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Dear Fisher,

I wrote to Hodge as Editor of the C.P.S.Proceedings, suggesting that the Editorial Committee might climb down a bit over your row with them. The suggestion that you might rejoin if they did was my idea, as I told them. My motive is the selfish one that I like to have my mistakes pointed out before publication, and so far in the C.P.S. I have been able to get away with anything if it was wrong. I enclose Hodge's reply, which is as cordial as one could wish, though I should welcome something a bit more specific and am answering with a couple of suggestions that they might adopt. One of them has, I have been unofficially told, been adopted already, but I want to make it official.

By the way somebody must have done the following. A law has moments 1, 0, μ_2, μ_3, μ_4; a set of n observations is to be made. What, given the law, are the expectations of $\sum_r(x_r-\bar{x})^3$, $\sum_r(x_r-\bar{x})^4$, and $\{\sum(x_r-\bar{x})^2-(n-1)\mu_2\}^2$? I get

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$$E\{\sum(x-\bar{x})^3\} = (n-1)\left(1-\frac{2}{n}\right)\mu_3$$

$$E\{\sum(x-\bar{x})^4\} = (n-1)\left[\left(1+\frac{1}{n}+\frac{3}{n^2}\right)\mu_4+\left(\frac{6}{n}-\frac{9}{n^2}\right)\mu_2^2\right]$$

$$E\left[\{\sum(x-\bar{x})^2-(n-1)\mu_2\}^2\right] = \frac{(n-1)^2}{n}\mu_4-(n-1)\left(1-\frac{3}{n}\right)\mu_2^2.$$

I can find plenty of harder things done, but none of them gives me quite what I want, and K.P. neglected the allowance for finite n in all the treatments of his that I have seen, though he was pretty badly down on Spearman for dropping it. I should be happier that I had got the algebra right if I knew that somebody had got the same. There is a curious point about the efficiency of the second moment when the fourth of the law is infinite. It is 0 by the formula, but that is really because the chance of error in it follows the same sort of law as the mean does for $dx/(1+x^2)$; there may be a high concentration of the chance near the proper value and the real fault is in trying to use the second moment of $\Sigma(x-\bar{x})^2$ as the sole criterion of uncertainty. μ_2 from $\Sigma(x-\bar{x})^4$ ~~It may be quite good for a few observations but it~~ may have an uncertainty small enough to be useful, but it won't decrease like $n^{-1/2}$.

I have ~~now~~ got the extension of χ^2 to the case of empty groups. It is

$$\text{const} - 2 \log L = \Sigma \frac{(m_r - n_r)^2}{n_r} + 2M$$

where n_r are the observed numbers, m_r the expectations in the occupied groups, and M the total expectation in the empty groups. There seems to be no great harm in applying this to unit groups, though the extension could be done easily if anybody wants it. My attitude is that it is right if applied to a lot of unit groups, and if an answer depends on a single unit group it is not going to be much good anyhow,

Yours sincerely,

Harold Jeffreys

I thought that for a thing like locating the end of a rectangular distribution, where the uncertainty is of order $1/n$, inverse probability and maximum likelihood would differ by $O(1/n)$. They differ by $O(1/n^2)$, so the difference is even less important than in the ordinary case. I think it would be useful for dealing with the Koshal type of distribution if there was a table of the optimum correction (as a function of n & the index) to be applied to the terminal observation to give the terminus of the law ; there seem to be many cases where this would be so good that nothing else

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could alter it appreciably, and it could be taken as a datum in estimating the other parameters. For the rest, χ^2 seems likely to be better than most things in sight and not more trouble.

I am coming down very much in favour of Spearman when there is no particular reason to expect the normal correlation surface. K.P. seems to have thought that what was wanted was a way of getting $E(xy)/E(x^2)^{\frac{1}{2}}E(y^2)^{\frac{1}{2}}$ and that the best way of doing it is to compute r in the usual way, no matter what the law. He misses the point that the correlation so defined between x and x^3 between ± 1 is only about 0·9, though there is an absolute relation between them. If the law is skew r doesn't measure anything particular, but the rank correlation still measures departure from a monotonic relation. That is why it gave too high a correlation in some of K.P.'s cases - it did its proper job. I think K.P.s estimate of its uncertainty can't be far wrong in any case.

Is the E.W.Barnes you quote for geological time the Bishop ? Or have I overlooked somebody I should know about ?

H.J.