

October 1, 1956

Dr. E. Kellenberger
Geneva

Dear Dr. Kellenberger:

* *initials*
see note
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Thank you for your letter of September 27, and for the reprints on L-forms recently received. I was, of course, unaware of this work until now and have been pleased to see it. The main advantage of calling the globular forms "protoplasts" may be as a basis of persuading Spiegelman and others to shift to this material for the physiological studies they had been doing before with *B. megaterium*. I am concerned to call more explicit attention to the rich literature on L-forms, and their relationships to my "protoplasts" which may not have been sufficiently emphasized. I have therefore submitted the enclosed note as a sort of postscript to the ms. you already have.

Recently we have been learning to produce and grow the L-forms of *E. coli* K-12. There is a great deal of individual strain specificity, and finickiness about media, in this and we still don't have a thorough picture. Our objectives are those already indicated in the first ms.; in general, the genetic properties of the L-forms (and, if they occur, the genetic basis of the irreversible L form.)

Your remarks about your student's work on Gal transduction are somewhat puzzling, and I will defer comment until we have studied them further. Are you dealing with transductions within the K-12 line? (My wife had noticed some deviations in the behavior of transductions among different *E. coli* strains). We have never observed "stable immunes" coming out of transductions to Lp^S recipients. However, an

Lp - "immune" heterogenote (hypothetically designated as: $Gal^+ Lp^S$ ex/ $Gal^- Lp^S$)

can give rise, in addition to Gal^+ and Gal^- haploid, Lp^S , segregants to another class which (in the same notation $Gal^- Lp^S$ ex/ $Gal^- Lp^S$) is now pure for Gal^- and

still immune, but actually homogenetic for Gal, and unstable for the immune character. I.E., the immune Gal^- invariably segregate sensitive Gal^- , and also meet other tests for being homogenetic for Gal (e.g., reversion followed by segregation). Does this contradict your findings?

We have some problem of communication here, as Dr. Morse has now moved to the Dept. Biophysics, Univ. Colorado Med. Ctr., Denver 20, Colorado. It would be very helpful if you could send another copy of your informative correspondence to him for his comment, which would be easier than if I have to copy your letters.

Yours sincerely,

Joshua Lederberg
Joshua Lederberg
Professor of Genetics

Please refer this to ↑