My dear E. E.,

It was nice to see your letter of March 16th.

There were a sprinkling of new things in my book on scientific inference on which I should certainly like your opinion. Section 4 of Chapter II, on "The meaning of probability", is, I think, of some prominent importance as stressing an aspect of the meaning of probability which seems to have been overlooked by the leading twentieth century writers, such as Kolmogorov and Feller. The statement of the fiducial argument in Section 3 of Chapter III is certainly more complete than anything I have done before, and I believe clears up the slight confusions left by Kolmogorov's and Jeffreys' discussions of the subject. As a mathematical beispiel, the asymptotic application of the fiducial argument to discontinuous data has, I believe, not been before attempted, and leads to some interesting comparisons. I hope to get the misprints in this analysis corrected for the second edition, which has already been asked for. However, from you I would particularly want to get the reader's reaction to the forewords that I have inserted before some of the chapters, such as that of
Chapter V, pages 106-110, on axiomatics and Gödel's theorem, which may well amuse you and perhaps help others. The example on page 169 gives the correct fiducial inference for a case which has been mauled over a good deal, I think by one of the Princeton men, for there has been quite a cult, led I fancy by Savage of Chicago and Tukey from Princeton, for exhibiting garbled versions of the fiducial argument without any reference to the strict application of that argument itself.

On your point about the 2x2 table, when making an exact calculation I always use the single tail, and if I want to compare significance with cases where both tails are used, I simply double the value obtained, without regard to the question of how lumpy the other tail may be. Usually, indeed, I think that the single tail is appropriate, though of course not always. For example if, being new to the subject, I test 500 students with phenyl-thiocarbamide and find there are more non-tasters among the men than among the women, I should only be sure there was sex difference if on doubling the probability the result was still small enough to satisfy me, but, having heard that Z̄, Ẑ, and Z had found fewer non-tasters among women than among men, I should be satisfied that my population was telling the same story as theirs if the probability for the single tail was satisfactorily small; but in the first case I would not feel at all concerned
with the discontinuities at the tail other than that which had
been observed.

No, I do not discuss this in the new book, where in Chapter
IV I am principally concerned to get across the important point
that it is frequencies within the least recognizable subset of
hypothetical possibilities that are relevant to scientific infer-
ence. The 2x2 table gives a fairly good preliminary example of
this, though in such a discussion I suppose I ought to emphasize
that if, without meaning to perform an experiment, three rabbits
die unexpectedly, and on enquiry I find that a mistake has prob-
bly been made leading to their food being poisoned, I believe any
rational man would think it probable that the cause of these
unexpected deaths had been revealed, even if the particular
poison suspected had not previously been shown to be lethal to
rabbits. As a recommender of scientific procedure, I should say
that at that point it would be reasonable to perform a definitive
and well-controlled experiment capable of verifying this infer-
ence, that in such an experiment there would be an adequate num-
ber of untreated animals, and verification would only be claimed
if there was a significant contrast between the reactions of the
treated and the untreated, and that contrast in the expected
direction. In fact I would distinguish for this purpose between
being sure, and being in a position to claim experimental demon-
stration.

Sincerely yours,