specific mass or density, and specific weight or weight per unit volume. The latter he terms weight-density, or (in a footnote) weightivity. Surely it would be better to use the term specific gravity itself in its proper sense of weight per unit volume. By this means extra nomenclature is avoided, and a long-standing confusion is removed. German writers (Westphal, Tomaschek, etc.) already follow this practice, but the dead hand of tradition still keeps it out of the English books.

A further criticism might be that in ranging over the whole field of Physics, including very recent work, the treatment is often rather summary and sketchy. The answer is, of course, that an exhaustive treatment is not intended. One cannot blame a book for not being something which it does not pretend to be. Still, it is undoubtedly over-concise in places. There is also a distinct tendency for new ideas to slip in without proper definition, merely on the strength of some analogy, and a little more exactness at these points would not be incompatible with the purpose of the book. Torque, Rotational Inertia and Electrical Resistance are examples of this.

There are one or two small errors of fact, and a few printing errors, which perhaps are almost inevitable in a first edition.

When all such criticisms have been made, it remains true that the book is an admirable introduction to Physics, for all except those whose heads are buried in the sands of tradition. But whether it is likely to be read much in India is open to considerable question. For the students in our universities, alas, read for the most part only what is prescribed, and a book which has such a flavour of originality is unlikely, one fears, to be brought to the notice of those who need it most.

H. J. TAYLOR.

## Theory of Statistical Estimation

THE fundamental stages in a statistical appreciation of a problem are, its specification through means of a hypothetical population, the distribution, particularly of the statistics which we put forward as estimating our parameters, and finally the problem of estimation itself. While great advances have been made in each of these aspects, there is no doubt that the most striking progress in recent times has been in the researches on the theory of estimation. The notable contributor to this progress is Professor R. A. Fisher himself who chose, quite naturally, the statistical theory of estimation for his Calcutta University Readership Lectures, 1938, "an orderly presentation of the material in book form" having been brought up by Professor Mahalanobis and his colleagues at the Calcutta Statistical Laboratory.

We seek in the problem of estimation the exact properties of a population from its practical model, the sample, and it is inevitable therefore that uncertainty or probability, should attach to all operations from the very beginning namely from even the selection of the sample. The problem of obtaining presumable values was attacked as long ago as 1763 by Bayes, whose theory based upon the "principle of insufficient reason" supplies one answer, though an insufficient one, since the assumed constant probability of a parameter falling within any interval of fixed size is *not* a probability of the kind related to the empirical law of large number. Gauss-Markoff's ideas implied in the "best unbiassed estimated" later adumbrated is one result of a search for something better than the principle of insufficient reason. But the chief difficulty in this method as Dr. Neymann says, is our sophistication in taking as the best what is called the best. Undoubtedly great advances have since been made and the new principle of maximum likelihood estimate, originally due to Karl Pearson himself; and later refined by Professor R. A. Fisher now holds the field. Its chief justification lies, as even the justification for Markoff's unbiassed estimates lies, in that, under certain limiting conditions, when all the observations are mutually independent and their number $n$ indefinitely increases, then it becomes less and less probable that the $m.\ l.$ estimate will differ by so much from the parameter that is being estimated. Dr. Fisher's lectures to the Calcutta University reviews this position, in particular in sections 6, 7 and 8 of these lectures. His argument is as follows:—

"If, then, we disclaim knowledge *a priori*, or prefer to avoid introducing such knowledge as we possess into the basis of an exact mathematical argument, we are left only with the expression

$$\frac{n!}{a!\ (n-a)!}\ x^a(1-x)^{n-a}$$

which, when properly interpreted, must contain the whole of the information respecting $x$ which our sample of observations has to give. This is a known function of $x$, for which, in 1922, I proposed the term 'likelihood'; in view of the fact that, with respect to $x$, it is not a probability, and does not obey the laws of probability, while at the same time it bears to the problem of rational choice among the possible values of $x$ a relation similar to that which probability bears to the problem of predicting events in games of chance. From the point of view adopted in the theory of estimation, it could be shown in fact that the value of $x$, or of any other parameter, having the greatest likelihood, possessed certain unique properties in which such an estimate is unequivocally superior to all other possible estimates."

On p. 29, he enunciates (and also supplies a proof of) the proposition.

*Proposition*: Of the methods of estimation based on linear functions of the frequencies, that with smallest limiting variance is the method of maximal likelihood, and for this the limit in large samples of $\frac{1}{nV}$ is equal to $i$.

His conclusion, if it is at all possible to state that briefly, is probably best stated in his own words.

"The problem of estimation is to find from the sample point the most appropriate point on the curve of expectation. Thus every method of estimation is virtually equivalent to dividing up space into what may be called equistatistical regions such that every sample point on the same region leads to the same estimate. The criterion of consistency then simply states that the equistatistical region leading to any estimate of $\theta$ should actually cut the curve of expectation at the point corresponding to this value of $\theta$. Efficient statistics have the peculiarity that the equistatistical region corresponding to such a statistic cuts the curve of expectation at right angles in the transformed space. The maximal likelihood solution is unique in that, in addition, its equistatistical region is linear. The equistatistical regions for minimum are not linear and touch the maximal likelihood regions on the curve of expectation."

In his last lecture there is a brief account of the manner of utilisation of the information recovered by ancilliary statistics, but the main centre of interest in his lectures lies in the statistical theory of estimation itself. Now it is well known that in any practical example the problem facing us is *what value shall we take as the* value of the parameter, and I am afraid we do not have an unequivocal guidance in such difficulty. Let us say, we are equal to the arithmetical labour involved in calculating both the Markoff best unbiassed estimate, and the Fisher *m, l,* estimate, and if these two do not agree, which are we to choose.

The following extract from the discussion that followed a Conference held in Washington in April 1937 at which Dr. J. Neyman dealt with statistical estimation (published in mimeograph by the U.S. Department of Agriculture, 1938) may not be irrelevant in this connection.

"Mr. Wallis: Doesn't Fisher claim that maximum likelihood solutions will always be minimum variance solutions also? I thought that Fisher claimed that he would get the 'best' estimate by the method of maximum likelihood.

"Dr. Neyman: I am aware of these claims. However, the proofs advanced by Prof. Fisher to support them were not considered satisfactory by many mathematicians and recently several interesting papers have appeared on the subject. As a result, many of Fisher's statements partly in a modified form and under certain limiting conditions, proved to be correct. I do not remember whether the particular claim you mention was found correct or wrong, but I will quote here papers by Hotelling, Doob, Dugue and Pitman, where you are likely to find the answer.

"But my point is that the question whether the variance of the *m, l,* estimate is minimum or not is not relevant from the point of view of the goodness of the estimate itself. In the above example, the variance of $g$ is smaller than that of $g_1$, but does this circumstance prove the absolute superiority of $g$ over $g_1$?"

It is very difficult to get a connected account of the Theory of Statistical Estimation except by wading through a number of periodicals but this brochure supplies a long-felt want.

We look forward to Professor R. A. Fisher to analyse with his characteristic powers of rigour and insight to further place the whole of this theory on firm and practical lines.

K. B. MADHAVA.