July 9, 1952

Dear Luca:

I return our ms. herewith. You certainly have no cause to apologize for your style, which is much closer to good English than to Anglo-Italian. I have marked a few places for your attention; many of them have nothing to do with linguistic style, but seemed to be points of redundancy, such as always creep in during preliminary drafts. I am quite content with the substance of the paper. Mrs, Lederberg's experiment with a lambda lysate seems quite correct, but needs a good deal of further study, especially to separate the F+ agent from lambda.

I would concur in calling F+ a "virus-like agent", but hesitate to describe it, even for convenience, as a virus. For just this purpose, I have suggested another term plasmid, which whil it is suggestive has the advantage of being new enough not to have too many associations. I regret not having a spare copy of my review, just completed, but it might be pertinent to refer tonit: Lederberg, J. 1952 Cell genetics and hereditary symbiosis. Physiol. Rev. In Press.

[While writing to this, I am listening by radio to the Republican Convention--our Sen. McCarthy is speaking; I hope my language remains coherent].

Other references which you may not yet have in full are:

Lederberg,J., Lederberg,E.M., Zinder,N.D. and Lively,E.R. 1951 Recombination analysis of bacterial heredity. Cold Spring Harbor Symp. Quant. Biol. 16: 413-443. [most general reference of our accumulated data; discussion emphasizes homothallism!]

Lederberg,E.M. and Lederberg,J. 1953 Genetic studies of lysogenicity in Escherichia coli. Genetics, In Press. [You should have received a mimeographed preprint of this one].

Lederberg,J. 1950 Isolation and characterization of biochemical mutants of bacteria. Meth. Med. Res. 3: 5-22.

Zinder,N.D. and Lederberg,J. 1952 Genetic exchange in Salmonella. J. Bact. 64: In Press.

I will remain in Madison till about August 15-Sept. 10. It would be best to continue to address me here.

Re LC&L, I agree that we should remove the parenthesis "(with which we concur)". I accepted his exptl. results on the basis of crosses on sm-agar, but have not properly confirmed his claims of the crossability of properly inactivated F+ cells.

Back to C L & L, the normal path of F transduction seems still most likely cell-to-cell contact. Your final speculation on ploidy is most stimulating, but I am still uncertain of its detailed application. Would you suggest that complete reduction eventually takes place? Otherwise, how does one avoid the very frequent occurrence of heterozygous recombinants. On the other hand, crosses of known 2n x n do give very high yields of diploid recombinants, as aexpected.

Such crosses involving Hfr x F+ and Hfr x F- are just now coming through, and at least some of the progeny are Hfr. The analysis is in progress; I think it quite likely that Hfr is localized in the usually eliminated segment. If I verify Hfr in combination with other markers (e.g. TLB_1-), I will forward the strains to you).

[Szybalski has mentioned your ms. on chloromycetin resistance. Although I am not likely to be able to comment on it very informatively, would you consent that I should see it?]

Sincerely

P.S. Your "Genetica della Fertilita'..." looks OK.