ISTITUTO SIEROTERAPICO MILANESE

12.xi.52

Dear Joshua,

Your two letters have been received. Concerning the papers you are interested in, we have got here all the journals and it would be quite easy for me to let you have photocopies of them. As to the addresses of the people concerned, they are rather out of date now (except Zironi's address, which is ~~this pl~~: Istituto di Microbiologia, via Darwin 22, Milan ; the Institute of Microbiology of the University is in the same building, and I do some work for it, though my main job is at the Istituto Sieroterapico). Zironi is Professor of Microbiology in this University, and was formerly the director of the Sieroterapico). Ciantini died five or six years ago. I do not know Dianzani; I thought his address were Siena, but if he has moved to Genova I should like to have his new address. His experiments were very poorly controlled. I have had a look at the other papers you mention ; the one by Muromtsev is merely a summary of previous work. Muromtsev must be a Russian scientist, although his exact address is not given, and he refers to papers of Russian authors but does not give quotations for them. The other papers contain some experimental data on paraagglutination, with diverse results.

I have no important progress to report. F^r x Hfr gives all F-; whether half of these, as expected, are transducible to F+, shall be tested in the future. I have no experiments who can say whether F^r contains an F like agent. Concerning the 1:1 segregation of F+:F- in crosses with F+ x F^r, which seems to me the most peculiar feature of this experiment, I should be looking for other markers linked with F in this cross, if any can be found. From the last paper by Fredericq on Ann. Inst. Pasteur, which you may not yet have seen, it seems that colicine E-resistance may be independent from other markers and segregates almost 1:1. Unfortunately, I find it rather difficult to secure good resistants to colicines, especially to colicine E.

On the question of filterability of F, I may have to withdraw my previous tentative conclusions. Mixtures of F+ and F- in various proportions show that after some time, all the culture is F+ even with low initial proportions of F+; there are however, some curious oscillations in the first stages which I cannot explain for the moment.

I am anxious to hear about the kinetic experiments. Concerning Hfr I shall certainly respect my earlier agreement with you. Thank you very much for your invitation to the States. I hope to be able to make use of it. It could not be earlier than 1954, because

next year I shall be rather busy with congresses. Unfortunately, my present position will not allow me to spend more than a few months away; but it would certainly like to come even for a few months. I shall make enquiries about the possibility of getting a Fulbright for the journey.

Concerning your coming for the Congress : there will be no substantial financial support for any participant, from the side of the congress. but there will certainly be some contribution to living expenses for people invited to give lectures. The paper asked from you was planned to be in a general session on genetic mechanisms, with other papers by Pontecorvo and Luria, if the latter can come. I hope you will be able to manage, but you will have unfortunately to look for journey expenses locally. If you think you may be able to secure them as a representative of some Academy, University, etc., let me know and we shall have a letter of invitation sent to this University etc. Of course we cannot ask that a specific person be sent by this University, but the method has worked in a number of other occasions.

Please give my best regards to Clive (Spicer)

Yours

Luca