

January 23, 1950

Mr. Gordon Allen,  
155 Corona Avenue,  
Pelham 65, N. Y.

Dear Gordon:

I am glad to hear that your optimism in pursuing your very ingenious dual selection method may be justified by some promising results. Do you still want me to reisolate a  $Vl^F Vl^A$  double mutant, in view of the possible difficulties in using phage after allowing for phenotypic lag? If so, is there any special stock that you would like this in?

I hope you were not entirely serious in demanding an explanation of the Mal segregation data! I know less, if anything, about it than you do. In fact I am indebted to you for the semantic clarification (or obfuscation?) of the expression "two stage reduction", because this may well be just what is happening. If the data are upheld, they might imply that the duplex prototrophs are the converse of the persistent heterozygotes: that is to say, in the former, reduction for Mal will have been preceded by reduction for the other factors, and there will have been a stage like  $Mal^+/Mal^-; Lac^- Vl^F/---$ . In the persistent diploids, the evidence is very clear that we have a situation like  $Mal^+/---$ ;  $Lac^+ Vl^S/Lac^- Vl^F$ . This is not an explanation, but a generalization that may help in planning further experiments. The fact is that two reductions have occurred between parents and reduced segregants from persistent diploids, and this may help considerably. Your suggestion of "somatic meiosis" is perhaps not so far fetched in view of Whittinghill's oogonal crossing-over in *Drosophila*.

The only more orthodox interpretation that I might have to offer for the Mal segregation is just that of powerful coincidence of crossing over: i.e., that a chiasma in the region necessary to give a  $Mal^+$  prototrophic strand might direct two other chiasmata in the same proximity, so that ~~with~~ the  $Mal^+$  and  $Mal^-$  would be concordant for Lac, etc. This is not nearly so elegant a notion.

You don't have to bother to point out the foolishness of these schemes, but I've reached the point where I'm willing to try any working hypothesis that may suggest some meaningful, and feasible, experiments.

Zelle and I have completed another set of single cell pedigree analyses, and it is very clear that segregants are split off one at a time, rather than in pairs or quartets. However, the nuclear cytology is sufficiently complex as to allow of this complication very readily, merely as a matter of nuclear segregation after meiosis. Therefore, this is not critical evidence for the number of viable products of meiosis.

Sincerely