

27th February, 1956.

My dear Mourant,

Looking at your paper on the Sikha, I see that you say "gene frequency calculations were performed by the maximum likelihood method of Ceppellini, Siniscalco and Smith (1955)". I have looked at this paper and note that the authors' say "The method is shown to be equivalent to maximum likelihood and is therefore statistically efficient". This may well be so, but the exposition is so complicated that I find difficulty in applying any check of its equivalence. There is, of course, no novelty in the so-called counting of genes in that all efficient scores will credit homozygotes with coefficients double those of heterozygotes for the same genes.

My question is, however, did you, or anyone in the Anthropological Institute's unit, really go through all its complication, and, if so, in what respect was the arithmetic in any way simplified compared with the direct maximum likelihood examples, published by other authors, which do have the advantage that the scores can be exhibited and seen to balance?

I am, of course, passing the paper for publication without

delay, but should be glad to know just what has happened to change your attitude to maximum likelihood techniques.

Yours sincerely,

If you happen to have the workings for your MNS series in 6 phenotypes it would make a good example for my Part II class which is doing M.L. fittings.

2nd March, 1956.

My dear Arthur,

Thanks for your letter of March 1st. Please do not trouble to send the data and fitted values for the Sikhs, if they have not been reduced by the method of Ceppellini et al., for I only wanted them in order to check that this method gives, as its authors seem to claim, the same results as maximum likelihood. As there are people all over the world, or at least in Australia, Brazil, Boston, etc., who have published rather simple methods, much more intelligible than that of C.A.B. Smith, for solving the equations of maximum likelihood, I presume that Mrs. Kopec's attitude is that only the method of Fisher is intolerably difficult. Now at any time during the last thirty years very authoritative and earnest teachers have been assuring statisticians that Fisher's methods of fitting are so intolerably difficult that in preference they should employ other methods, such as that of moments of Karl Pearson, or of others that have ^{and} sunk below the horizon. I was therefore rather grieved to find in your book such volcanic eruption of the same propaganda. It is actually

quite baseless, as I or numerous statistical teachers in different parts of the world, find their pupils quite able to make efficient fittings with the method of maximum likelihood, and I feel pretty confident after glancing at the paper by Ceppellini et al., the paper to which you refer, that this will not be found to supply any simpler "just as good" substitute. The algebraic development, however, is so complex as to be opaque, and I cannot tell whether it is exactly my method heavily disguised, or some different variant that is being offered. That was my only reason for wanting to secure data that has been fitted actually by this method. Perhaps Mrs. Kopec can supply me with some.

I have verified in other cases that the methods of your book are not equivalent; and this I do not criticize, for it is for you to judge what tolerance limits you choose to use.

Sincerely yours,