21 February 1944

My dear Kendall,

In drafting your chapter on Fiducial Inference, I see you have not seen the paper of mine published in 1941, of which I enclose a copy. I hope you will find time to read the first two sections and to survey the rather full tables which the paper provides of this test of significance.

One of these tables, together with Sukhatme's, has been published in the Second Edition of Statistical Tables (1943), and I believe the worked example in the introduction may help to the understanding of the logic of the method. In the absence of these two sources, I fear that much of what I have written to you by letter must have been comparatively unintelligible.

It is particularly distressing to me to find my methods equated (or nearly so), as you are inclined to do, with Jeffreys' logical approach, with which from first to last I believe no-one has had the least reason to associate it.

Both Jeffreys and myself have been careful to make the distinction sufficiently plain. In fact the argument from the probability integral in the paper on Inverse Probability (Cambridge Philosophical Society, 26, 528) to which I have referred before in correspondence would seem the better model for an expedition of my method than any based on a pseudo- *a priori* distribution which in my opinion bears no relation to the logical situation presented by statistical estimation. This argument from the probability integral is applicable in all single-variate cases and is of course the basis of the confidence-intervals which you ascribe to Neyman. I have no doubt that Pearson was deceived by Neyman's statements as to the origin of his method, as it appears that S.Kolodziejczyk was also deceived about the same time (see page 143).

As to the question of sampling distributions in repeated sampling from populations having a fixed variance-ratio, it seems to be most misleading to refer to this question as having been settled by something published by Neyman in 1941 , in view of

Section IV(p.374) of the note(Annals of Eugenics VII,pp.370-375) I wrote on first hearing of Bartlett's criticisms, giving the actual distributions of Bartlett's ratios. This was, I believe, the first occasion on which such actual distributions were given, and it should be quite clear that I recognised at this date the basis of such distributions as violating the conditions of the fiducial argument(p.373). It is misleading to represent these distributions as a subject of dispute only settled by this or that later calculation, when in fact what is disputed was only the relevance of these distributions to the question at issue.

About your examples: No.1 is one of the topics that I discussed in 1930. The general situation for univariate problems is that there is a common probability-integral P such that the distribution of r given ρ is $\frac{dP}{dr}dr$ and that the fiducial distribution of ρ given r is $-\frac{dP}{d\rho}d\rho$. This does not work out so simply as you make it in this example. Your solution would imply that P was some function of $\left(\frac{r}{\sqrt{1-r^2}} - \frac{\rho}{\sqrt{1-\rho^2}}\right)$. I remember illustrating this transformation in 1914, discarding it in favour of $z = \tanh^{-1} r$, for (as the figure in <u>Statistical Methods</u> shows), it is a good large-sample approximation to take P as a function of $(z - \zeta)$. It is not however exact for the inter-class case; for intra-class correlations however P is a function of $(z - \zeta)$ only, so that one can replace $\frac{dr}{1-r^2}$ by $\frac{d\rho}{1-\rho^2}$ in passing from the sampling distribution of r given ρ to the fiducial distribution of ρ given r.

In the other two examples to which you refer I am not quite clear as to the data. They seem however to be attempts to infer a fiducial distribution of a parameter from a discontinuous distribution of a variate. In such cases I believe one only gets statements of inequality, and not of equality, respecting the parameter, <u>e.g.</u> the probability-interval of the parameter lies between limits set by fiducial statements based on x-or-more observed and x-or-less observed, where x is the actual number observed.

Yours sincerely,