

STANFORD UNIVERSITY MEDICAL CENTER

DEPARTMENT OF GENETICS

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Dear Harriet,

Dave Goddard, as Home Secretary of the NAS, did Commission Beadle and me to write the memoir for the NAS on Ed Tatum, so I am indeed starting to collect the material for this. These memoirs are not such comprehensive biographies but I thought that the opportunity should not be wasted to collect a meaningful set of documents which are already difficult to accumulate and which will certainly disappear if not tended to. I will probably try to find some occasion during the next 6 months to spend a day at the Rockefeller library and meanwhile will be in touch with Howard Tatum and with a number of his professional associates towards this end. If you have any ideas of points to look for that may emerge from our own joint work or from anything else that you happen to know about, this would be a good time to try to sensitize me to them. I do not have any very clear organizing theme in mind at the present time, and as I already said, the memoir is not the kind of vehicle for analytical exposition that I hope our paper in Daedalus will be a prototype of.

One thing that I am finding again is that even getting the local history straight for 1940 is already very difficult! There literally seem to be just no records on personnel or department policy or any other important themes for that interval. One part of the problem is that Ray Lyman Wilbur retired in 1943, but his own memoirs are avowedly incomplete with respect to the last 10 years of his term and specifically with respect to the encouragement of scientific research at Stanford! What he did with his own papers, I just don't know, and I am not sure it would be worth a great deal of effort to try to chase them down because they so awkwardly overlap the period of interest with respect to Ed Tatum.

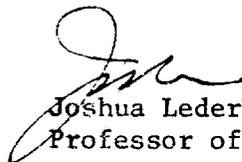
I did talk to Arthur Giese who was a contemporary of Ed's in the department and also to June Tatum, and they did both corroborate the picture that 1943-44 was a very difficult time indeed in biology. June mentioned Wilbur's death and the fact that his successor Tresidder was relatively ineffectual and not very interested in science as being contributing factors to the well known fact that "the department just fell apart around that time". To add to the calamity, C.V. Taylor was in his terminal illness during 1944-45 but refused to relinquish the chair at this still very critical time. I have not been able to get a clear confrontation on the question of whether Ed was denied tenure and would have been obliged to leave: I just don't know what the tenure system at Stanford was in 1944. It is quite clear from what both June and Arthur said that he was not offered a promotion from assistant professor nor was the university in a position to offer very much by way of facilities. Yale did make him a very attractive offer and in any case that was irresistible.

I did question Beadle about the same circumstances and he was unable to tell me whether he had already decided to leave Stanford for Cal Tech at the time that these questions arose vis-a-vis Ed Tatum. I did not feel it would be worthwhile to press him on whether he would have made a strong effort to keep Ed at Stanford if the resources were available. He himself made a rather strong point that he does not have a clear recollection of the details of that time, and as you can see from his enclosed letter, he in fact expressed his gratitude that I was trying to approach the same matter from a more critical standpoint.

I am still looking to see whether there are any histories of the university for the interval of its flowering. I would imagine that Wally Sterling would try to write something along these lines, and perhaps I will give him a buzz.

Anyhow, I just wanted to raise the general background of this particular pursuit without having anything very concrete to ask of you about it at that time.

Sincerely yours,



Joshua Lederberg
Professor of Genetics

JL/rr

CC: as memo to file.