UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF GENETICS

PROFESSOR R. A. FISHER, Sc.D., F.R.S.
MISS M. F. I. SPEYER, B.Sc.
Research Assistant and Secretary

WHITTINGEHAME LODGE
44 STOREY'S WAY
CAMBRIDGE
Tel. 55822

Nov. 24th, 1949

Dear Lederberg,

Thank you very much for your letter of the 9th. I am sending you under separate cover 123 and a derivative, 123 Lac_3-$V^r_{1,5}$, which when crossed to 58-161 or 58-161 Hfr gives recombination of both characters. It is interesting that Nfr and Hfr crossed to 123 give different recombination values, though the absolute rate of recombination is apparently the same. Lac_3- crossed to W 1073 gives recombinants which are partly Lac+, showing that the Lac genes are not allelic. Of course I don't know whether this Lac_3 is allelic to Lac_3 of the K 12 series you have established. I am not sending biochemical mutants of 123 because I have not yet any reliable ones; I waited to answer you, hoping I might have had some by now, but unfortunately this work is still behind.

Crosses Hfr x Hfr with new biochemical mutants seem to give very poor yields, and Ar- x B_1- is the best-yielding of all of them (10^{-4}, 10^{-5}); perhaps I shall have to return on my statement about mating types !

The cross W 814 x W 1059 or W 1073 did of course succeed when I used ~~nutrient broth~~ penassay broth instead of nutrient broth; reasons of the discrepancy will, I hope, become clear when an experiment now being made will be finished. I have also tried the mating $S^- \times A^R$; it seems that with this method as well, Hfr gives better yields than Nfr (58-161) but the ratio is only about 100 times.

I have done some work with the salts, but so far no important results have come out. There are, of course, differences, and formulas favoring the formation of insoluble compounds like Mg-NH_4 phosphates seem to give poorer yields

I had a letter from Maaloe, to whom I am sending the nearest approximations to the strains he wants which are in my hands ; they should however be quite satisfactory for his purposes. As I am doing a similar work on Hfr, as he will be doing on Nfr, it will be interesting to compare our results.

It was most interesting to hear from you of the new coli strain showing recombination. Have you any idea of the denominator of the fraction of strains showing sexual reproduction, i.e. how many strains have been tested for mating behaviour ?I am doing again something on that line this year. I hope to have it soon, and see whether it mates with 123 . Incidentally, 123 is not so very different from K12. It was not tested for viruses T4-T6, because I have lost them ; samples of viruses I took from Milan in October were not reliable, and I shall write again to the States or to Latarjet to have those viruses. With T2 123 gave irregular results , but should be defined as sensitive . It has a peculiar difference from K12 , in size of colony; colonies of 123 are small on all media I have tried. It must be a polygenic character; however it gives a not too bad segregation when crossed to Hfr (no segregation when crossed to Nfr) . For sugars, I did not find differences , so far with K 12. I know nothing about the origin of 123 ; but I should write to the NCTC about it.

As to the question of radiation-induced lethals, I am most interested to hear that you will carry on that research ; I think it would be interesting to do it with X rays as well as with UV, they will probably give different results. I shall be grateful if you will let me know about some of your results, and of course I think it is better if this research will be carried on in yours rather than in my labora-

tory.

Did you try anything for recombination of lethals ? A start with Hfr was fully negative, but it may be that the new techniques of crossing in complete media, which you have developed, may give a different answer.

Concerning cytology : nothing done so far on my side. I hope you will be more successful with diploids.

Yours sincerely

Luigi Cavalli-

P.S. Your strains have just come in good condition.
thank you very much