St John's College
Cambridge.

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Dear Fisher,

Who originated the result that the chance of the mean of n errors following the law $dx/\pi(1 + x^2)$ follows the same law as one observation ? The only published reference I know is the one in your Phil.Trans., but you don't give the proof and your treatment might apply either to something that you had just noticed yourself or to a result that you had seen somewhere and supposed to be already well known. You were beyond the range of letters when I wanted to know before, and nobody here seemed to know. The proof by the moment-generating function is not difficult, but you may have had some other.

I tried the next stage, $2dx/\pi(1 + x^2)^2$, recently, in the hope of finding out in what sense the probability of the error of a mean can be said to tend to the normal when the fourth moment is infinite. I can't do it exactly, but the tail follows the same law with an extra factor $n^{-\frac{1}{2}}$, if the law is rescaled to keep the second moment the same. I expect to be doing both cases in lectures next term ; my feeling is that the usual proof of the normal law is unnecessary if the separate laws are like Type II and bunk if they are like Type VII, and that special cases are more instructive. Hence my wish to get the reference right.

I sent that correlation paper to the R.S., with a covering note explaining that it was really a translation, but they appear to have accepted it nevertheless.

By the way your lad Stevens has a paper in the Annals on a problem very similar to the one I had in earthquake aftershocks, in dealing with the diagonal elements of a contingency table. In my problem the thing was so glaring that I could do it in bits ; the main thing was to get rid of the diagonal effect before I could see whether there was anything systematic anywhere else. But if ~~xkm~~ it is less obvious, and the whole of the diagonal elements must be combined, Stevens hasn't got the most efficient method. He just adds all the elements together and compares the sum with expectation. But if there are 50 specimens in one row and 3 in another, then if all 3 of the second row come on the diagonal it is better evidence than if there is an excess of 3 in the diagonal in the row containing 50, so that the lumping of the diagonal elements sacrifices a lot of information. In my problem, and I think also in Stevens's, it is better to introduce a parameter a common to all rows, such that there is an excess chance a that an observation, given its row, will be in the diagonal, the rest of the chance being distributed ~~a~~proportionally among all columns. I.e. the chances take the form $(1-a)p_s + a\delta_{rs}$ where $\delta_{rs} = 1$ or 0 accordingly. Then the business part of the test is the ratio of the maximum likelihood solution for a to its standard error.

Yours sincerely

Harold Jeffreys

I am wondering whether I dare renumber the Pearson Types. His numbers are a complete muddle. The main types are I (real roots, possible values between them) VI (real roots, possible values running from one root to ∞) and IV (roots imaginary). The rest are transition and degenerate cases. The position of III, which is a transition between I and VI, between II and IV, with the latter of which it has nothing to do, is anomalous.

I was glad to see your review of Yates. His work strikes me as very elegant, though I don't know how much use is actually being made of it.