Ed denies (via Bassett) he ever got this.

October 3, 1950.

Dr. E. L. Tatum,
Dept. Biology,
Stanford University,
California.

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Dear Ed:

Esther and I are both very sorry that we did not have an opportunity to visit with you again during the summer, but our few weeks in Berkeley proved to be all too short to do all the things that we had hoped. We reached Berkeley several days before the beginning of the second summer session, which gave us an opportunity to see something of northern California. We visited Dobzhansky at his summer station at Mather (via a one-way mountain road in and out of Yosemite), and then drove northwards to Lassen Park, which we greatly enjoyed for a couple of days. We returned to Berkeley via Eureka, the Redwood Highway, and Napa valley. The course itself was moderately successful: at least the students refused to believe everything that I tried to tell them. I managed to do a very little research with Stanier on UV effects of adaptation in Pseudomonas. As one might expect UV strongly inhibits adaptation, but not the adapted enzymes. The inhibition can be photo-reversed, which suggests that photoreactivation is not the interruption of a process leading to killing, but an actual reversal of the photochemically terminal event. There is not a great deal of difference between these notions, but Novick and Szilard had been thinking more or less in terms of a poison, which could be destroyed by light before it had reacted with the cell constituents. The greatest use of thse findings may be as a better means of distinguishing adaptation than by slight differences in the shapes of curves of O_2/time. As I had anticipated, it turns out that unadapted cells also contain small amounts (a few %) of the oxidative enzymes usually regarded as adaptive.

The main point of this letter is to ask you about the progress of the organization of the Neurospora map, and to ask whether you would be kind enough to send a few stocks. James Crow and his student, Jean Pierle, have now gone through most of the preliminaries, and would like to start building up multiple marker stocks for crossover studies. From what I knew, I thought that the following markers would be most appropriate (on the sex chromosome):

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leuc- 47313
A/a
lys or citr (stock #??)
centromere
ad-p 35203
meth 35809
albino 15300
lys 4545.

It will probably take several months at least to build up the stocks, but by doing them in blocks, and building two stocks in alternation it should not be impossible. We would also like to use a centromere marker for another chromosome, as a check on slippage: i.e., an ascus showing apparent second-division segregation for the second centromere marker would be treated with suspicion as a possible slippage of nuclei II and III after meiosis. I don't have enough information to know what best to use, but thought at least to try 51602 (B2-temp). Would you send this also, if not suggestions on something better to use?

Jim and I will very much appreciate any help you can give us, and we will certainly keep you informed.

The Columbus meetings were interesting, but exhausting. Conspicuously good papers- Stadler, Darlington (the devil itself), and Ephrussi. Beadle gave a rather surprising talk: he discussed the historical origins of the "one-to-one" theory, and referred repeatedly to the one gene-one function (!) hypothesis.

Latarjet wrote that he is on the hunt for possible traces in Paris of coli C1 and C2, but refused to offer any encouragement. The Ephrussis will be here in a day or two, and I'll ask them as well - but they will undoubtedly be heading westward themselves.

Enough for now——

Sincerely,

Joshua Lederberg