go next day to the Bibliothèque Nationale to check exactly what it was that Bayes *had* written. I discovered the next day that the Bibliothèque's run of the Philosophical Transactions began only in 1832, soon after Belgium itself began as an independent state. On returning to London, I found that, as usual, Fisher had been right about Bayes. And now, reading our correspondence for the first time in sequence, it is clear to me why Fisher was so annoyed: he had made his point twice already, in earlier letters, and I had failed to notice.

I report this because it may help to explain the tone of the important letters between Tukey and Fisher, and some of the references to Bartlett and

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