10/26/52

Dear Luca:

Page proof has just come in for our paper in Genetics. As you see, it has now been advanced to 1952, vol. 37:720-730, and the proofs for the JGM should be corrected accordingly. As only one of the two copies has had to be returned, I can enclose the other. 200 reprints were ordered for you and should be shipped directly to you. The issue is expected in about a month.

There is very little additional to report. Hfr recombination appears to be about 100x as efficient under conditions of active growth as under any other that we have tried so far. The existing data are not yet comparable with the previous F+ x F- crosses; Nelson is working on this. It appears likely that a similar requirement for growth conditions is needed in the latter crosses, selecting for prototrophs. Nelson finds that when one does kinetic experiments , mixing the washed parents and then plating at various times that not only the background ("plate recombinants") but also the slope is increased in unwashed, as compared to thoroughly washed agar. The likely interpretation seems to me that mixed clumps are formed in the suspensions, and that growth conditions are subsequently heeded to allow the clumps to go on to zygote formation. It is impossible to prevent growth altogether, owing to syntrophism. This may mean that the Maccacaro-Booth phenomenon is less trivial than appears at first sight: that is that residual growth does more than increase the probability of interpenetration of microcolonies. You mentioned something about agglutination by lysozyme-- could you tell me more about this, as it might be useful in the same connection.

With Salmonella, I have been looking into the genetic mechanism of phase variation. It looks very much as if alternative phases are both represented in any one strain by factors at two loci, but that one or the other is somehow inactivated. This inactivation is not only for its phenotypic expression, but for its transmission by FA. I speak of inactivation, rather than loss as the cell "remembers" its original second phase when the first phase has been substituted by transduction. As a by-product, many new Salmonella types and some old ones have been reconstituted. The story may bear some relationship to heritable Dauermodifications or somatic differentiation, but there is no indication that it has a cytoplasmic basis: i.e., the "inactivator" is associated with the locus in transduction.

Thank you for the reprints of the Scientia and W.H.O. papers. The latter was admirable writing (and experiment). I have also seen the full chloromycetin ms. (via Szybalski). Would the gradient plate technique not have been useful in scoring progeny? The Pallanza report also arrived (but only a week ago, having been long awaited). Would you like to give me explicit directions as to its further circulation? We can copy the few pages we would like to have.

Barigozzi's formal invitation just arrived-- again sans mention of financial help. I shall reply as if I were attending (which may or may not be). Thank you for any of your own efforts on my behalf.

Spicer is now busy working here for a few months on a WHO fellowship. We are enjoying his visit very much, as we did that of his predecessor (Bruce Stocker). Like Stocker, ~~Erxxxxx~~ Spicer is working with me on Salmonella.. I realize that your present improved position may make it less likely, but I hope that you will not forget altogether if an appropriate occasion should arise to think about enjoying our hospitality yourself. Given enough time, I think we could make a very substantial contribution to your living expenses [which wouldn unfortunately, I depend on your securing a regular visa](perhaps $300-400 per month, which is not, I realize, munificent-- but still not uncomfortable even for a family). Please regard this [redacted] only as a hopeful suggestion to create the means for a closer collaboration.

Are you acquainted with Dianzini (Genova) and his transformation experiments? They seem to be not so well controlled,as one would like, but may have some features of interest. Unfortunately, Austrian (in a recent review Bact, Rev,) rather improperly quoted a mild comment I had made in MGB which, I hope, has not been offensive.

While I remember, could I ask you the favor of furnishing addresses for the following Italian scientists, as listed. I have seen their papers usually as titles or abstracts only and would like to enquire further:

Muromtsev, S.N. "Annali d'Igiene" 12:379-384 Il problema della ibridizzazione vegetativa in microbiologia. I have not seen the paper; journal not available

Zironi (loc. cit. Bizzari) 10:277-284

Denes, G. G. Batt. e Imm. 15:60-78 1935 Journal not available

Bizzari,M.Boll. Soc.Int. Micr. Sez. Ital. 9:260-262 1937 Jl. not available

Ciantini (7B. ISTSM. 1944) new developments since 1944?

These may all have to do with "paragglutination"--mI am collecting notes for a comprehensive review I may sometime write. If you know of any other comprehensive summaries of it during the past 15 years, I would be grateful to hear of it.

Sincerely,

Joshua

Joshua Lederberg