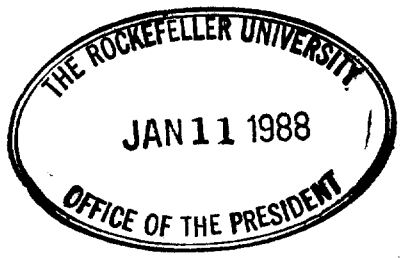

Box C321 UCHSC
Denver, CO 80262
January 6, 1987

Dr. Joshua Lederberg, President
The Rockefeller University
1230 York Avenue
New York, NY 10021-6399

Dear Joshua,

It was a delight to receive your letters with its reminders of the equally delightful visit to Madison in November of 1958. It was the first time I had met you and the only time we have conversed at length. I'm not sure what prompted your first letter, but it may have come out of your stay at the Hall Institute in Melbourne and the article you wrote with Gus Nossal. Soon after that visit you moved to Stanford and I moved to Colorado. So I am afraid that my files from that period are lost. I do think that you sent me a copy of a talk you were planning to give but I don't remember any details.

I'm enclosing copies of my early reprints that are relevant to the selection theory and some recent papers giving my version and interpretations. I am embarassed at the amount of duplication but these are all invited lectures or papers.

The cell selection theory had tough going until approximately 1970. Two things happened to change all that. The detailed structure of the Ig molecule and the genetic basis of its diversity were gradually elucidated. And the molecular biology of protein synthesis with messenger RNA and ribosomes left no room for a role for antigen. The final coup was the production of monoclonal antibodies. Although from a strictly rational view, this provided no more evidence than already existed, its widespread utility and dramatic effectiveness essentially eliminated all opposition. My personal opinion is that Mel Cohn's three papers describing results from five cells (published after talking about them for three years) were a disgrace which has never been repudiated. However, your earlier paper with Nossal and subsequent work by Makela provide strong contrary reports.

I have become interested in the process of scientific advance and was fascinated by your discussion in the Ann. Rev. paper. A notice of a talk I gave recently as a sounding board for a "History of Immunology" lecture at the FASEB meeting this spring stimulated a lot of controversy. I have come around slowly to the opinion that Thomas Kuhn is wrong about the basic thrust of his paradigm shift. While he makes many good points about the human failings of scientists the overall effect of his model is to strengthen the dominant relativism of our society and induce cynicism regarding the value of scientific progress. Thus, I have tried to show that the cell selection theory was a logical and inevitable result of the evolution of concepts and technology in many related fields. Even if neither Burnet or I had ever gone into this field, the general progress in molecular biology would have forced a shift in immunological thinking (whether you call that a paradigm or not).

It is possible that biology because it is more complex is somewhat different from physics where first Newton and then Einstein have played a dominant roled on thinking in their field for a long time. In my talk I tried to show that Newton's undoubted genius owed a large debt to a long list of advances beginning with the introduction of the zero into European numbers from India through the Arabs in Spain.

Sincerely yours,

David Talmage