17th February, 1955

My dear Ron,

I am returning your manuscript herewith. I found the second chapter very interesting reading.and The use of likelihood to deal with discontinuous data is novel to me - perhaps it shouldn't be - and certainly appears to clear up a lot of the troubles that arise with confidence belts. Its full implications in the varied cases met with are naturally not yet clear to me. I look forward to the variety of examples which you promise for the later chapters. It may well remove one of the logical difficulties which I am frequently encountering and which I may perhaps make clear by a specific example as follows.

You may remember the analysis of Bonnier's data which I gave to the Genetical Society a year or so ago and which consisted of a set of experiments on mutation rates of drosophila comparing two different methods of irradiation. If the data from all the experiments are pooled as may legitimately be then under certain circumstances there is an element of discontinuity which affects the significance level of the combined results. This element of discontinuity does not arise when the data are not pooled owing to the sub-division and ordering of the probabilities of different events. Thus the pooled data may be represented in the form

	Treatment		Total
	1	2	
Mutant	n_1'	n_2'	n'
Normal	$n_1 - n_1'$	$n_2 - n_2'$	$n - n'$
Total	n_1	n_2	n

Many different orderings of the 2 x 2 tables relating to the separate experiments will give the same combined table. Any analysis which takes into account these tables, therefore, has only very mild elements of discontinuity. Consequently such an analysis tends to give a higher level of significance than the analysis of the pooled data.

There are one or two minor points I might mention now. The first is the distinction between confidence limits and confidence belts. The Neyman-Pearson school always seems to me to be getting into difficulties with the two tails of the distribution. From the practical point of view I have never felt this difficulty, as the last thing that is required in practice is to treat deviations in the two directions together. In other words, one requires fiducial limits or confidence limits for the upper and lower 2½% separately, rather than a confidence belt for 95% as a whole.

At some point it might be worth dealing with the use of estimates other than the sufficient estimate when one exists. This, though not of logical is of practical interest as the sufficient estimate is sometimes somewhat tiresome to calculate.

On p.21 you say that the aggregate of cases of which the particular experimental case is one could certainly be sampled indefinitely to demonstrate the correct frequency. I do not see how this could in fact be done.

I did not get the implication of the sentence at the top of p.29.

I am much looking forward to a discussion. I believe this book is going to do a great deal to clear the air of a lot of misconceptions.

Yours sincerely,

Jack,

Sir Ronald Fisher
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P.S. March 18th, 23rd, 24th, 25th and 31st seem to be possible dates at the moment. Perhaps you would find out from Barnard what suits him.