in French translation; in particular you wished to show me the curious axiom which Kolmogoroff had excogitated in wrestling with the problem of fiducial inference.

As I am now engaged in setting out more fully, from the point of view of mathematical logic, the bases, as I understand them, of scientific inference, I am wanting to recover the reference of this little essay in axiomatics.

For a time, as perhaps you know, I was doubtful whether Sir Harold Jeffreys and others were not perhaps right in thinking that the form of reasoning, to which I gave the name 'fiducial', required some special axiom, but I am now fully satisfied that this is not so, but that the matter turns on the fact that the word 'probability' was framed by our predecessors in the 17th and 18th centuries not only with abstract and deductive inferences in view, but with the intention of actually applying the idea to the real world, for example, in the advice given to gamblers, and that in consequence the true meaning of the word includes both the specification of what is known, which enters readily into deductive processes, but also a specific requirement as to what is unknown, which is a type of datum we constantly have to use in inductive reasoning, but which is not easily accommodated to the canons of deduction.

All this confirms me in my belief that what is inferred, using the method of fiducial probability, is a classical probability just as conceived by de Moivre or Montmort, and not in any sense a special kind, or species, of probability, as has been diligently insinuated. In fact there are a great many cases in which fiducial inferences could be experimentally verified to any degree of accuracy.

It is in trying to make it clear that I am myself introducing no new axiom that I want to refer to the attempts of Jeffreys and Kolmogoroff to cope with the problem in this way. I hope you will recall the reference without trouble.

Fisher to W.E. Deming: 25 September 1934

Many thanks for sending me your paper with R.T. Birge from *Reviews of Modern Physics*.[1] 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 has 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.* [*CP* 18], 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 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, 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 the astronomers, we used an estimate of σ based on the mean error, rather than on the mean square error. If s_1 is the estimate, then the distribution clearly will be a function of s_1/σ only and there is nothing but hard work to prevent a misguided astronomer from 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 σ and these would not, of course, agree exactly 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 led to a definite numerical contradiction,