24th March, 1956.

My dear Arthur,

Judging from your letter I think we now understand each other about maximum likelihood. Probably you were misled by thinking of it primarily as a means of discussing blood group data, whereas, of course, the method was established before blood groups were heard of, and its quite general advantages were early understood.

Of course particular types of data offer particular difficulties for which, from time to time, special methods were proposed, such as my paper in the <u>Annals</u> showing a general treatment when the number of adjustable parameters is large.

The story was complicated by old Karl Pearson wishing to defend earlier quite indefensible methods of his own, and by the loyalty of his son in endeavouring to re-fight his father's lost battles. Their line has been that the method of maximum likelihood is impracticably difficult, whereas the method of moments, though exceedingly laborious, was practicable. This kind of propaganda was answered on their own ground by people like Koshal

applying maximum likelihood to typical Pearsonian problems. I
did not like your book being used for analogous propaganda.

What Ceppellini and Smith have done is to put forward an
iterative method intended to converge on the maximum likelihood
solution as, of course, previous methods have done. On the
question whether it does so converge, I think you should ask
Smith for a proof if he wishes to make this claim. As you have
found experimentally, it is by no means always the quickest
method and it needs demonstrating that it is ever so, for in
each particular case special simplicities may often be observed
and taken advantage of. Certainly Stevens' first shot, when he
was very new both to blood groups and to maximum likelihood.
procedures, was not very neat and expeditious, but no-one looking
at the problem since his time has found it a particularly diffi-
cult one.

Sincerely yours,