

DATE: June 13, 1977

TO : To the file

FROM : Joshua Lederberg

SUBJECT: Dr. Russell Perry Hager's reminiscences about Ed Tatum

I had noticed that Dr. Hager was listed as a technical assistant in the 1942 PNAS paper and got the idea of trying to look him up for recollections, since I have found so few people who were in the lab during 1941-42.

Dr. Hager later on became a physician and internist. He was quite easy to locate from American Men of Science and the telephone information service. When I called him he told me that he had had a stroke not long ago, but was recovering and his voice was certainly very firm.

He was just delighted to talk about Ed Tatum, whom he regarded as a very fine person. He was more ambivalent about George Beadle, whom he said, "He was a hard man to get along with. He liked me at first, but later on wanted only people who would be able to do scut work and get the work on neurospora, rather than Ph.D.'s." He had come to Stanford as a research assistant, after completing his Ph.D. on *Habrobracon* during the summer of 1941 at Woods Hole. He was a little confused about the dates, but recalling that he had been at Stanford at the time of Pearl Harbor, it became quite clear that he first arrived at Stanford in September 1941. He believed that he had been on the very first isolation of an auxotroph, referring to a thiamin mutant. I had to tell him, but did not persevere, that that was not the first isolation.

His account was that Tatum and Beadle had numerous bull sessions concerning their continuing work on the biochemical genetics of *Drosophila*. Tatum teased Beadle for continuing to work with an organism like *Drosophila*, whose chemistry was so difficult. Why not work where you know more of the chemistry and simplify the whole deal? Beadle responded that they also needed to know the genetics. Well, they could not work on bacteria because the genetics was not accessible. So Beadle then suggested neurospora as a simple organism whose genetics already had been worked out. Tatum then responded that he was quite sure that neurospora could be grown on a simple synthetic medium. Beadle put in the idea of irradiating the single spores, hopefully in meiosis(sic!), and from there it went to the idea of getting auxotrophic mutants. He was really not able to give me more detail about the transitions of these ideas. Later, when a second thiamin mutant was isolated, according to Hager, it proved to complement the growth of the first one, and this led to the notion of blocks in the two halves of thiamin.

Apparently, the work was organized in such a way that the mutants were isolated and identified in Tatum's laboratory; when an auxotroph was indicated, Ed would run down the requirements in more detail. Then Beadle would do the genetics to look at the segregation.

Hager recalled that he had commented to them at the time that this work would earn a Nobel prize and was as important as Mendell's. Their response was "this was such a simple experiment."

Hager himself felt a bit out of place because he had been hired to do Drosophila research, and they had changed direction in the interval. However, they retrained him to be a technician doing the scut work on neurospora. In some respects Hager resented this, but he was willing and eager to put up with it because he felt it was terribly important, and, as he says now, was one of the most significant experiences of his own life.

I urged him to record a memorandum redescribing all this. He demurred somewhat, but if I would just write him a letter he thinks the he might be able to just dictate something that I can transcribe.

JL:ek-f

---

Telephone #: 401-781-2040 or 401-521-2331