My dear Harrod,

I was most interested to have your letter on H.T.J. (have
I got the initials right?) Norton.   I am writing at once in respect
of that which can be immediately verified, but I expect to be
able to write more fully later.

I had never heard of Norton until I saw your mention of
him in connection with the Bloomsbury set.   (By the way, I
found that whole chapter most instructive and entertaining.)
I certainly thought I had seen everything published in this
country on Mathematical and Statistical Genetics in the first
twenty-five years of the century.   I do not think I have ever
seen Norton's work even referred to.   I have just now verified
that there is nothing of his in Punnett's large off-rint col-
lection which passed to my department on his retirement, nor
in my own collection.   The absince from Punnett's is, however,
much the more significant since he was Professor of Genetics
here from 1910 and at the height of his re utation for the
next ten or fifteen years.

George Owen, who is lecturer in Genetics here and a Fellow
of Trinity is going to hunt out Norton's record as Fellow , and
exhume, if possible, the Fellowship thesis referred to.

At the moment I do not think your new version could escape
criticism, for it gives the impression that Norton actually
made a material contribution to the advancectn of the subject.

What is possible, and the thesis may show, is that he could have made such a contribution, but in fact never made it for any of the many reasons which deter people from publication. It may be that you will have to challenge Haldane to produce evidence for the statement, which seems to me extraordinary, that "he supplied rigorous proofs for many fundamental theorems."

Yours sincerely,

P.S. This Thesis is not to be found in Trinity. Norton was fellow for five years, 1910-15.

On another point (this is addressed to you rather than to your book) I wonder if you have seen how completely the old problem of inductive inference is transformed if one considers not the abstract notion of probability in isolation, but induction arising and experimental designs as two aspects of the same whole — the acquisition of knowledge by experimentation (and with reservations, by observation.) If you look at my book on _The design of Experiments_ as far as the "lady with the teacups", you will see what I mean, and why the earlier types of argument (e.g. Jeffries), seem to be in the air.