

10 December 1931.

Professor J.B.S. Haldane,
John Innes Horticultural Institution,
Mostyn Road,
Merton Park,
London, S.W. 19.

My dear Haldane:

I think the iterative process for combining linkage values from different sorts of crosses would work all right, though I should be inclined myself to throw the whole data together in a single likelihood, in the kind of way I illustrate with Tedin's data in the ninth chapter of my book. I would do this if you cared to send the stuff along.

I have verified your maximum likelihood solution for Hogben's problem but get a different sign for the term in R , in the expression for the amount of information. I am glad you have taken this problem up as it looked as though Hogben's formula gave very wrong weights to families of different sizes. However the whole theory is seriously complicated by the increased frequency with which families with a large number of defectives get into the records, and I was not surprised at the frequency with which Hogben's examples differed significantly from Mendelian expectation. Weinberg

has written a good deal on this point.

I naturally find your dominance paper rather thrilling but I doubt if I can say usefully very much about it. Reading it through I believe you could make it a good deal more concrete to others as well as myself if you would give quite a lot of space to explaining and exemplifying such things as 'acceleration', and exactly what observable facts it is intended to cover. In fact there must be a whole body of paleontological or embryological lore, which I know nothing about, though it may be immensely important, and I feel a little nervous about telling the paleontologist or the embryologist that its all made clear by this nice new dominance theory in my present state of blank ignorance as to what there really is to make clear. Don't think that I don't see that you've made some excellent points, I think you have, but want a chance of appreciating them, that is of forming my own judgment about them, and if you could give us about as much as the rest of your paper on the complex suture lines of the genus on page 3, which I can't read, it wouldn't be taking too much space on a side issue.

In things like the kidney I suppose the previously current explanation was that a kidney might get diseased so there might as well be a certain amount of spare tissue in such an emergency. Your suggestion here, if I had it right, is that it must often be more or less congenitally defective

and needs the spare for these cases. I think this is a very strong point as it applies to all ages, and organic disease other than that due to congenital defects may not come early enough in life to affect reproduction.

I think we shall be talking on the same morning on the Ithaca programme and I am inclined to expatiate on some aspects of dominance theory which, judging from this summer, the Americans are rather keen to hear about. I should, of course, be delighted if you took up the theme on the same occasion, and if so we ought to consider a little and discuss in advance how much to attempt to cover in the time.

Yours sincerely,