13 April 1934.

Dear Jeffreys,

Thanks for your letter of April 10th. You have not got hold of my botanical problem yet, for it has nothing to do with mutations or mutation rates, but solely with the proportions in which different types which can be recognised by experimental breedingas distinct, exist in a wild population, i.e. for any two plants the experimenter can, in time, ascertain whether they are alike or not alike, and so can find that he has 6 different sorts in the 10 plants tested. But he knows nothing about which is the mutant type and which the parent type.

The information which I supposed him to have, that the frequencies of different types in nature were in geometric progression, might have been different without altering the logical form of the problem, e.g. an alternative problem might have been to interpret the same data on the supposition that there was in nature a finite, but unknown number of types, all equally frequent. One would then infer the likelihood of any proposed number as great as or greater than thenumber so far fuond and so thelikelihood of each of the possible values of the probability of getting a new type at the next trial. Either item of supplementary information leads to essentially the same logical

situation though with a different mathematical content.

I like Bayes' approach to the notion of probability, because it is historically true that expectations, as of cargoes at sea or contingent rights in property must have been bought and sold long before the value placed upon them was formerly analysed into the two components, probability of possession and value if possessed. Indeed it is curious, though I believe it is a fact, that this analysis was not carried out by the Greek or Arabian mathematicians or indeed by anyone until the sixteenth century, if then. It seems to me the basis for what we may call the frequency aspect of the probability theory.

The tram problem is a good one. If instead of a whole number, you have a continuous variate, e.g. if the traveller arrives by parachute near a stone marked "City of X", 1 km from city centre", he has quite a nice basis for estimating the radius of the city supposing, of course, that he knows that all cities are circular and that distances are marked from their geometrical centres. He then has an unambiguous fiducial argument as follows: "If the radius of the city exceeds r kms., the probability of falling on or within the 1 km. circle is $\frac{1}{R^2}$. If this event has happened the fiducial probability that the radius exceeds $\overset{R}{5}$ kilometres is therefore $\frac{1}{R^2}$ and the fiducial probability of it lying in the range dR is $\frac{2dR}{R^3}$. The fiducial median city has a radius $\sqrt{2}$ kilometres and an area 2 . The fiducial mean radius is 2 km. and

the facidual mean area id infinite. The most likely for the city however, is 1 km., for this maximised the probability of his observation.

I don't think this quite applies to the tram-cars. If No. A has been observed one can argue that if n is the total number the probability of meeting with number A or less is $\frac{A}{n}$ and if n exceeds any value number this probability is less than $\frac{A}{N_0}$. But one clearly cannot differentiate at first sight I do not see how to handle the inequalities. In fact I fancy that the reason why the fiducial type of argument was for so long unrecognised is just that the earlier writers always took frequencies, that is discontinuous observations, rather than measurements as the typical data in the theory of probabilities. Continuous variates with their standard deviations and regression coefficients are, in this matter, susceptible to a much simpler rigourous treatment.

Yours sincerely,