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Dear Dave:

I hope everything is going well with you at Cold Spring Harbor. Tracy, as well as myself, has been concerned not to have heard from you since you left the Midwest. I trust this is a sign of distraction rather than difficulty.

I want to check with you about a detail on the behavior of E. coli, line 28, mentioned in your reports. In Table 1 of one of your summaries which reviews the over-all behavior of lines 1 to 31, line 28 is recorded as being not only ss but also motile in three out of three trials. This does not quite agree with our present experience; that is, I have not yet been able to obtain a motile derivative of the line 28 that we are now using. As there has been, however, some confusion in the designation of this particular line it is possible that you were using a slightly different strain. Do you have any other details on this particular experiment?

Aleck Bernstein has been occupying himself with the chemical properties of the flagella of different Salmonella types. It has been possible to confirm the old observation of Sertic that phase II flagella are generally agglutinated by acriflavine whereas phase I flagella are not. This shows up not only in the agglutination of the intact bacteria but also, so far as current experiments indicate, to the isolated flagella as well. We are setting out to do somewhat more detailed physical-chemical comparisons of this material. The difference of inagglutin ability of the different phases appears to be quite general. All of the phase I flagella that have been tested are inagglutinable while all of the phase II flagella are agglutinable. This concords rather nicely with the bi-local determination of these complexes. Aleck and Esther have also verified the crossability of a number of O-55 strains. They are, however, only just now about to make serological determinations.

I am personally occupied at the present time very largely with cytology and with efforts to demonstrate the mating process under the microscope. More and more compelling evidence has been coming up in support of the integrity of the gametes in E. coli crosses and with a post-zygotic mechanism of elimination. However, the process has yet to be observed under the microscope — but as you may imagine, the technical difficulties are considerable.

As you may know, Helen Byers came back from Europe this summer a bit late but also carrying with her an amebic infection. This has been cleared up, however, and she is busily engaged in her classes and in some research.

I am just about to finish a comprehensive experiment on the experimental production of incompatibility by transfer in motility medium. Twenty separate colonies of 58161 were carried for two passages in tubes or plates of motility medium. Fourteen of the twenty isolates were F-. Of the remaining six, one appears to have an exhalted F+ status -- a result which had been noticed before, while most of the remainder appeared to be either like 58161 or relatively, but not completely, sterile. These isolates will be tested through additional passages in motility medium. In addition, the first passage strains will be checked to determine what proportions of the strains had already become F- at that time.

I hope to hear from you and what you are doing before this work is completed. At that time, however, I will return the draft manuscript of the paper on this subject, which I think should be prepared for publication before too much more time passes.

I am looking forward to hearing from you in the very near future.

Yours,

Joshua Lederberg

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