

COMITATO ORDINATORE
PER IL IX CONGRESSO INTERNAZIONALE DI GENETICA

Segreteria Generale: Istituto di Genetica dell'Università
Via Celoria 10 - Milano (Italia)

Milan, 22/4/52

Dear Lederberg;

I must apologise for a long delay before answering; I hope the paper was sent to Genetics, anyhow, so that there has been no actual delay. I entirely agree with it and have no objections, except a few very unimportant ones. I should for instance suppress "W 1133" in the last-but-one line of page 3, since the crosses were actually carried out with two TLB₁-S^r strains which are, as far as their origin is concerned, unrelated to the strain you mention, and which is not in my possession. In the first lines of page 4, re Hfr, it seems that selection for resistance to nitrogen mustard in K 12 was carried out by Visconti and myself, while I actually cannot charge Visconti with the misdeed of having obtained Hfr and having left it resting for three years; the work with Visconti concerned coli B only. My work on Hfr was not published, except in the few lines on the 1950 paper, ~~xxx~~ or in the quotation of C. and H. paper on NCOC 123; but a paper was read about it at the 100th meeting of the Genetical Society of Great Britain at Cambridge, in 1949, carrying the title: "An actively mating strain in E. coli".

Re heading etc., I have written to Fisher, who will soon be back to Cambridge, whether he thinks I ought to have as heading of the paper Dept. of Genetics, Cambridge U. rather than Istituto Sieroterapico Milanese, mentioning ~~xxx~~ Milan in the footnote; I imagine such detail may easily be altered in proofs, and should prefer if possible to leave the question open *for* now.

I wonder if you think that an explanatory sentence in the footnote may be useful to help the puzzled reader, who knows that we have always been some thousand miles apart, by saying that ~~xxxxxx~~ the initial observations leading to the facts observed in this paper were independently ~~xxxxxx~~ made in Madison and Cambridge/Milan, and as they were either very close or complementary it was decided to continue the work in collaboration. Actually, I find this collaboration/^{an} extremely stimulating, and well worth advertising experiment in cooperative research.

The first half of this month has been very depressing, in view of several organizational tasks and a couple of congresses, plus some family accidents. I had to stop work, practically. I should be resuming it this week. I have only had time to control Hfr x TLB₁-S^r and got in the first experiment 20% Lac+S^r recombinants. The mixture was aerated for 1 and 1/2 hour. I have started controlling whether attenuated Hfr can transduce F+.

Actually it seems that streptomycin may help a lot towards observation of recombination with Hfr. If something turns

COMITATO ORGANIZAZIONE
PER IL IX CONGRESSO INTERNAZIONALE DI GENETICA

Giuseppe (Gennaro) Galvani
Via Cavour 10 - Milano (Italia)

out, I may have a good opportunity of making a film of it.

I had news that Hayes gave a talk about the whole story at the Oxford meeting and had great success. In his last letter he said he would prepare his paper for JGM end of this month. I have sent to him a copy of your typescript, and I hope he will make the best use of it.

I shall write soon about all the old Hfr data; I should not like to delay further this letter. I am sorry not to be in a position to help you much about the JGM paper. I may be in a better position when next papers will come. What about a sensational title like "A unique case of genetic infection for fertility in E. coli" for the JGM paper?

Yours sincerely

Carroll

Your strain was found to be fertile.