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AUG 7 1955

Dear Esther and Josh

I apologise in answering so late your letter of July 16 but I was in Brittany for holidays and I just got your letter. Incidentally, I did not receive any letter from Esther last year and I suspect it to have arrived during a post strike in which a good deal of the mail was lost.

I am very happy to have your criticisms on our work although I can hardly agree with some of them. Until now we have only worked with Hfr strains and only with the T1 Hfr lac⁺ perhaps sequent. All the results we have now make a coherent picture for this sequent (and the OF⁺ sequent of the F⁺). It seems quite clear, from the data of "erotic induction" we have obtained with several inducible prophages, that, in a cross Hfr⁺ x F⁻ Hfr⁻, practically each time the inducible prophage enters the F⁻, it develops. It appears unambiguous therefore that only a sequent of the Hfr enters the F⁻ with high frequency. I do not know, for the true being, how to reconcile these results with your data from the hot separations.

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I would just like to make a few remarks.
The first one is that our data deal only with the TL- δ segment of the $\theta\phi$ - The mechanism we have advanced, Elie and I, in our last short paper is only concerned with such data and we have no hypothesis now to account for the "normal frequency" cases where markers from the S^2 -H region are transferred. My second ~~or~~ remark is that little is known, for the time being, on the true nature of het strains. It appears from Sennor's and my own experiments on transductions in K12 that transduced strains are obtained carrying two alleles ~~at~~ of a given marker, ^{and} which replicate "haploids" clones for more than 100 generations. What is the relation between the het obtained through crosses and such transduced strains, and even with double lysogenic strains carrying two λ prophages (which are obtained by superinfection, mixed infection, transduction and recombination), we do not know. It seems to me that a good understanding of all these cases of partial diploidy is necessary. We have not (Elie and I) tried to work with the het strain

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You sent to Jacques but Jacques has tried to do some experiments with it, without any success I believe.

To answer some of your remarks, we attribute a single linkage group $O \Pi \Delta \Pi \text{ Lac } \text{fal}_6 \text{ d}$ and our theory explains very easily the different fal_6 segregation ratios in ~~crosses~~ $H_f \text{ } \text{ly}^+ \times F^- \text{ } \text{ly}^-$ and other crosses. Each time, Δ comes into the F^- , the prophage develops and the zygote is destroyed (50 to 60% of the conjugations). Only when a breakage occurs between O and Δ , the zygote properly does not come in the F^- and the zygote is viable. Since fal_6 and Δ are closely linked, such breakage occurs generally before fal_6 and only the zygotes in which a Π or $\Pi \Delta$ or $\Pi \Delta \Pi \text{ Lac}$ segment has entered can give a recombinant - the fal_6 frequency is then increased by a 10 times factor with Δ .

Of course, our proposed scheme is only a tentative one and the earlier it is rejected by new data, the better it will be. This is by definition our job. The next

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efficient way for this is to keep in close contact and we would appreciate very much to get your HS (Thank you for the 3 short notes you sent me some weeks ago).

I thank you very much for your kind offer for me to come and work with you during some months. I appreciate greatly your invitation. Unfortunately, I am now the father of four children ranging from 6 to 1 year, and I am terrified by the thought of carrying such a squadron overseas. I hope to have one day a new opportunity of going to the States for some weeks and I would very much like to spend some time and talk with you. I keep an excellent "souvenir" of the too few hours I had in Paris with you. May I ask whether you will not come soon in Europe. Every-body here would greatly appreciate your visit.

Many thanks again and I hope a new theory will soon emerge from 1812 data.

Yours sincerely

Francis