

Feb 24.

Dear Fisher,

Thanks for your two letters. I passed the relevant passage of the first to Hodge, but White is taking steps at the Council. The result remains to be seen ; I gather from reports that things did not go so completely in the Editorial Committee's favour last term as appeared from Wilson's letter to me. The important thing, it seems to me, is that the Council should make definite rulings on some matters of principle. I should very much doubt whether Wilson is the real villain of the piece ; he is not very impressive at close quarters, however unfortunate his way of expressing himself in writing may be. The procedure suggests Hardy to me more than anybody else, as he has the right combination of pig-headedness and reputation; of course always quite honestly convinced that he is right.

I'm pretty sure they wouldn't invite Yates to send a paper ; that would be too big a reversal of the normal order. Presumably they would refer it to Wishart. I am a bit lost over this really because I failed to make anything whatever out of Bertlett's paper, and haven't seen either your earlier or your later one. Anyhow I think the right answer is the one in my P.R.S. paper, but I don't see Wishart advising rejection on that ground.

Thanks for references about moments. I had forgotten the one in S.M. The L.M.S. looks impressive.

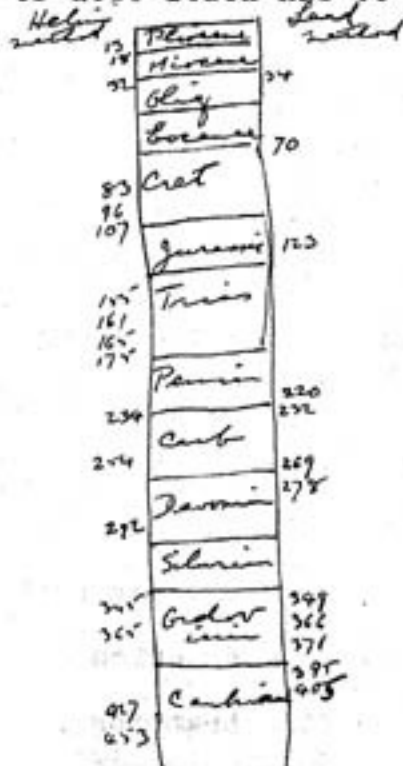
I don't think there is much point anyhow in considering the result of an infinite number of cases, because what we always want is an approximation for a finite number. I should say that what was wanted here would be the expectation of the correction and of the square of its error. I got a surprise to find that for the triangular

distribution the difference between the extreme observations, so corrected, is better than the s.d. in estimating the range, and the mean of them is nearly as good as the mean of all. For this kind of case the posterior probability is very skew anyhow, ~~and can't be expressed by two parameters,~~ but when the results are liable to have to be used for something else, and there is a limit to what one can be bothered to take into account, an expectation and its s.d. are useful. It doesn't concern me much, as my laws run the other way. I have however had to cook up something analogous where the information ^{about x} went into the form

$$\alpha < x < \beta ; \alpha = a \pm s ; \beta = b \pm t.$$

I haven't got your tables yet but it looks as if I shall have to.

I expect the Bishop took the ages from my book "The Earth", but directly or indirectly they must surely have come from Holmes. I have not compared them to see. Holmes's little book "The Age of the Earth" (Nelson 1937) is the most up to date. He shows the results in a diagram on p.178, pretty well the only way as some interpolation by rates of deposition has to be done. Ages in m.y. Pre-Cambrian



runs back to about 1800 m.y.B.C. 4000 m.y. would mean stretching the data a bit as there are two lines suggesting about 2500 as an upper limit. I had the idea once of trying to fit a Pearson curve to see if there was any hint of a terminus, but there were too many internal correlations.

On interhal correlation, I had an appalling case the other day in trying to test the annual periodicity of earthquakes. Six years' monthly totals, by a straight analysis, gave a pair of terms with $\chi^2 = 70$. Good enough? No. Substituted in the totals it left residuals with χ^2 about 350 on 69 d.f, and increasing the s.d. to allow for the non-independence (due to aftershocks) reduced the harmonic contribution to about 14. But there was still a strong tendency for residuals to have the same sign in successive months; tested against an even chance it gave my $K = 1/1400$. So the only thing left was to ~~xxx~~ make separate determinations for each year and find an uncertainty from the differences between years. When this was done it left $K = 1.5$.

I am suggesting that it would be a good idea if somebody would renumber the Pearson types. They reduce to three main types, three transitional cases (one of them normal) and the rest are merely taking special values for the indices. The present order is hopeless; actually VIII and XI are identical except for a change of origin. Is any instance of XII known? The times of arrival of a train between here and London should give a pretty good VI, and VIII applies to earthquake aftershocks.

Nobody seems to have much sympathy with MacBride; he was right in suggesting that I don't know enough geology, but he was most unfortunate in his choice of a moment to rub it in.

I have been nursing myself for a sort of flu all this week and am just beginning to notice things again.

Yours

Harold Jefferys