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Dr. Joshua Lederberg
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Dear Joshua,

I thought your letter in Nature was very good, but I am afraid I can't throw any further light on the early history of transformation and its impact on geneticists. I did not have any real contact with geneticists until several years after the Avery publication, and I was not in touch with Harriett Taylor during the period of her developing interests in this field.

It does seem to me that some of the responsibility lay in Avery's extreme modesty and his attitude toward publication. He was obviously correct in assuming that an important discovery would inevitably be recognized, but another man in his position, realizing that the discovery had broad biological significance, might not have settled for publication in JEM. I was amused some years later, in teaching at CalTech, to find that they did not have JEM in the library. It is a far cry from the hucksterism of modern molecular genetics to the atmosphere of Avery's laboratory, in which science was much more a personal and artistic acitivity, and much less a matter of social obligation (and recognition). Nevertheless, as Dubos once remarked to me, it is more than coincidence that Avery devoted his life to studying the organism that was then the major cause of death in our country.

I happened to hear Avery deliver the seminar at the Rockefeller in which this discovery was first announced, and I don't think it is retrospective romanticization for me to say that the extreme importance of this discovery was quite apparent. I recall that Avery gave the seminar from notes, then read the last page or two of the discussion off his manuscript, obviously in an effort to make sure that his interpretations were stated with great care and conservatism. It seemed to me pretty clear that he was transferring genetic material, though he was too modest to say so clearly; I was delighted to see Muller support this interpretation a few years later. I have no idea, however, how acceptable this view was to most geneticists.

While Wyatt has perhaps exaggerated the lack of impact on the genetics community, it seems to me that a much more serious distortion of his early history was that presented by Stent in "Phage and the Origins of Molecular Biology". There he states that people working on phage were not much influenced by the Avery discovery, which may well be true: but he goes on to imply that it was reasonable to expect that the discovery could have little impact until the phenomenon was demonstrated in phage. I had a chance to take Gunther to task for his interpretation: whether or not this conversation had anything to do with it, he has since published a more accurate version of the history. In any case,the solipsism of many phage workers at that time is evident, and tells us more about them than about anything else.

We dedicated the first edition of our textbook of Microbiology to Avery, as an unsung hero: the statement is: "to the memory of Oswald T. Avery, whose life-long study of a single pathogenic bacterium culmininated in the discovery that DNA is the prime carrier of genetic information". I hope we will continue this dedication in the second edition next year.

Thanks for writing me. I am sorry I can't give you more concrete information on facts of real historic importance.

Cordially,

Bernard

Bernard D. Davis

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