St John's College
Cambridge.

1937 June 5.

Dear Fisher,

I wonder whether I may bother you about pp.12-21 of this paper. You won't agree with the earlier part, I expect ; but ~~tha~~ some things of yours that I have seen in various places are very near some remarks of mine here, and you may have been even nearer in other places that I have not seen, and I should like to get the references right.

On pp.93-101 of Statistical Methods (1925 edition), you recommend grouping to test individual degrees of freedom separately, though you don't go as far as I do here (p.14) ; but you may have gone ~~tha~~ further later. The trouble arose lately when I was testing for normality a series of observations given by Bond.

p.15. I saw a letter of yours some time ago where you specifically recommended doing several experiments at once, and in my line of business the earth usually does a lot at once for me.

p.20. All I have here is really a rather vague impression, and I should be glad if you would confirm or contradict it. The main thing is that you have the kind of information that is needed to bring the prior probabilities up to date in many subjects. But my assessment says that a systematic difference suggested is as likely as not to be there, and that if it exists it is equally likely to account for any fraction of the r.m.s. departure. On your information it may be possible to say whether these still hold.

I have asked the Press to send you a copy of the new Sci.Inf. with the addenda. I should really have liked to scrap the lot and do it again, but at the present rate it looked as if the thing would take about 60 years to sell out. Looking at it again I don't seem to have made enough of the places where I agree with you, but

I think I have remedied that in an article I have sent to 'Nature' for the Dingle controversy.

When I have time I should like to write a note showing how some of your methods fit into my theory. Maximum likelihood is obvious (I can't understand why K.P. was sticky about this, since m.l. for a large number of observations is an immediate consequence of inverse probability, and K.P. always accepted inverse probability) but sufficient and efficient statistics also have a definite place. The situation in my method indeed is that it goes straight for the efficient statistics and the mathematical difficulty comes in when we have to decide what to do when somebody has provided only inefficient ones.

Yours sincerely,

Harold Jeffreys.