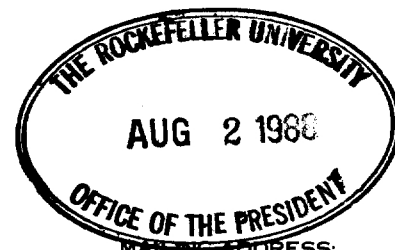


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26 July, 1988

Joshua Lederberg, President  
The Rockefeller University  
1230 York Avenue  
New York, NY 10021

Dear Dr. Lederberg:

Your letter with enclosures has finally arrived! I am sorry you had so much difficulty locating me. I too regret not meeting when we both resided so close to each other.

After 38 years, my recall is not so spontaneous. Add to that the fuzziness that accompanies one soon to be eligible to join the septigenarian club and you can appreciate that I will need a little time to come up with the definitive version of what happened back then.

I can say without hesitation that no preconceived theory was involved. The initial discovery that combining avirulent strains of *C. diphtheriae* with bacteriophage resulted in the production of virulent mutations, definitely was an act of serendipity on my part. The theory which Neal ascribes to me concerning release of toxin by phage lysis was an unlikely hypothesis, as I indicated in my initial paper in 1951.

At the moment, I can't recall if Barksdale questioned me about the initial step leading to the discovery of the phenomenon. I certainly don't remember employing phage to enhance toxin diffusion.

I assume you are aware that my reason for using phages in the first place was to simulate a phage typing scheme for strains of *C. diphtheriae* according to the Salmonella typing model. As soon as we started testing the avirulent strains, it was obvious that something was amiss. That something quickly put a hold on what was supposed to be my principal research work, as you can well imagine.

In spite of definite discouragement from my advisors in the Department of Microbiology, who understandably suspected contamination, I decided to switch tracks and proceed full blast in the direction of determining the relationship of the phage to the toxin mutation.

Incidentally, Neal was only partially correct when he indi-

cated that I didn't want to pursue "the possibilities of a phage-host relationship further." I was interested, but was convinced that I would have to go into graduate training in biochemistry, which I wasn't prepared to do at that time.

I will dig into my old files to see if I kept any of my original notes. I fear they were discarded after I got so embroiled in the field of psychiatry (which is a story unto itself!). But if I should come across anything pertinent to the history you are looking for, and/or recall more detail, I will let you know.

I want to thank you for your interest in my role in discovering lysogenic conversion in *C. diphtheriae*.

Very truly yours,



Victor J. Freeman, M.D.

P.S. Thank you for the copy of "a discovery account."