

September 19th, 1935

Dear Dr Deming,

I am very glad to see from your letter that you only wish to suggest that ^{when σ} is known the traditional theory of errors procedure (the u contours of your paper) ^{is} are appropriate. I got the impression from what you had written that you considered that there was ground for choosing u rather than t, other than the possession of definite knowledge of the value of σ .

If I remember right, 'Student' in putting forward his new test was perfectly clear that he regarded it as a correction of the test traditional up to his time, needed especially for small samples owing to our uncertainty of the true value to be ascribed to the variance of the population.

There is a good deal in the approach chosen by Neyman and Pearson that I disagree with, but, so far, ^{it seems.} they ~~seem~~ to have been led to nothing more

than the conclusion that the tests of significance which I and those who agree with me had previously put forward were the best possible for their purpose; in fact, to use their terminology, the \bar{u} regions are uniformly the best possible in relation to one class of alternative hypotheses, the population variance being given, while the t contours are uniformly the best possible for another class of alternative hypotheses, the variance being unknown. It is, however, in my opinion, a pity that these writers have introduced the concept of "errors of the second kind", i.e. of accepting an hypothesis when it is false, seeing that, until the true hypothesis is specified, such errors are undefined both in magnitude and in frequency. Their phraseology also encourages the very troublesome fallacy that when \bar{u} deviation is not significant the hypothesis tested should be accepted as true.

Yours sincerely,