THE JOHN INNES HORTICULTURAL INSTITUTION.

31 Mostyn Road,
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6th August, 1947.

Dear Lederberg,

Your letter of last month arrived just as I was setting off for a Conference of Plant Breeders. I have also been to another Conference on Growth and Differentiation since then, as well as having to give a great deal of time to my crops which are in full flower in July and August. So please excuse my delay in replying to your letter.

You seem to have entered well and truly into the mathematics of crossing-over and its measurement. There was a great deal done with it some ten or fifteen years ago; and, while your approach is a new one in many ways, you might find the older literature of some interest. I am sending a review of mine under separate cover. It will help you to trace any papers that you might wish to read.

As you will see from this review, I am aware that not all cross-overs are recovered as such, and in my original calculation of map-distances from your data, if I used the term "cross-over" I implied "recognisable cross-over". The estimates of map distance must of course be minimal for this reason. I did not think that greater accuracy (such as might be achieved by a priori adjustment for unrecognisable double cross-over) would, however, be worth while as the basic assumption of the calculation was that there was no interference between the three regions. This is itself, of course, a questionable assumption which would serve to minimise the extended value of x (total distance).

Your calculation adjusts for unrecognisable double crossing-over within each region by using the same assumption of no interference. These adjustments may well be too large, because, if we are to judge by higher organisms, interference over short distances is the rule rather than the exception. So, I wonder whether your more elaborate calculations are made worth while by such additional accuracy as they might achieve as compared with my simpler ones.

About the results from the 2 and 4 strand calculations; surely these must be identical if you assume that, in the 4 strand case, the strands crossing-over at any one chiasma are independent of those crossing-over at any other, i.e. in other words if you assume the absence of what we always called chromatid interference. I can see no escape from this conclusion myself, so that I would regard any discrepancy between your 2 and 4 strand estimates as suspicious, rather than any agreement as accidental.

I was interested in your algebra of 4 strand crossing-over. So far as I am aware it is quite unique, and would be of value to anyone doing such analyses. Can it be made to take care of chromatid interference? I expect it can.

One further point, on page 3, line 4, of your letter, you refer to the enumeration of zygotes. Are your individuals zygotes? I thought you assumed (as I certainly have in our discussions) that meiosis (or its equivalent) followed fusion so that single products, the equivalents of gametes, were recovered for observation. If so, your "approximation" on page 3 is already contained in the initial assumptions.

May I say how impressed I was with the numbers of observations you have made on recombination between BM, Lac, V, and TL. Such data give a really worth while basis for calculation.

All good wishes to Mrs. Lederberg and yourself.

Yours sincerely,

K. Mather.

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