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DEPARTMENT OF BIOLOGICAL SCIENCES

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Dr. Joshua Lederberg
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Dear Josh:

Good to hear from you. The Tatum memoir is thoughtful and sensitive.

I'm afraid not much can be said about pdx (299), or about pdx mutants in general. N. sitophila has been little used, and linkage groups are not defined. I'm not aware of any evidence for or against allelism of 299 with the single known pdx locus in N. crassa, but the best bet would be that it is at the same locus. All the known N. crassa pdx mutants have proved to be alleles at the same locus, pdx-1, in linkage group IV. It would be perfectly feasible to introgress 299 into crassa and do allelism tests and mapping there, but no one has been motivated to do so.

There is also no evidence to my knowledge of a pdx-specified enzyme or gene product, either in Neurospora or Saccharomyces or Aspergillus. Only a single pdx locus is known in yeast, two in Aspergillus. In neither organism is the lesion identified.

Let me write down a few comments and thoughts regarding ELT, not necessarily because they will be of any use for your memoir, but because it has sparked my memory.

Ed was extremely kind and helpful to me as I got started at Stanford. I felt almost as though I were part of his group, and had good opportunity to observe his relations with his students and associates. He was infinitely patient and kind, even with the weaker students, whom he kept on with a sort of personal sympathy and loyalty, trying to bring out their best. I think he valued integrity above all. He was absolutely honest and showed great care to spend grant funds carefully and thriftily. He went to great length to avoid using his position to obtain favors. For example, none of his students were allowed to apply for N.I.H. Fellowships while he was on the panel handling them, according to Dot.

Ed loved to do things in the lab with his hands, to build equipment, etc. But his time to do so was shrinking during the Stanford years at the same time that the bailing-wire and sealing-wax approach, on a shoestring budget, which may have typified earlier research periods, was becoming less and less appropriate or necessary.

I think not much need or should be said about research accomplishments at Stanford during the 1948-57 period. He had a few excellent students (e.g. Adelberg, Fuller) and one very good postdoc (Gross), but not as many good people as he deserved. He became increasingly involved in activities outside the lab -- editing for J.B.C., national committees, etc. While he came into the lab regularly, and consulted with Laura Garnjobst and with the others, he found it increasingly difficult to carry on experiments himself. I still remember his expression of frustration when he had to go upstairs and read manuscripts or undertake other chores that took him away from the lab. But it was a choice he made. And he confessed that he enjoyed the committees and panels that took him East.

I have wondered about one thing that isn't mentioned in your memoir. Somewhere I obtained the picture that there was a hiatus between Stanford and Yale in the 1940's, during which Ed was at loose ends, and that the Tatums went to St. Louis during this period when Ed was without a post, at Lindegren's invitation. Is this just my imagination? Ed never told me of this period, but I do know that the Tatums and the Lindegrens were on close, friendly terms. Sometime in the 1950's, I went with Ed and June to San Francisco -- probably to Bacteriology meetings -- and watched the Tatums and Lindegrens during several hours together. They were obviously old and warm friends.

On page 9 of your draft memoirs, it might be well to check the papers on transformation. The earliest, preliminary publication, not often cited, involved not only N.C. Mishra, but also a Hungarian, Gabor Szebo, who was at Rockefeller when the first experiments were done. (Szebo has extended the work in recent years, at Debrecen.) Transformation was with DNA, not RNA, I think you'll find. The inositol-independent segregants did not segregate like Mendelian markers in crosses, and this was one problem which contributed to lack of credibility at the time.

It turns out now that meiotic instability and loss is characteristic of Neurospora transformants. Neurospora can now be transformed with high efficiency (as much as 10%). Transformation typically involves chromosomal integration, though usually at nonhomologous loci. The transformants are relatively stable mitotically, but are mostly unstable meiotically. So the Mishra-Tatum observations weren't so out of line as they originally seemed.

Perkins/Lederberg

12.16.85, P. 3

On. p.8, 4th paragraph, I would suggest omitting the clause about rebuilding his personal life. Ed had some 16 years of marriage to Viola before her death. Wouldn't a simple statement suffice: that her death, which occurred the year before his own, left him shattered?

Finally, one small point. On p. 6, line 18. Tatum didn't have his own department. Substitute "group" or "lab"?

Are you still projecting a book on the origins of one-gene one-enzyme and the people involved?

Best wishes,

A handwritten signature in cursive script that reads "David".

David Perkins

DDP/cmh