

January 23, 1948.

Dr. S.C. Rittenberg,
The University of Southern California,
University Park,
Los Angeles 7, California.

Dear Dr. Rittenberg,

Thank you for writing me about your plans with Salmonella. It turns out that we are interested now in very much the same problem, but there is no reason why we should not turn this common interest into mutual advantage rather than mutual embarrassment. Long before I had any indication of success in my coli recombination experiments, I was very much struck by the patterns of the diagnostic serological formulae in the Salmonella group. To me they implied very clearly that new types were generated, at least in part, by genetic recombination. My incidental study of the nutrition of the Salmonella group was motivated by this inference, as I hoped to find among existing types material with which to do recombination experiments on a nutritional basis. For a variety of reasons, no critical test was possible with the "natural" material. Having just become securely re-established at Wisconsin, I am turning my attention to this problem again.

The more work that can be focussed on the group, the more surely the problem will be solved. It is my feeling, however, that the first few attempts will surely be unsuccessful, for there is every reason to expect strain specificities in the recombination reaction. In coli, as you know, K-12 is the only one among three strains tested which "paid-off". I would suggest that we both continue this study according to our own lights, and keep each other well-informed as to developments.

As to the genetic control of antigenic structure, I think that the existing data allow as definite a conclusion as will ever be possible without the aid of recombination factorial analysis. The flagellar antigens are certainly determined by some hereditary unit distinct from the antigens themselves, in view of the constancy of potentialities in the course of phase variation, and because the specificity of subsequent flagellar production is supposed to be ~~xxx~~ unaffected by treatments (e.g. phenol agar) which suppress the phenotypic manifestation of that capacity. However, I am at a loss to provide any simple, hypothetical explanation of phase variation that takes all the facts into account. It is the genetical basis of phase variation that I hope can be studied adequately if re-combinational techniques can be devised.

I should be very interested to see some work done on antigenic variation and inheritance in E. coli K-12. If that sort of problem should come within your fancy, or that of one of your students, I shall be glad to send you any strains you would need.

Yours sincerely,

Joshua Lederberg
Assistant Professor of Genetics