20.10.52

Dear Bill,

Thank you very much for your letter and manuscript, and pardon me for answering so late. I have been busy on a variety of duties, and was so pleased to be able to start experimental work again that I forgot ~~about~~ to write to you and everybody else. I am enclosing some pictures taken at Pallanza, a few of which were sent to all participants. Meanwhile I have had a letter from Joshua, who states : " I was delayed a few days in studying the "final" version of ~~our~~ JGM paper I must admit that it does read better now even than the American version. In order to avoid confusion, I think that all corrections in proof should be cleared through you. If some changes are possible, I would like to discuss the following". I am not giving details here of the corrections proposed by Joshua, but am preparing a list of them and of some additional ones which have come to my mind, so that I can send you a copy to be deposited with you and which might have to be used, in case I cannot receive or send back in time the copy of proofs which I hope to receive from you. You do not need to send a copy to Lederberg, as was earlier planned. For some of the corrections I shall need your help . I hope you have a copy of the manuscript in my hands, so that I can refer to it in the list ; the copy in Lederberg's hands has an entirely different numbering of pages and lines.

Meanwhile I also had a letter from Watson , suggesting a hypothesis and inviting you and me to join in a note about it. Watson 's hypothesis is rather nice, and you will undoubtedly have heard of it. It fits most of Newcombe's data; as to mine, it fits only the other way round, i.e. assuming that the eliminated chromosome -or, as I still believe most likely - piece of chromosome, is the other one. This may be a difference in the strains employed by Newcombe and myself and am therefore trying to put to a test this hypothesis.

A rather interesting new strain has turned up. It is an F- strain which I obtained three years ago, which crosses with F+ at a rate about 20x smaller than any other F- strain. I have not succeeded in trying to infect it with F+. When crossing to F-, half of the prototrophs are F+, half F- , but I do not yet know, except for two of them, if the F- prototrophs are uninfectable as the F- parent strain. The funny thing is that this 1:1 segregation was found on crossing to three indepenendt F+ auxotrophs, and no linkage with any tested marker was found, so that this uninfectable, or F-resistant condition, seems to be due to a locus on a chromosome inherited independently from the other markers. This locus for maintenance of F is not, of

course,unprecedented in microbial genetics; see for instance K,k and kappa in Paramecium, or even lambda and the locus linked to Gal, in K-12. However,at the present point there are other interpretations possible. If this theory were true, I would feel very embarrassed of having used the symbol F in the JGM paper; it would undoubtedly be preferable to use the hard symbol ♀ and reserve F,f for the locus. Would you see many objections to changing F into ♀ in proofs ? It ocurs in about twenty different occasions. I do not know which symbol you have used in your paper.I must also hear Joshua's reactions to it. I am sure the printer would charge for the alteration but I do not mind this point too much.

Thank you for sending the fly wheel toy motor,my children will be delighted about it. There is one further favour that I should like to ask you. I remember that in the earlier correspondence,you mentioned (I think to Lederberg) that a man called Lightbown had found a streptomycinase. I should be interested in it,not quite for research, but for routine purposes (making a medium for blood cultures,and sputum).Could you perhaps let me have the information necessary to get hold of the strain,and of the information necessary to produce the enzyme ?

I have no diea whether it will be possible to me to come to England next year. However, I trust I shall see you ,and I hope Nora, again for the Genetics and Microbiology congresses. Dates will be 24--31 August in Bellagio,and 6-12 Sept.Rome,respectively. In the interval there will be the Biometric Conference,where I have again some time to waste on organization,but you might like to spend the interval sightseeing.There are lots of places worth visting between Bellagio and Rome. Lederberg should come also.

With best greetings also from Pupa for Nora and you.Pupa is working hard at her examination in Physiology.

Yours ever