

UPPSALA UNIVERSITETS INSTITUTION  
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INSTITUTE OF PHYSIOLOGICAL BOTANY  
OF THE UNIVERSITY OF UPPSALA  
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Dear Josh:

Many thanks for your letters a month ago. I was very glad to learn about your initiative to prepare a biographical memoir of Ed Tatum. I deplore the delay in my answering your letters, but the last weeks have been a real turmoil of committee meetings, etc., so I have got a bit behindhand with my correspondence.

Needless to say I will do my best to provide you with all the information I have as regards Ed's activities and interests during his earlier years. However, I am afraid it is not much I can tell. I don't think Ed ever was a great letter-writer (nor was I), and from the period before 1941 I have only one letter from him, of which I enclose a photocopy. I am sure that before 1944 I did not receive more than that one letter - and a few post- and Christmas-cards -, since I have never thrown away any personal letters from my friends.

As you said in your letter, I met Ed at Utrecht in 1937, and there we made friends. That was the beginning of a long and solid friendship. After our stay in Holland in 1937 we met at least four times (New Haven 1947, Paris and Sweden 1954, Stockholm and Uppsala - the Nobel Prize - 1958, and New York 1969). I had hoped to see him last year at the Nobel Jubilee in Stockholm; he had written to me that he might come, and so I was shocked when André Lwoff just before the celebration ceremony told me that Ed had passed away a few months earlier.

In order to find answers to the questions you asked in your letters I have dug up some old letters from me to my parents during my Utrecht period. It was a great pleasure to read them after so many years. You know, I was only 25 at that time and it was my first scientific trip abroad. My letters show a rapidly increasing appreciation of "that strange American bacteriologist", who worked in "the foreigners' laboratory" of Kögl's institute together with me and the Belgian chemist Victor Desreux. I had never before seen a man keeping a cigarette in his mouth while using the same mouth for pipetting. Alas - Ed was a great smoker already at that time!

It was not mentioned in my letters, but if my memory serves me right, Ed mentioned that before Utrecht he had spent some time working with C. Fromageot at Lyon in France. I don't know whether Claude Fromageot is still living, but if so, you may get information about Ed's earlier activities from him. They seem to have collaborated even earlier than in 1936 to judge from a reference to Biochem. Ztschr.. 267, 360 (1933!). As to Victor Desreux he must also be able

to report to you, although I don't think he and Ed maintained any contact during the following years. Desreux became a professor of chemistry or biochemistry somewhere in Belgium but may have retired by now.

Anyway, in Utrecht Ed worked on the isolation of a growth factor for propionic acid bacteria. It may have been a continuation of an earlier study together with H.G. Wood and W.H. Peterson (J. Bact., 33, 227 (1937)). As it later turned out, the factor in question was probably identical with folic acid, chemically identified later by other people. Kögl's institute was definitively not the right place for a study of that sort, at least not by a visiting scientist. I don't want to expatiate on this, but the situation was certainly sometimes rather frustrating to Ed - he hints at that in the letter to me (copy enclosed) but in his characteristically tactful and temperate way.

Even if none of us three foreigners learned much about scientific methods and matters during our Utrecht sojourn, we had a good time together and certainly learned a lot from each other, which at least in my case proved to be of value for my intellectual development. Ed had learned French during his stay with Fromageot, now he was very keen on learning German too, and so we talked German together. As I mentioned in one of my letters home: "Here we are, representatives of 3 different nations and languages, working in a 4th nation and using a 5th language to understand one another". I think Ed's French was much better than his German. If I remember correctly he gave a lecture in French to a French audience when he visited France in 1954.

I am afraid I have been rather incoherent in my recollections, and as an excuse I can only refer to lack of time. Finally, however, I will try to answer the four questions you asked me in your letter of Nov. 11.

(1) What Ed worked on at that time? See above. To my knowledge he never published anything of his studies in Utrecht on the propionic acid bacteria factor.

(2) How he happened to choose that lab? I think it was simply because of the great fame surrounding Kögl's lab at that time. Remember: the auxins (one right and two wrong, though) and the new vitamin, biotin, had recently been isolated there, and the former had also been chemically identified (well - one right). Thus, this must be the place for elucidating a new growth factor of a Propionibacterium. The conclusion turned out to be wrong, however.

(3) His intellectual outlook at that time? Although we spent much time together I don't feel capable of commenting on this question. We were discussing almost anything, while working in the lab, on excursions, and in his home, where I sometimes was invited. What appealed very much to me was Ed's sense of humour, which, as you know, sometimes found drastic expressions, and his capacity for pronouncing impressive understatements. Furthermore I enjoyed his playfulness, which surprised me somewhat to find in a man three years older than myself. One day, for instance, we started producing soap-bubbles in the lab, making them as big as possible with the aid of various viscous mixtures and filling them with gas from the Bunsen-burners. They soared elegantly up to the ceiling where they smashed. But this has indeed nothing to do with Ed's intellectual outlook.

(4) How he was recruited to Stanford? It seems that Ed gives the answer in the P.S. of his letter to me in Jan. 1938.

As regards an "intimate history" of biotin I can only refer to

Kögl's & Tönnis' paper of 1936 "Über das Bios-Problem" (20. Mitteil. etc.) in Hoppe-Seyler's Ztschr. f. physiolog. Chemie, 242, 43-73, which is a good one, and the dissertation of W. van Hasselt in 1935 (cited by Kögl & Tönnis).

When I now have been looking back at my life at Utrecht in the spring of 1937 it strikes me that the Ed Tatum I met later in life differed very little from the young man I got to know there in the Kögl institute. To me at least he remained remarkably unchanged all his life. All his main traits of character, which you of course know better than myself, and which we estimated so much - his kindness, helpfulness, simplicity, and sense of humour - were already at that time fully developed and contributed to the quiet charm of his personality.

I am sorry I had to write this letter in such a hurry, but I promise to send more information later on, if such turns up.

With the best wishes to you and Esther for the new year,

Sincerely yours,

*Nils*

(Nils Fries)