

an approach of limited scope, in which all probabilities have a frequency interpretation, in which our answers are nearly objective, but in which personal judgement has to be introduced qualitatively in making use of the answers? Or do we take an approach which appears to take us quantitatively further, but in which our answers are subjective and often difficult to specify numerically at all precisely? At present I favour the first approach, especially where it is important to convey to other people the statistical uncertainty in the conclusions.

A final general comment is that the discussion above is of the question of how to reach conclusions about parameters in a model on which we are agreed. It seems to me, however, that a more important matter is how to formulate more realistic models that will enable scientifically more searching questions to be asked of data.

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Of the previous speakers, I suppose that I am in most general agreement with Professor Bartlett, but at the same time I have a natural sympathy with anyone who is trying to thrash out better ways of handling the problems of statistical inference. Professor Savage has spoken of the enthusiasm with which he sees new lines of thought being opened out in front of him, and perhaps I had somewhat the same feelings round about 1931 when I visited the U.S. and discussed ideas which seemed to be opening out in front of Neyman and myself.

There were a good many references in the previous contributions to the Neyman and Pearson theory; they did not altogether correspond to the theory as I see it, but that perhaps is because my own views have changed somewhat and developed. Of course, through lack of close contact with my partner during the last twenty years, it would be a little difficult to say where precisely the N. and P. theory stands today! I think, however, that a few words on past history may not be out of place, because I believe in the value of emphasizing the continuity as well as the differences in what have been the broad lines of development of our subject. I have the impression that by showing how the same situation is being tackled by alternative approaches the whole subject gains in richness in a way it would not if the exponents of one

line set out to discredit another line by saying it was followed in error!

There has perhaps been a tendency to speak of the Neyman-Pearson contribution as some static theory, rather than as part of the process of development of our thought on the background of statistical theory. N. and P. were after all very much persons of their time. They built on things which they found in the middle 1920's. For example:

(a) The way of thinking which had found acceptance for a number of years among practising statisticians, which included the use of tail areas of the distributions of test statistics.

(b) The classical tradition that somehow prior probabilities should be introduced numerically into a solution. Perhaps only lip service was still being paid to this idea, but one can certainly find some evidence for the strength of the tradition in certain of the writings of Karl Pearson and of 'Student'.

(c) The tremendous impact of R. A. Fisher; his criticism of Bayes's Theorem and his use of Likelihood.

(d) Fisher's geometrical presentation, which first came home to me in the small diagram in his paper on the distribution of a correlation coefficient (Fisher, 1915, p. 509). Out of this readily came the concept of alternative 'critical regions' in a sample space.

(e) Fisher's tables of 5 per cent and 1 per cent significance levels, which lent themselves to the idea of a choice, in advance of experimentation, of the risk which the experimenter was prepared to take of the 'first kind of error'.

(f) The emphasis on the importance of planning an experiment, which leads naturally to the examination of a power function, both in choosing the size of sample to enable worthwhile results to be achieved and in determining the most appropriate test.

(g) Then too there were contributions from 'Student', some of them from personal discussion; I remember particularly his letter to me of 1926 which helped to put the concept of the alternative hypothesis into the picture.

What I think we found, as no doubt Savage and others think that they find now, was a dissatisfaction with the logical basis, or lack of it, which seemed to underly the choice and development of statistical tests. We found this not only in the theoretical work of what was then