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His note must have been in Lederberg file

OCT 18 1977

October 8, 1951.

Dear Nick:

I'm quite glad to go along with your plan to give a narrative account of the history of E. coli recombination, although whether it will keep your audience awake is still an experimental question. At the outset, I want to urge, very strongly, that you also consult Professor Tatum, mentioning if you wish, that I thought it worthwhile to bother him about it.

Although his papers don't mention it, Tatum's purpose in going after nutritional mutants in E. coli was partly to test the possibility of recombination. In 1945-46 I spent some time in Ryan's lab (while I was a medical student at Columbia) working on reverse-mutation and heterokaryotic competitions in Neurospora. The selection of prototroph reversions in Neurospora suggested the possibility of similar selection for recombinants in bacteria. Professor Tatum had a very similar notion. After he came to Yale, I wrote to him asking for some di-auxotroph mutants. Both of us had gotten prototrophs from monoauxotroph combinations, but could not, of course, be sure that they were not merely reversions. His ultimate response to my letter, facilitated by Ryan's personal contact with him, was an invitation to work on this problem at Yale (where Tatum was just organizing his lab.) whenever I could get leave from medical school. An elective quarter, followed by a summer vacation period materialized starting in March 1946, partly as a result of the abandonment of the Navy V-12 program, and the shift back to non-accelerated schedules, after ~~xxx~~ V-J day. I went to Yale with the help of a fellowship from the Jane Coffin Childs Memorial Fund for Medical Research, a cancer-research foundation administered at Yale, and as things turned out took my Ph.D. at Yale, and my appointment here, rather than ever return to medical school.

It would be difficult to say how or when the first prototroph occurred: probably both Tatum and I had seen recombinants before either of us knew of the other's interest, but the main problem was to establish the significance of the prototrophs, and this meant learning how to use additional markers. It was also necessary to learn the conditions under which syntrophism operated. The project was started, on the ~~xxxxxxxx~~ one hand, on such a long-chance basis, and on the other positive results were obtained so readily (e.g., in the first critical combination of two diauxotrophs) that there was o "lab opinion" greatly different from our own. The first hint of the work s at a local bacteriology meeting. ("reprint" enclosed). ~~xxxxxxx~~ The CSH osium was the first real discussion of it (Evelyn can tell you about it). e sent to Nature appeared prior to the publication of the 1946 symposium. me other anecdotal material is contained in my chapter in "Genetics in h Century, and in ~~xx~~ our CSH 1951 ms. It happens that Esther worked riginal isolation of the double auxotrophs at Stanford (see PNAS 31:219) relevant material is enclosed.

Sincerely

J. Lederberg