20th. April 1948.

Dear Mr, Misra,

Thank you for your note on Neyman's theory of two
kinds of error.   My own impression had always been that this
whole line of exposition had a good deal of fudge about it.
In the great majority of research situations there is, in fact,
only one well defined hypothesis to be considered capable of
yielding a test of significance, and in such a test naturally
we have the means of fixing a frequency with which this hypothesis
would be rejected should it be true, but no means whatever of
discussing how frequently it would be ~~expected~~ accepted or rejected should
it be false;   for it may be false in innumerable different ways,
which have not been analysed or discussed.   What I call fudge
is to pretend that in general the null hypothesis of a test of
significance is known to be one involving one or more variable
parameters, to discuss this case as though it were the only one
that arose and in discussing it to take over the results of much
earlier discussions on the theory of estimation, which, of course,
are applicable in such a case.

Note, for example, that the only criterion of any use
developed in all this work of Neyman and his followers, namely
the so called power function, is in reality a particular case of

what in 1920 I called the Likelihood, which necessarily, on principles
established in the theory of estimation, supplies all that is
needed in discussing the efficacy of tests of significance in
problems to which the theory of estimation is applicable.

Your note interests me a great deal, as I have been sorry
to see people at the Statistical Laboratory as intelligent as
Bose and Roy seemingly accepting Neyman's work at its face value.

Yours sincerely,