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

Dear Bordon:

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 Vl2 Vla- 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+/—.
In the persistent diploids, the evidence is very clear that we have a situa-
tion like Mal+/—; Lac+ Vla/Lac- Vla. 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 Shittinghill's
meiotic 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 both 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.

Zalle 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