

25th September, 1952.

My dear Isaac,

I am reminded that I have never answered your letter of August 20th. As to the Escherichia, I expect you have heard from Cynthia at Achinota reporting progress on what I think is a most important point, namely the utilization of prototrophs for further genetic crosses.

I have been wondering what could be suitable for the Genetical or the Biometria Congress next year as a contribution from this department. [At Stockholm, you may remember, the organising committee chose tetrasomic linkage from the topics I offered then, and though I am afraid my short paper was not very intelligible to the meeting, its conclusions have been confirmed in some detail in later work. However, there is as yet scarcely more than confirmation as it is only in this summer, 1952, that I have the first backcross progenies from bi-duplex plants, and the second backcrosses will only just be available before the congress at Bellagio.]

In the States this year, I found my method of treating inbreeding problems by the direct use of the generation matrix regarded as highly controversial by the small, but fairly influential group of students of Sewall Wright in the Middle West, so that is a second topic that might be interesting.

I have slowly, but I think surely been establishing a situation in the thirteenth linkage group of the house-mouse, which you may recall I established by finding linkage between leaden and polydactyly. The possibility of clearing up the polydactyl situation which everyone else had found intractable, depended on Mrs Holt's work, and the situation has been complicated by Ordmeberg having been allowed to supersede her in her mouse work at the Galton Laboratory and by his steadily ignoring all that she had done ^{for instance} in the second edition of his book on the Genetics of the House Mouse. This I take to be mere jealousy, for she knows a deal more about mouse genetics than he does. The other factors, leaden and Splotch which Snell and his associates found to be linked with leaden are coming on well in my material though owing to the extreme modifiability of Splotch on different backgrounds, I have not yet any considerable mass of unquestionably good four-point backcrosses.

George Owen is away and I would encourage him to submit something experimental or theoretical to one or other of these conferences. It might be, also, that ^{Mr Wallace or} Mrs Pye would like to report some further points of interest cleared up in plant genetics, or in mice.

I am sending you these ideas for your reaction, but please send my letter to anyone else, e.g. Buzzati Traverso, if