25 September 1934.

W.E.Deming, Esq.,
Bureau of Chemistry and Soils,
U.S.Department of Agriculture,
Washington. U.S.A.

Dear Mr. Deming,

Many thanks for sending me your paper with R.T.Birge from "Reviews of Modern Physics". I think the paper will be found most valuable. It is, I believe, the first attempt to give to physicists, or even to astronomers, a comprehensive account of the ways in which quite modern work have extended and revolutionised the classical theory of errors. You ask me for criticisms, but really I have found very little in substance to criticise. I think the discussion on page 135 is somewhat hard on "Student's" z test (By the way, since 1925,"Student" has adopted the transformation I suggested, $t = z\sqrt{n}$, so that he uses the t test as much as I do). I would not myself admit that "Student's" test is ever misleading , and it can only be called hazardous in the strict and nonpolemical sense that it lays down and accepts a certain definite hazard. It is the u test which requires guesswork and is, therefore, exposed to objection by those who want their inferences to flow from the data only.

As I expect you know, up to well within the last 15 years writers on statistics were accustomed to be extremely careless in confusing that which is estimated with our estimate of it. The same terms and the same symbols were used for both without distinction. In 1921, in a paper of the Phil. Trans., aimed at clarifying some of the contradictions and paradoxes of the subject, I introduced two new terms, intended to be antithetical, namely, "parameter", used to specify the parent population, and "statistic", calculated from the observed sample. I was quite deliberate in choosing unlike words for these ideas which it was important to distinguish as clearly as possible. That work has now been largely done , so far as concerns the better writers, on the subject, and certainly there is no confusion in your paper, where I think you systematically use Greek and Latin letters to distinguish these two classes of quantity; but, perhaps by a slip, you do (Section 3e, line 7) use the expression "corresponding parameter of a sample", which on consideration you may agree is rather a dangerous one for some classes of student. A population is completely specified by its one or two or more parameters. A sample of n would need n different statistics if these were to be used to specify it. They are, in fact not used for this purpose at all, but essentially for estimation.

To each statistic there corresponds a particular parameter or parametric function $\cancel{x}$ to which the value of the statistic tends, as the sample is increased indefinitely, but to each parameter there "corresponds" in this sense as many different statistics as a cat can have kittens. In fact there is no 1:1 correspondence as suggested by your clause and I am sure it is better not to use the word parameter for one of the fluctuating quantities obtained from samples, which one may call statistical estimates, or something of the kind if that is preferred to the word statistics.

I may say in this connection, that I think your exposition in Section 3e of the fiducially related values of σ and s is altogether excellent, the only thing I should add on the logical side is that the statements of fiducial probability obtained should only be taken from distributions, such as s for given σ, where the problem of estimation of the parameters has been completely and therefore uniquely solved ~~sampled~~, i.e. where s is known to contain the whole of the information contained by the sample. One can see the necessity for this stipulation by considering what would happen if, like~~ly~~ the astronomers used an estimate of σ based on the mean error, rather than on the mean square error. If s_1 is their estimate, then the distribution clearly will be a function of $\frac{s_1}{\sigma}$ only and there is nothing but hard work to prevent a misguided astronomer from

[We] and

tabulating the percentile points of the distribution for different sizes of sample. Then, given s_1, it would be possible, apparently, to state the fiducial 5 per cent and 95 per cent points for sigma and these would not, of course, agree exactly ~~from~~ with the values derived by the mean square method from the same sample. The use of fiducial probability in this precipitate way would in fact, have le~~a~~d to a definite numerical contradiction, of a kind not unlike those which brought discredit on the use of inverse probability, based on some form of doctrine of insufficient reason.

In the light of the theory of estimation the logical contradiction is easily resolved. An inductive statement (unlike a deductive one) is only true if it is the whole truth; suppression of part of the data and the treatment of the remainder as though it were the whole, although these data are really true and the method of treatment unexceptional if applied to the whole, will, as all statisticians know, lead to very false results. What the theory of estimation is capable of showing is that a definite portion of the information supplied by the sample is omitted or thrown away in using an estimate based on the mean error, but that the whole is retained or conserved in any estimate based on the mean square error. Consequently when faced with such contradictory statements, apparently equally well founded on the fiducial argument we can, with the theory of estimation behind us say that one statement is true and the other is false and why. It is for this reason that I think it worth while to emphasise that the theory of fiducial probability is only an outgrowth or branch of the theory of estimation and that the attempt which Neyman and Pearson have made to make it stand alone without regard to the quantity of information utilised is bound to lead to contradictions and confusion.

Again let me congratulate you most heartily on the completion of a very fine enterprise.

Yours sincerely,