

5 July 1934.

Dear Watkins,

Gosset has sent me copies of the last two letters you have interchanged, but I cannot grasp properly what the argument is about. It might be worth noting that with ten chromosomes and a generally loose linkage, as shown in maize, a pair of loci taken at random have a chance of only about one in fifty of being closely linked. Consequently, if the number of factors considered exceeds 50, which I have no doubt is the case, modifications due to linkage might begin to be important, but will certainly be unimportant for smaller numbers of factors. If this is so, then if your object is to show that Winter's results can be explained with only a few factors, then linkage will not help you, unless, of course, you are willing to start with an inherently improbable assumption at the basis of the argument.

In general its effect would be something like this: - when two genes are positively linked, i.e. in the initial material, both of those which make for higher oil content are in the same chromosome though there will be a decrease in variance due to crossing over at some stage during the experiment, but the chromosomes selected will be either non-crossovers or others

like them due to a second recombination and in Gosset's calculation such a set of two or more factors will be counted as one only. If on the other hand the genes are initially negatively linked there will be an increase in variance during that part of the experiment if there is one, in which double heterozygotes are common. Presumably, however, in every case genes which occur linked with one allelomorph in some of the foundation plants will also occur linked with its opposite number in others and you will have a mixture of the two effects.

This is only a rough outline and it would be an excellent thing if you could work out a theory of the experiment involving reasonable suppositions as to linkage.

Yours sincerely,

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