THE JOHN INNES HORTICULTURAL INSTITUTION.

31, Mostyn Road,
Merton Park,
London, S.W.19,
England.

11th March, 1947.

Dear Lederberg,

I must apologise for my delay in replying to your last two letters. I am a bad correspondent, and you will grow accustomed to such apologies from me.

First of all, one or two points from your letter of 8th February. You are, of course, quite right (p.2. para.3) that on the earlier evidence V is linked with L &/or P rather than with L & P. It looks now as if "&" is correct and not "or", but it was not clear at that time.

How do you propose to test 2 or 4 strand crossing-over? I thought it could be done only where more than one strand can be recovered from the same meiotic configuration, and I was not aware of any possibility of doing this in your material.

You say you think the possibility of spurious linkages like B.......M has been covered. Are the data final on this point, as it seems to me of basic importance?

The experiment you outlined in your first letter and whose results you gave in the later one of March 3rd is very valuable. It seems to place V between P & L as you say, but I am not clear about its implications for the left-end of the map. This is probably due to my incomplete appreciation of the experiment, for I am not clear about one or two technical points. Did M enter into it? I thought not at first, but then you refer to crossing-over in region F (between B & M) in your discussion of the results from the B data. In using B plates to estimate the ratio of B- and B+, do you insist on the bacteria being B_1+? And in estimating the B_1- : B_1+ ratio do you insist on them being B+? Prototrophs, I take it, are B_1+ B+ P+ L+ T+. I ask about these points as I cannot yet see clearly why B- was less frequent than B+.

Your idea of using cross-over suppressors is an interesting one, but I think that the data from them may still not be final.

as I see no theoretical reason why a two demensional system should not also have such suppressors. I feel myself that tests of the kinds listed as (a) and (b) in my last letter should be capable of settling the linearity question as clearly as anything else, if you have the genes for them.

The second edition of my "Statistical Analysis in Biology" is now available, I'm glad to say.

I hope that you will find time to continue letting me know how your experiments are getting along. I find them intensely interesting, from both the analytical and theoretical points of view. I have told Darlington about them and he too is very interested. He suggests that you may care to write something about them for the new Journal "Heredity" which he and Fisher have founded, and the first number of which should be out either this month or next.

Please give my best wishes to Mrs. Lederberg.

Yours sincerely,

Dr. J. Lederberg,
Yale University,
Osborn Botanical Laboratory,
New Haven,
Connecticut,
U.S.A.