

OC. 25 72

not sent  
made a fresh  
start

Professor Olby

Thank you for your note of October 16th and for the typescript chapter.

I really do not have very much to add to your account. I think I might give a somewhat different distribution of emphasis to different aspects of the story, but I find it difficult even to state very precisely which alterations would differentiate your approach from any that I might have. I would recommend that you look at the reviews indicated in my letter in answer to Wyatt insofar as they deal with the background of genetic insight in bacteriology. In particular in #13 is a comprehensive perspective of the various biological interpretations that might well have been offered, entirely legitimately on the basis of existing fact, for the pneumococcal transformation. Perhaps the one point that I would emphasize more strongly than you have -- although this may be a personal reaction out of my own entry into the picture, is how difficult it was to get biologists generally interested in bacteria as legitimate objects of genetic investigation. The attached quotation from Cannon is a latter day caricature of an attitude that reflected the absolute separation of the disciplines of bacteriology (medical) and genetics for many decades. I will be elaborating on this again in a memoir I should be writing soon on the background of the discovery of bacterial recombination.

I am also enclosing another quotation, from Burnett, which is faintly arrogant in its exaggeration -- for I believe that the concept of natural selection was grasped by many microbiologists. But it has to be said that that grasp was a rather tenuous one and that a large part of the bacteriological tradition failed to make a clear distinction between the characteristics of a bacterial cell and those of a population or culture. I had to put a lot of emphasis on this elementary distinction in my earlier reviews, and I think

(Professor Olby cont'd)

it is true that very few bacteriologists addressed the question of the rates of mutation, per cell per generation that together with natural selection could account for the changes seen in their cultures. I would hesitate to call Griffith a Lamarckian for this would imply some greater clarity about the distinction between articulate and populational response than I believe he exhibited in his earlier paper. Very few bacteriologists at that time knew how to phrase the question!

The problem of the fixity of bacterial species loomed so large in the period from 1900 to 1930 that I felt the discussion that appeared on page 16 was overdue and should have been presented in some more detail at the beginning of the chapter. Of course, in the latter part of 19th century a great many claims of bacterial variation clearly attributable to contamination and one can understand the rigidity of the "monotypists" who by modern standards obviously veered too far in the opposite direction.

The discussion at page 17 of Avery's reluctance to accept Griffith's claims is new and fascinating to me. But I wonder if you are not projecting more interest in the biology, in contrast to the chemistry, of the pneumococcus than Avery himself had.