St John's College
Cambridge.

1937 May 13.

Dear Fisher,

Thank you very much for your letter and papers. Your letter confirms my previous impression that it would only be once in a blue moon that we would disagree about the inference to be drawn in any particular case, and that in the exceptional cases we would both be a bit doubtful. I am particularly glad to note your treatment of an unknown systematic error as a motion not before the House, while a specified one can be treated on its merits. This is what I have been saying about the 'excluded middle' for a long time.

Your letter encourages me to ask whether you also agree with me about the ideas of Eddington and Milne that all valid observations can be deduced by pure thought, without reference to other observations. As far as I am concerned this opinion does not agree with experience, and I prefer the alternative explanations that E. and M. are either very skilful in selecting their observations or very lucky. I agree completely on this with Dingle's recent Nature article.

It was my disagreement with Eddington over this matter that first brought me into the subject in 1919, and I have been trying at intervals ever since to do for induction what Whitehead and Russell did for deduction in the introduction to Principia Mathematica; to get the postulates down to a minimum. You are doing the same. My only objection to your treatment of estimation is in the infinite population, which you introduce only in order to give a meaning to probability, while recognizing that it is hypothetical. I recognize that probability is hypothetical, so that the number of hypotheses is the same. It only happens that I find my way easier to understand. Any theory of induction is bound to be a bit fuzzy at the edges, but apparently you recognize that too, though you put the fuzziness at the fiducial limits, whereas I put it into the practical difficulty of assessing the prior probability when the previous data are abundant but inadequately classified.

I am writing this because there is a tendency about to attribute what I believe to be an entirely exaggerated idea of our disagreement to us, for which we are both possibly partly responsible, and I think an occasional mention of cases where we agree would be for the good of the subject. I have a paper coming out soon in the Proc. Roy. Soc. partly on what had seemed to me the entirely mysterious fact that Student's z distribution and mine obtained by the inverse theory are identical ; there turns out to be a perfectly genuine reason why they should be the same in form, and with a trifling extra assumption Student's result can be made into an alternative proof of mine.

Yours sincerely,

Harold Jeffreys