

July 22, 1955

Dear Tom:

Thank you for your letter, and the data on the cultures. The Gal₇ story seems now pretty clear cut— these modifiers have been creeping in almost everywhere and have to be looked out for very carefully. Esther has some definite Gal₇ hemizygotes from another cross.

Jacob and Wellman sent us the mss. on the "partial transmission" about a week ago. Our comment was the same as yours, even to "coitus interruptus!" Their own data seem to point pretty definitely to an attribution of the altered segregation frequencies as a direct result of the treatment— the major class of recombinants levels off, and it is the incidence of Gal⁺, etc., among these that continues to rise. If he is dealing with one linkage group, as he implies, then one end of each chromosome must already have entered early, and would do so in entirety (late blending) if it were left undisturbed. This would be particularly interesting if one could be sure that this was partial fertilization. Unfortunately, one still cannot say from these experiments whether the genetic promiscuous has been broken in transit, or whether its pairing properties have been altered so as to change the crossover pattern. A reduction of specific pairing (a la Cavalli & Jinks) would have the same effect of accentuating the bias in favor of the P- parent. Someone ought to repeat the blending technique in connection with diploids to try to isolate the aneuploids that would clinch the partial-fertilization theory. Alternatively that technique might be applicable to higher organisms too.

In re Demerec linear segments, I have not yet seen enough data to be absolutely convinced of it, and am somewhat suspicious of the shortcuts that they have been using to score types (i.e. by differences in count on various media, rather than specific characterization of individual isolates). It is hard to see how they could be off on the fact of linkage (regardless of sequence). I had a structural notion too, not quite like yours: the anabolic enzymes are integrated onto a linear structure which corresponds to the chromosomal sequence. The mutants the Demerec looks at are not defects in the enzymes per se, but in the sites to which they normally attach. If he examines more mutants, he may find some unlinked that are seemingly functionally equivalent to some that are., unless there is enough polymerism in Salmonella to vitiate that search. At any rate, I'd want to ask whether very many tryptophanless, for example, have been isolated and all found to be part of a single segment.

We will be delighted to see you if you can drop by. Our house is still there but the grounds may be unrecognizable. Everyone around the lab. is well (except for the heat). Larry is vacationing briefly in Colorado. Gaylen and I have been isolating hyphal bits for clinching evidence on heterokaryosis in Streptomyces-- there is no doubt about it; so far nothing more interesting.

P.S. - The Genetics barn burned down this AM. *Sincerely,*

Tell Dave Skene if you see him. Except for some

*Just a note
Vadwale
(1955)*

valuable birds which make it a tragic loss, it would
have been almost welcome & was permitted long ago.
J.