

ISTITUTO SIEROTERAPICO MILANESE

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Direzione Scientifica

Milano, 3/7/52

Dear Joshua,

I am enclosing the manuscript for JGM. Abundant margins were left for corrections, hoping that they will be enough, so as to avoid you the trouble of retyping. There probably is in the paper more stuff than we may want to send to the press; lengthy introductions were, however, designed to let microbiologists who are not genetically trained understand something of the whole. Several hyphens may pretty well be abolished. As to English form, you may be confronted with the alternative of translating the whole into American, which is, I am told, a language admitted for publication on English journals, or accept the present Anglo-Italian form; perhaps you will find a suitable compromise, so as not to irritate the editors. ^{for the latter alternative} There is no room left for your acknowledgments, but the present typescript is certainly not the final one, so you can just add them on a separate sheet and I shall have the whole retyped in order with your corrections and additions, hoping to let the Editor have the final typescript for the middle of the month. I have no acknowledgments to make, and Serafino Belfanti should better be added. Perhaps the sentence "(with which we concur)" related to Hayes's streptomycin resistant gametes may be omitted, as I have some not yet fully verified doubts about it. *Re the genetics paper*

Hayes's results puzzled me a great deal. I have recontrolled older data using a variety of collodion filters: those with 0.74 μ APD give turbid filtrates; lower APD gives sterile filtrates and negative results. You may prefer to attenuate statements about cell-to-cell infection, however.

Penso did not deserve a better reply, if he did not offer financial help for you to attend the symposium on actinomycetes. The Microbiology congress is out of my control at present, as well as out of control of reliable people. Invitations for papers to the Genetics Congress will be out in September, and the chance of your inclusion is about 90%.

I am enclosing also copy of a paper communicated to the Italian Microbiology congress in April. Any alterations that you may like me to carry on it should reach me as soon as possible. I had forgotten about sending it to you before - it was a matter of no great concern.

Re NTCC 122, it is a curious story. Weigle - whom I do not know personally, asked me 123 as lambda indicator. I was not satisfied about the culture of this strain which I had in my hands, as it

did not cross, at that time, with K-12, and suggested to him to write to NTCC and get it directly. Odd enough, NTCC sent strain 122, ~~which~~ apparently because they had lost 123, and 122 proved lambda susceptible in Weigle's hands. I asked him a subculture, which I found to be prototrophic, possibly crossable, at a very low rate, with K-12. The yield of ~~pro~~combinants was low enough to discourage further research on my side. The whole story seems absurd!

Our cytology is not progressing fast. My ambition would be that of having a film made out of the ~~xxxxxxxx~~ conjugation-copulation business, but Hfr and F- seem to conduct their love affairs far too secretly.

Thank you for your J. Bact abstract, and for the cultures sent on May 23, which arrived yesterday. They are being isolated, because customs officers have pushed their official curiosity to unscrewing all the bottles.

Summer is humid and hot here too; besides, we are experiencing an airborne contamination due to molds growing rapidly on minimal which spoils most experiments. We shall have to stop work and disinfect from top to bottom. Unfortunately I have no sterile boxes.

I shall be in Paris from the 20th ^{of July} to the 1st of August or later. The Institute remains my best summer address. What about your address?

Yours sincerely

Inca
Casali