COMMENT AND REVIEWS

COMMENTS ON D. V. LINDLEY'S REVIEW OF SIR RONALD A. FISHER'S "STATISTICAL METHODS AND SCIENTIFIC INFERENCE"

Statistical Methods for Research Workers was first published in 1925. In 1950, to mark the twenty-fifth anniversary of its publication, I was asked by the editors of the *Journal of the American Statistical Association* to make an appreciation of the influence of the book on statistical methods. In the course of preparing this article (Yates, 1951) I took the opportunity of reading the reviews of the first edition. Lindley's review (1957) of Fisher's new work, *Statistical Methods and Scientific Inference*, shows a reaction which is very similar to that shown in some of these reviews. But, as Lindley himself admits, this and the further book *The Design of Experiments* " laid the foundations of statistical science as we know it to-day ". Had Lindley studied these reactions and the background that gave rise to them he might have been hesitant in committing to print his concluding remarks :

" . . . the best that we can hope for is that the book will make statisticians realise the unsatisfactory nature of the Fisherian argument and make them more ready to read and accept the Bayesian argument. The worst that can happen is that it should command an important position just because of brilliant work done by the author thirty years ago."

Why has Lindley found so little of value in this new work ? I think it is because he himself, like so many mathematicians trained in the formalities of theoretical statistics and not working in direct contact with scientific research workers, has no direct experience of the type of inductive inference that is in fact required in the course of scientific research. Thus at the outset of his review he states :

" . . . it is clear that statisticians to-day are much happier in designing or analysing experiments than they are in explaining just why they are doing what they do. For example, new significance tests are continually being produced but the concept of a significance level is not clearly understood."

This, I would submit (as one who has spent a considerable amount of time designing and analysing experiments in association with the scientists concerned) gives a completely false picture of the real situation. Statisticians who have experience of experimental design and analysis are very clear why they do what they do, and if they have a gift for exposition can explain it when necessary. It is the mathematical statisticians without such experience who spend their time producing new significance tests which have no relevance to the material under examination. Thus, for example, M. G. Kendall in his *Advanced Theory of Statistics* (1943) takes as an example the data of a simple factorial experiment (a $2\times2\times2$ design with confounding of the 3-factor interaction) given by me specifically to illustrate the design and analysis of experiments of this type (Yates, 1935), re-analyses it using a number of procedures that any expert in the field would regard as incorrect, and in consequence makes what is in effect a different test of significance, the results of which in fact disagree with my own. Yet he nowhere considers it worth while to inform his readers of this discrepancy in method and conclusions !

It is, I think, this failure to appreciate the type of inductive reasoning that scientists follow and criteria that any system of inductive inference must satisfy if it is to be of any real use, that has resulted in a complete failure by Lindley to recognise the new contributions that the present work has made to the subject.

It is not my purpose in this note to undertake a review of the book nor to enter into detailed discussion of the issues raised by Lindley in his review, many of which are better discussed in journals devoted to mathematical statistics. There are, however, a few points on which I would like to comment.

From the passage quoted above it is apparent that Lindley favours the Bayesian argument. Yet he nowhere comments on the objections to it that have been made for many years by Fisher, and which are summarised and amplified in the present work. Perhaps his idea that he has convicted Fisher of a " mathematical error " has led him to conclude that no further refutation of the concept of fiducial probability is necessary and that as the Bayesian solution is the only alternative that is available this must forthwith be accepted. This is a poor argument for the Bayesian solution !

As for the " mathematical error " itself, this accusation appears to be based on a disregard of the principles Fisher lays down in his book. It is of course well known that if, for example, the full data from a sample from a normal distribution is available this cannot be efficiently combined with the data from a second sample merely by using the value of t in the first sample. But in such a case the values of $\bar{x}$ and s^2 serve to demarcate recognisably different sub-sets. If the value of t is all that is known concerning the first sample—as might indeed be the case if the data were reported by another worker—then the corresponding fiducial probability could be correctly used as a prior probability.

Lindley complains that Fisher has made " an ingenious attempt " to present an argument for significance tests which avoids consideration of alternative hypotheses by taking an example in which the probabilities of more extreme events are negligible. Actually in this example no real problem arises since only a single tail is involved. The subsequent treatment of composite hypotheses and the use of likelihood instead of significance for discrete observations surely removes the need for detailed discussion of what observations are to be considered more extreme in such cases.

Lindley also objects to the fact that geneticists take no account of the number of chromosomes when making a test of significance for linkage. This would seem to me to be likely to lead to just the sort of confusion of evidence that usually results from the introducton of prior probabilities. Chromosomes are of very different length, their number is not always exactly known, and there is no reason to expect that the genes entering into linkage studies are a random selection of all genes. The question asked by the geneticist is therefore rightly : are the values of the observed frequencies such as might arise if the linkage were zero ? The assessment of the evidence on the assignment of genes to the different chromosomes comes later.

Finally, Lindley criticises Fisher for not making adequate reference to contemporary writers and for apparent ignorance of their work. I do not know whether Fisher has in fact studied all the references (including one unpublished thesis !) that Lindley cites—I certainly have not—but surely a word of thanks is rather due to him for the extremely interesting historical review of the development of ideas on probability. F. YATES.

REFERENCES

KENDALL, M. G. 1943. The Advanced Theory of Statistics. London : Griffin.
LINDLEY, D. V. 1957. *Heredity*, *11*, 280.
YATES, F. 1935. *J. R. statist. Soc.* Suppl., *2*, 181.
YATES, F. 1951. *J. Amer. statist. Ass.*, *46*, 19.