

# ISTITUTO SIEROTERAPICO MILANESE

"SERAFFINO BELFANTI"

ENTE MORALE AGGREGATO ALLA UNIVERSITÀ DI MILANO  
MILANO - VIA DARWIN, 20

TELEF. 30640-30677-31757-32823-33917

TELEGR.: SIEROTERAFICO - MILANO

U. P. I. C. MILANO N. 48709

Direzione Scientifica

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Dear Joshua, Your letter and strains just received. Thank you very much for them. W-2058 was sterile, but I don't think it will be worth making a special sending for it.

Your separation of the K-12 markers into two linkage groups corresponds to that by Jim Watson, except that he goes further and splits your M, S, Th chromosome into two further ones. I have written to him that although his scheme is formally useful for predicting which markers will go along with the selected markers of the F+ parent, I do not believe there is still evidence for prezygotic elimination. However, your finding of a diploid with elimination of a Mal-S segment from the F- parent might be explained by the assumption of partial reversal of polarity. I have a series of crosses with same markers and variously strong F+ and F- strains which seem to show the possibility of partial reversal of polarity in an F+ x F- cross. Unfortunately the data were collected at different times and one of the strains used, Fr, has other complications like low recombination rate which make the interpretation suspicious. There are other data which may be more useful to prove it. In ~~my~~ <sup>after some incubation</sup> ~~menage~~ a trois experiments, I mixed an F- with an F+, then added a second F- strain with selected markers adjusted so as to permit only ~~cross~~ ~~single~~ detection of recombinants between the two F- strains. This was done adding one, or the other F- to the F+, and in both cases the recombinants showed ~~the~~ almost the same pattern, one of the two F- always behaving as relatively F+ irrespectively of which was added first and thus could be more easily infected. I cannot, however, entirely discard the hypothesis that in these experiments, the ~~behaviour~~ <sup>behaviour</sup> ~~was~~ was due to difference in susceptibility to F+.

I hope to receive ~~the~~ soon your contribution to the joint paper on antibiotics. Chain ~~asked~~ <sup>asked</sup> write ~~two~~ <sup>two</sup> days ago asking with great hurry to have the ~~title~~ <sup>title</sup> of the paper, and saying that he did not know about your decisions; I have repeated to him that we shall be giving a joint paper ~~and~~ and have suggested the title: Genetics of resistance to bacterial inhibitors, giving my name as first according to ~~an~~ <sup>your</sup> earlier ~~agreement~~ <sup>agreement</sup> proposal. However, I presume this is only for the preliminary programme, and both order of names and titles ~~may~~ <sup>may</sup> be altered successively. I told we would send the manuscript, which he ~~thinks~~ <sup>says is</sup> urgent, before end of April, and hope to get soon your notes so that I can keep this promise. I asked to have copy of the proofs, to be cleared through me, to have some chance for corrections, but I am not sure whether this will be feasible.

One news that reached me also ~~two~~ <sup>two</sup> days ago, and which will interest you, is that I have been asked to act as Vice Presi-

dent of the Section of Genetics of Microorganisms at the Congress of Microbiology. Although this is a very late proposal, I have accepted because I think that something can still be done. ~~First~~ My tasks are a) to choose a President, b) to say which of the contributed papers are worth an extra time. The President shall be a well known person with a possibly important position. I understand a geneticist, like Buzzati, would be acceptable by the Congress organizers; on the other hand, I have been considering whether a distinguished microbiologist, like Prof. Tatum, who has had an active part in the development of the field, or someone who has an active part, like Pontecorvo, or Beadle (though I hear he is no more active now) might not be more suitable, in view of the suspicion with which genetics is viewed in some microbiological circles. As I have no personal knowledge of Professor Tatum or Beadle, I am writing to you if you can give me advice on this point, or suggest other names. As to ~~turning~~ asking longer papers or lectures, again you may have some advice to give. Of course, you will be the first to be asked to give a long paper; should an official invitation be useful, just let me know and I shall let you have it (hoping to get in the meanwhile letter paper with the heading of the Congress, which I have asked). Would you suggest other people from your Dept.

Yours

*Luca*

P.S. Re- your proposed experiment for testing diploids in respect of postzygotic elimination; have you seen a paper by Bonnier and Luning, on Hereditas, suggesting that in gynandromorphs of Drosophila, some centromeres are preferentially eliminated? ~~when~~ It might seem a parallel of post-zygotic elimination, although far less extreme. === I have not recovered a B-M-. I simply tested whether 58-161 x W 677 in the presence of biotin could give B- recombinants ~~which it did not~~ which it did not. But there was something odd in this experiment, in that a B- strain would not grow on the plate after crossing. I should retest and see what may be the cause of inhibition. Thank you very much for the Pyrex filter. If you are coming, and could carry the complete U-tube with you it would be best. Otherwise you may perhaps find someone willing to carry it to Europe.