PERSONAL

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Dear Joshua,

I have no recollection of having been involved in any way in Ed Tatum's coming to Stanford as George Beadle's research associate. Shortly after the spring quarter in 1935 I began my first sabbatical leave of absence, from which I didn't return till November, 1936. I attended the Cold Spring Harbor symposium on photobiology, spent a day at Harvard, but didn't meet Beadle (don't even know whether he was there at the time), and sailed for Europe where I participated in the program of the Internat. Botan. Congress in Amsterdam. I had intended to work in Otto Warburg's institute for the first 6 months, but changed my plans because earlier in 1935 Gaffron in Berlin and Roelofsen in Delft had published some conflicting papers on purple bacteria metabolism (I'd received the latest one during the CSH symposium), and I found it necessary to try and resolve the conflicting claims before going to Warburg. The Delft lab was equipped with all I needed; Roelofsen's cultures were still flourishing; so I stayed there for the next 6 months, working uninterruptedly about 15 hours every single day. When at the end the muddle had been cleared up, I went to Berlin to do a few critical experiments with Gaffron, then on to Basel to acquire some familiarity with chlorophyll chemistry in Stoll's institute where I spent the remainder of my sabbatical leave. There had not been time to visit other places in Holland, and I didn't meet Kögl, Haagen-Smit or Tatum that year.

The return voyage to California, on a Swedish freighter through the Panama Canal, provided the opportunity to write a paper I'd promised to do for the Bull. Assoc. Dipl. Microb. A rather hard task because there were no dictionaries on board, and the paper was to be in French. Back home, I immediately had to concentrate on the Ann. Rev. Biochem. article, and it was only after that was finished that I first met Ed Tatum who had come to visit us. We had never corresponded, and I knew of his work on Propionibacterium nutrition only from his publication. I expect that George Beadle will know who and what caused him to get Ed appointed by Stanford.

In the late '30s Ed taught a course at Hopkins Marine Station on the use of microorganisms for vitamin determinations. It may have been that same summer that Bill Arnold, then my Research Associate, gave a course on biophysics. C. V. Taylor attended and greatly admired it, saying that in his opinion every biology student ought to be exposed to this approach. Perhaps that was why he proposed to President Wilbur that the vacancy due to Prof. Burlingame's retirement be filled by the appointment of two new Assistant professors: Ed Tatum and Bill Arnold. Wilbur agreed, and they were added to the faculty in 1941. Already in November of that year Bill was recruited to join a team of scientists studying problems connected with various aspects of ballistics, on an indefinite leave of absence from Stanford.

When the war was over he was not reappointed to his former position, a shocking development to me, who did not know that Ed Tatum's fate was to be quite similar. Ed stayed at Stanford during the war years, and he and I jointly gave a general microbiology course on the campus in 1942 or 43; Gus Doermann and Dave Regnery were among the students.

In 1944 I was invited to spend a few weeks at Yale Univ. to consult with the faculty on the establishment of a full-fledged microbiology department there. My report to Pres. Seymour contained recommendations for the appointment of a number of outstanding specialists in various branches, along with their names and accomplishments. As a result Ed was offered a position as Associate professor, at a salary of \$3,500. He came to see me one evening, and we talked about his prospects at Stanford where he much preferred to stay. But he could hardly afford to do so at his then salary of \$2,500, and told me that he would gladly decline the Yale offer if Stanford would raise his pay to \$3,000. When I said this would be readily acceded to (of course!), Ed felt greatly encouraged, almost jubilant; all seemed to be going well.

Alas, my hopes and expectations were totally shattered at the faculty meeting Taylor had called to consider the situation. To my utter surprise and disgust, a large majority voted against any kind of advancement for Ed, arguing that this was a fine opportunity to compell him to leave, and then he could be replaced by someone who "could teach biology which was so badly needed!" Crestfallen, I immediately offered to resign myself--it was not accepted--and after the meeting I went to see Ed, expressing my apologies for having raised false hopes. And so, Ed went to New Haven.

During the following years I've often pondered the question what had caused the opposition of the faculty. The answer probably is that C. V.'s ascendency to departmental chairman had generated deep resentment, finally breaking out in open revolt. From the beginning Taylor had emphasized the importance of experimental approaches and particularly the use of physical and chemical methods which many of the faculty were not equipped to do, and probably even failed to understand. Thus they banded together and for a while could defeat Taylor's aims.

I hope this information, incomplete as it is, may be of some help in preparing the biographical memoir. Sorry there are gaps but, as I may have mentioned to you on an earlier occasion, I gave my scientific library, notes and papers included, to the microbiology department at the Univ. of Groningen (Holland) three years ago, so that they are no longer accessible to me here.

Fond personal regards and wishes,

Kees