St John's College
Cambridge
1931 July 30.

[1937! (JHB)]

Dear Fisher,

I have read your paper on the methods (plural intentional) of moments with interest. A bit on the severe side, perhaps, but probably called for. However you tempt me to ask whether you would consider a short paper for your journal on the relation of maximum likelihood to inverse probability. An idea seems to have grown up that they are opposed, and I think this is very much in need of correction. It is most odd that K.P. always accepted i.p. and yet was sticky about likelihood, and adopted a method of fitting that was incohsistent with i.p.. Wrinch and I gave the essence of the relation, for sampling, in our 1919 paper, but it would stand a bit more generalization. We might have gone further at the time, but for one thing we thought it was obvious, and for another we thought that statisticians already used it; as indeed they did for sampling and fitting normal distributions.

I am interested in your doubts about Pearson curves as such. They have not come my way much ; I once got a distribution of the $x^p e^{-ax}dx$ form, in radioactivities of rocks, but have not had anything more complicated. In seismology the law of error is practically a normal law with a uniform distribution superposed ; the infinite Pearson curves don't seem to fit the outlying observations very well, but it seems possible to locate the normal distribution very well without anything more complicated than the dodge I introduced in my Strasbourg paper, which is quite complicated enough when there are some hundreds of groups to fit. However I should like to know whether there is any evidence (1) that the K.P. function with infinite tails both ways is better than the exponential of a quartic, which would make the method of moments right (2) whether the one with finite limits -a and +b is any better than is got by taking a new variable

$$y = \log \frac{x+a}{b-x}$$

and fitting a normal distribution ? The functions would no longer look like a single family, but I don't think that is any great objection. The K.P. ones aren't really because an imaginary tail is rejected to keep the results real.

In astronomy the question of correcting an observed frequency distribution for a known standard error is continually coming up. Eddington gave a method a long time ago (reproduced in Brunt) which is mathematically equivalent to solving the heat conduction equation for negative time. If he had the true probabilities presumably it would be all right, but it is applied to the observed numbers and if applied completely I think would necessarily diverge. Do you know any way of doing it otherwise than for the normal law ? The kind of thing it is applied to is the distribution of absolute magnitudes of stars.

Yours sincerely,

Harold Jeffreys