May 20, 1938

My dear Panse,

I am very glad to hear from you, and send you my best wishes for the scholarship.

I have looked up Miss Barnard's paper, and I think you refer to her use of the number 394 on p.361. This is the total number for variation within series, there being 398 skulls in all distributed among the four periods represented.

In view of my later work, Annals of Eugenics Vol vii, p p. 183, it appears that in testing significance with a discriminant function having four coefficients, i.e. with 3 adjustable ratios, 3 more degrees of freedom should be deducted from the total within series and added to the one representing regression on time. So that, in analysing the variance of φ we should ascribe 4 degrees of freedom to the regression and only 391 to the residual sum of squares within series. The correct handling of the test of significance was, however, obscure at the time of Miss Barnard's paper.

With respect to your reference to Vol vii, p. 182, the right hand sides should certainly check approximately with d/λ_1, and I am glad you found they did so in your own work. I have not recalculated my own to see what error there may be in them.

The question you raise on degrees of freedom affects, I think, the comparison *Malvi* and *Cutchica* if, as I think, you have calculated a different discriminant function for each of the pairs of varieties compared. With t = 3.612, the variance ratio, with four degrees of freedom, will be about 3.25, which will still give, I think, quite a significant value of z, showing that *Malvi* does differ from *Cutchica* significantly in leaf form, without using more than the three standard measurements, though it does not show that it differs from it in the same manner as *Bani* is distinguished from *Bengalensis*.

I think that when the sums of squares and products are strikingly different in the different populations, as is shown, for example, in the very small variance of *Cutchica* for sinus length, one cannot safely do more than compare pairs of equal samples from the different populations as you have done, unless, as with the Iris data, one has a definite hypothesis to consider, which supplies proper weights for pooling the dispersions observed within different samples. If the different samples, on the contrary, show similar dispersion, i.e., similar variances and covariances for all measurements, it will often be worth while to seek for a single discriminant appropriate for all comparisons.

The conditions will be more often realized when the measurements are relatively equal in the different samples than when the averages differ greatly. And relatively equal values may, none the less, give well-determined discriminant functions if the samples are large or the variances within samples small. Thus, if it were suspected that one variety had diffused into another, giving mixture in proportions varying as one passes across the country, one might, I think, seek for a single discriminant for grading samples taken from several different places, basing it on a single pooled dispersion matrix, and regression on spacial position in the same way as Miss Barnard used the approximate times of the Egyptian skull series.

In another case again a meteorological factor such as rainfall might be better than any coordinate of position.

I mention this because a discriminant function cannot have the full utility of an index unless it is rather widely applicable, and, though necessarily the technique of finding the best discriminant for one special purpose enables us to use a different discriminant for every comparison we have to make, yet there is no doubt that the most important results will be those in which a single discriminant, or a few, suffices to make the most important distinctions for a long series of forms.

Yours sincerely,