September 21, 1938

Dear Jeffreys,

Thanks for your letter. I think there is no doubt at all that K.P. was talking, not about the strict binomial, but about something rather less well defined. His Moment formula is, of course, correct for a polygonal figure made by joining successive binomial ordinates together with two additional zeros outside the range, and one of the things that puzzled me was that he should have spoken of such a figure as a binomial distribution. Neyman quotes no other writer as having used this terminology in 1895, or at any other time, and I imagine K.P. was quite confused on the subject, and that his paper under discussion shows ~~he has taken~~ him as taking some steps towards a clearer discrimination. That he had not yet got it clear is shown by his treating the polygonal figure as equivalent to what is wanted for grouped data.

The second point that puzzled me was his assertion that the exact formula for the true binomial had been "hitherto deduced as the approximate result by assuming the binomial should be approximately a normal curve." Actually, as Neyman says, he gives

in the same paper a calculation which contains no reference to the normal curve. (Neyman, foot of p.14). In view of this it can only be supposed that Pearson intended to say that his predecessors had arrived at the formula by an inaccurate, or, at best, approximate process, and that his demonstration was the first accurate one, whereas I should guess, without verification, that the accurate treatment of the true binomial goes back to de Moivre, and was not overlooked by Laplace and Poisson. Neyman offers no explanation for this rather extraordinary claim.

The third thing that is puzzling about the footnote is that it shows that Pearson regarded the formula containing ξ_1 as of "considerable importance".

It is probably also significant that, after the publication of Sheppard's correct treatment of grouping in the following year, we hear no more of correction for trapezia. It would appear, at least, that Pearson discovered no practical application for the result which, in 1895, seemed to him of considerable importance.

My criticism of the Pearsonian curves is aimed at the curves as a comprehensive system intended to cover all ordinary ~~not~~ non-normal distributions, and at the corresponding system of treating them as subjects for University education, which had been greatly

overrated. In different passages of his attack on Koshal, Pearson complains (a) that the method of maximum likelihood is being widely used by the young and foolish, and (b) that the only case he could find of its application to Pearsonian curves was that of Turner and Koshal. Naturally, he does not collate these passages and conclude that the Pearsonian curves are not being much used in biological research, which, of course, is the fact.

Criticism of the four-moment system should not, of course, be taken as implying that none of the special cases within it, of which the normal is one, did not, in fact, occur in practice. Actually Helmert had run into type III in 1875, as did Pearson in calculating the χ^2 distribution. Gosset ("Student") got type VII, or what, at one stage, was called type III in his problem. In fact, I think any simple mathematical form might prove to be of real importance. Do you not think that excessive values in the fourth moment in astronomical observations are usually due to the varying accuracy with which different observations are made. A mixture of normal ~~laws~~ distributions with the same mean gives, of course, a positive excess. I think if anything is said in the Annals about Neyman's note, it will have to answer it more explicitly, as he does not touch on the real evidence

that K.P.'s thought on grouping was confused at this period. There is no crime in that, but it seems worth while to stress the fact that Sheppard brought clarity into a point that was obviously worrying K.P. Biometrika is the only proper place for your note.

I enclose what I have written so far in Gosset's obituary. The part about samples of two commences on p. 7. Please let me have the thing back, as I shall very soon want to send it in.

Yours sincerely,