

St John's College  
Cambridge.

1938 Nov. 27.

Dear Fisher,

There are some reasonable people on the Council ; I have passed your literature on to one of them, who was very outspoken. It does seem a bit thick that they should have published Bartlett's paper without consulting you and then made it a condition of accepting your paper that you should get his permission. Wilson's use of 'trivial' also doesn't strike me as a suitable word in the circumstances. However he was ruder in the correspondence I have had with him - at least it seemed that way to me. *The matter is not ended!*

I had heard vaguely that you had left the Society, but had no notion of what it was about, and rather thought it might have <sup>been</sup> ~~been~~ over my 1936 significance tests paper - in which case I should have agreed with about 3/4 of what you said. The Editorial Committee consists of Hardy, Hodge and Hall (pure mathematicians) Wilson (quantist, knows about metals) Dee and Ratcliffe (Cavendish physicists, young and enthusiastic.) Except Hardy I don't think any of them is over 35 ; the pure mathematicians all have experience of the L.S.S. council, but I should doubt whether the others have had experience on any other society. The trouble about pure mathematicians is that their decisions can be so clear cut in their own job ; a result follows from the postulates or it doesn't, it is new or it isn't, and it doesn't matter what the postulates are. A paper can be short and to the point, in which case it is trivial, or long and (to a Philistine like me) not worth doing anyhow, and then it is important. *It's a bad training for statisticians. A mathematical opinion would be more to the point; they don't believe in universal agreement. Frank Ramsey called some of Principia Mathematica "slippy" in the L.S.S. & Russell was the referee. His report was quite right, it is."*

~~But~~ I asked the Editorial Committee to refer my dispute to the Council, but Wilson gave me such an abbreviated account of the result that it was quite unintelligible, and with some difficulty I have managed to get something out of other officials and members. They seem to have dragged in a lot of irrelevant matter and thereby got a decision that looks in their favour, but on the questions that I wanted the Council to attend to the rulings seem to have been in mine, I hope to get something more definite, but there will not be another meeting this term.

By the way I have a little paper on the extension of the minimum  $\chi^2$  method to the case where some of the groups are empty. May I send it along? I may be doing it in lectures this week so can't do it now. It took me some time to see your point about heavy grouping; it always seemed to me that unless the observations are in about the minimum number of groups that will give a determination at all, the grouping corrections are negligible in comparison with the uncertainty that is there anyhow. But with a Type I law with an index ~~of  $\frac{1}{2}$  to  $\frac{1}{2}$~~   $\frac{1}{2}$ , the ~~xxx~~ terminal observation has such a lot to say that to treat it as one of a group involves a serious loss.

The charming feature of the C.P.S. affair is that there is reason to suspect that one of the referees was Eddington, whom I had reason to tick off recently for giving an answer that involved differentiating an infinite number of times a function that is zero except at a finite number of points, where it is infinite; justified by introducing a polynomial whose integral from  $-\infty$  to  $+\infty$  is 1. The R.A.S., I gather, accepted it with enthusiasm.

Yours sincerely,

*Harold Jeffreys*

*would you show the enclosed to B. Storer? He expressed interest at the B.A.*