

28/1/52

Dear Lederberg,

It has been rather surprising to read in your letter of Jan. 16 the result of an experiment which I had just planned, viz the transformation of F- into F+. I was convinced since a long time that a cytoplasmic inheritance should be liable to behave as infective on suitable conditions, and since the times of my correspondence with Hayes I had set down to look for transforming principles in the self-incompatibility story. However, filtrates (through Seitz) and heat killed suspensions of K-12 were inactive in inducing the transformation of TLB₁- into a self compatible strain. I had started the wrong end! I recently decided to try the action of intact cells, and set up an experiment which is exceedingly similar to one of yours, and designed according to your manage a trois: K 12 x TLB₁-S^F F-Lac+ x TLB₁+F+Lac-, incubated together for a time, then plated on EMS St Lac. Also the reverse was done: BM+Lac- x TLB₁-S^FF-Lac- x TLB₁+E+Lac+. Your letter came before this experiment was to an end. So, while there is no doubt on your priority, I can confirm your result ~~with~~ ^{to which I} think I would have come independently ~~with the same result~~. A slight addition is the fact that while it is unknown, in your case, whether it is BM-F- or TLB₁-F- which is susceptible to F+, there is no doubt, from the mentioned experiment, that the latter ~~is~~ is (very highly) susceptible. Our experiments have been so closely overlapping ^{or complementary} that I am glad to accept your proposal of a collaboration on the problem. ~~xxxx~~ On another point we have been doing independently similar experiments: i.e. the effect of aeration. I had noticed that cultures grown under aeration are much less fertile than deep grown cultures. I had thought, at first, that aeration might suppress gametogenesis, and struck by ~~the~~ ^{a possible} analogy with lysogenesis, I tried fluctuation tests for prototrophs to see if I could detect "bursts" or similar. The results were practically negative. Later I investigated whether fully grown aereated cultures might ~~not~~ be infertile, because they have reached a fuller saturation point than resting deep grown cultures. This was in agreement with the fact that taking saturated deep grown cultures, mixing them together and aereating, a good yield of prototrophs is obtained. To test the hypothesis further, I mixed aereated cultures in the logarithmic phase (1/20 of saturation or so) incubated them further under aeration, and thus obtained a very high yield of prototrophs. It seems therefore that it is not aeration, but some catabolite accumulating more easily at saturation in aerobic growth that suppresses F+. This hypothesis can be tested further. I should like to know if you agree to this. I should add that I use common broth, without glucose or buffer, and that by aeration I really mean "rolling" ~~after~~ according to Ryan's apparatus (see MGB 2).

The inheritance of F+ is most interesting. Would you not call or rather expect, a transforming principle involved in it? The most important point seems to me now that of finding conditions where the

