February 15, 1939

Dear Jeffreys,

Thanks for sending me Hodge's letter, which I return herewith. It is good of you to suggest the possibility of my rejoining the Society, but I am glad you made it clear that this was your own suggestion, as I only mentioned the affair to you in corroboration of your own impression that the affairs of the Society were being pretty badly mismanaged.

So far as I am concerned, I resigned for an entirely specific reason, which I believe I made clear to the Secretary, namely, that the right of answering a misunderstanding and, as I think, a misrepresentation, of my own work in the Society's Proceedings was peremptorily denied. I suppose I ought, at the time, to have taken the matter up with the President, whoever he may be, but in the circumstances I was content to leave the matter as it was.

I might, indeed, sometime, like to rejoin the Society, especially if I ever came to Cambridge, but I should really hesitate to do so if the Secretary at the time thought the right way to treat Fellows whose work had been criticised in the Proceedings was the way in which I had been treated.

Some of the formulae you give are to be found in the little Appendix on technical notation at the end of the third chapter of Statistical Methods: p.77 of the 7th edition. The last one can be checked from the formula

$$\kappa(s^2) = \frac{1}{n}\kappa_4 + \frac{2}{n-1}\kappa_2^2$$

substituting $\kappa_2 = \mu_2$ and $\kappa_4 = \mu_4 - 3\mu_2^2$ This I take from p.206 Proc. Lond. Math. Soc. 30: 1928 or 9. If you want to see what the k κ notation will do, I should look at this paper, because they simplify what, in any other notation, are really very heavy formulae. In fact, ~~really~~ literally hundreds of pages by Tchouproff and others have been devoted to developing formulae for only the simpler cases. A partitional represent-ation is, in fact, essential even to gaining a clear idea of the classification of the relationships which exist.

Is this the kind of thing you mean about a terminus? If the probability of an observation differing from the terminus by less than x is, when x is small, ax^{α}, then the chance that x independent observations all exceed x is $(1-ax^{\alpha})^n$, so that, if p is this probability, supposed not very near to 0 or 1, $-\log p = nax^{\alpha}$, ~~so that~~ and, for given p, x varies as $n^{-1/\alpha}$. This decreases more rapidly than $n^{-\frac{1}{2}}$ if α is less than 2, i.e., if the frequency curve either meets the axis at right angles or terminates at a fixed ordinate, or tends to infinity at the terminus, so that from an approximate estimate of a and one could, putting $\log p = -1$, for example, estimate the terminus from the end observation with increasing advantage, compared with any ordinary method, as n is increased.

I should certainly like to see this possibility investigated, as it introduces a group of cases for which the notion of amount of information as I have defined it is inadequate, since the expressions of my definition no longer converge, and the amount of information must be called infinite. Comparison among different possible estimates all containing infinite amounts of information in the limit are obviously not very helpful. The difficulty is, indeed, abrogated in Koshal's case, for his primary data are grouped, and in such cases the amount of information is finite, but I should very much like to know by what more extended concept amount of information could be replaced so as to compare different possible estimates in finite samples when the distributions of these estimates supplies more than a finite amount of information.

I expect you have noticed the convenient way in which the scores (in table ~~20~~ XX of our Tables) for ordinal data, work when one or more variables are distributed very abnormally. ~~are arrange~~d.

Yes, scandalous as it may seem, we took the supposed durations from the Bishop's great book, without giving the time needed to discover where he got them from. I rather hoped that, in consequence, someone would call our attention to some more critical estimates. I imagine that really it is not impossible that the age of the planet exceeds 400 million years, even though a lower estimate is preferable.

I hope you enjoyed two letters in the Times - I think yesterday - in which McBride gets in full measure what he has been asking for for a good many years.

Yours sincerely,