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Dear Alex,

I cannot tell you what a warm feeling was evoked by your letter of September 6th. It made me regret that I was not closer to hand to be able to embrace you. It made me think I really have never adequately expressed the appreciation that I indeed felt for the way you had looked after me. I was too innocent at the time to know the kinds of problems that you must have faced in securing my appointment. My own later experience has given me the perspective one needs to understand it but I had never really placed your responsibilities vis-a-vis getting me started in that perspective.

There were many interesting things in the correspondence you were able to send me, although only a few touched on my main motive in asking you to dig it up. This motive was to get a further mirror on the attitudes of biologists and of academic administrators concerning the new developments in molecular genetics that were just breaking at that time. Inevitably this is of course deeply intermingled with measures of personal reputation. I can hardly be totally uninterested in this but too many years have passed for me to continue to have a deeply morbid curiosity about the more personal aspects of how people felt about a brash young man who had just started his academic career.

I can have no quarrel with the restrictions attached to the letter that you mentioned as being omitted from this set. I hope though that it will be possible to tag that in the university archives, so that it may eventually be part of a more complete record should anyone have the interest to follow it up further sometime in the future. I have really been very discouraged to find how much interesting and important material has been lost or destroyed. By coincidence I had just replied to a query, which I think was in no way related to my own burrowing, from the Stanford library archives which makes a comment in this direction.

There is one point in the correspondence that, to be quite candid, did startle me, though it is only by way of reminding me what I knew very well and have learned to forget: the extent to which "race" was in anyone's mind in justifying academic appointments. The references in these letters are of course all by way of sincere efforts to overcome that kind of prejudice, but they are inevitably most eloquent in speaking to the atmosphere that they were designed to penetrate. What I find almost amusing is that a set of attributes that might well have been caused for legitimate criticism were connected with my racial background rather than with my own personality.

Obviously we face the same kind of confusion between person and group today, even though the arena has shifted to other ethnic areas! The correspondence did do me a service by reminding me of the role of anti-Semitic feeling in the development of various academic disciplines. This was something I could hardly be totally oblivious about although I had been looking at it from a rather different angle.

Alex, I really have been disappointed I have not succeeded in making a more direct contribution to agriculture by way of applications of my work in genetics. Of course, I do not have to lecture myself about the need to establish the basic foundations in order for this to happen and there are already many starts for which I can claim to have made some initial contribution. I do not think this was entirely my own fault -- the agricultural research administration certainly gave very little evidence of imaginative thinking in supporting projects that might lead to an earlier application of molecular genetics to agricultural problems. Of course, until we can get the biochemistry of seed proteins down to the level where we understand the amino acid sequences and the regulatory mechanisms at a fundamental level as we have in our grasp for their bacterial analogues, it is really difficult to make any rational revolutionary steps. However, I think the time is ripe to expect this to be coming about and I am looking forward to joining a conference at the National Academy next spring which has the development of ideas along these lines as one of its principal aims. Knowing what we do about genetic structure, it seems to me almost ludicrous that we cannot promptly modify the amino acid composition of major seed proteins so as to improve their nutritive value and without prejudicing their crop characteristics. I hope I am not being unduly insensitive to the practical problems of breeding plants in a way that emulates our test tube experiments with bacteria! But Carlson's most recent work with protoplast fusion in tobacco is just a harbinger of a multitude of new techniques that are bound to emerge.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr