

July 4, 1955

Dr. E. L. Tatum  
Department of Biology  
Stanford University  
California

Dear Ed:

Thank you for your note of the 24th. The Amherst Symposium was a very pleasant affair. I wish we could look forward to the same leisurely pace at DeBoit, but it seems otherwise.

Esther and I have been thinking very seriously about the chances of packing off to Stanford. At first, we weighed our thoughts down with all the day to day entanglements at home and the lab., but if we insist on waiting until these have evaporated, we never will get away. Instead, we shall have to make a plan, and adjust our other affairs to it. Just yet it is not certain that next Spring (1956) will be possible, but let us try to make a concrete plan as if it were, and we can settle precise dates later. What are the problems? First here, then at Stanford.

U.W. 1. Leave. We can get leave (without pay) practically any time. I am scheduled for a course from Sept-Jan. 20 which I would prefer not to disturb too violently, but this is adjustable too.

2. Transportation. I assume we would drive. If we manage the spring quarter, would this be February-March-April? Assuming so, is there any special difficulty driving through Sante Fe\*in late January? or would we have to go further south?

3. Home and Lab. No time is much worse than any other. Spring quarter might be the most convenient compromise.

Stanford: 1. When is spring quarter? Would any other interval be feasible?

2. Teaching-- I would be glad to give my course, if expected, at whatever level of advancement your curriculum would accommodate. At Madison, I alternate yearly between an advanced genetics course and a more elementary one for bacteriology majors. However, I have not done a lab., but would try to cooperate if you've had one going.

3. Housing. What are the prospects? (I don't think we'd plan to rent our house for the short interval, though we might try to get someone to look after it).

4. Salaries. I suppose this would be the toughest nut! We can take a month as vacation time to cover our travel time, but can you handle us for our actual residence at Stanford? Esther's position here is Project (i.e., Research) Associate which ranks and pays about equivalent to a ~~starting~~ assistant professor. If you can't manage otherwise, I could try to get a research leave from the WARF research committee. This would be unpredictable, and probably inconsistent with teaching, but I don't know if a shared plan would be acceptable.

\*or Albuquerque

It is probably too late to get any help from that source for spring 1956, but if necessary, I might start the wheels turning for the following year.

5. Research. This one's easy—the problem we talked about, the mycelial fragments, has lots of possibilities. Would it help to have a written project outline? Have you made one up? Should I send some notes with my own ideas on it? I am assuming you're not going to be too preoccupied not to be the senior active working partner with Esther and me on this. Otherwise, there'd be no hope of getting anything done in a short visit, and there'd be no fun to it anyhow. Has anything been published on the fragments at all? Is there any quantitative information on viability from your own notes? In my own mind, so far, there are three main aspects to look into:

a. The fragments themselves; what is the smallest unit capable of vegetative (or sexual) viability. (This may take some micromanipulation).

b. Using fragments as possible recipients, e.g., of transduction or other genetic effects that may depend on relatively exposed nuclei.

c. The effect of fragmentation itself (which may possibly include some nuclear fragmentation) on genetic stability, especially in heterokaryons. I am surprised no one has done much since Newcomer on cytogenetic sequelae of ultrasonics in higher plants.

We know that there may be many difficulties in your way, and that until it is sealed, anything you (or I) may say will have to be tentative. At the moment, we have no alternative conflicting plans for next spring/ It probably will be more urgent for you than for us to settle the plans reasonably in advance.

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The Detroit symposium has me worried, since I am not sure either why I was asked, or why I accepted. I have done too little work on gene-enzyme relationships in the last five years to be able to anything very concrete from my own experience, so that about all I have left is a certain amount of pessimistic criticism, and this may be all I'll do, on the basis of the papers as I hear them. It might be better to give up going. But I really haven't had the leisure or the energy to give much thought to it.

As for the organization, since you asked, I would admit some regret that the afternoon session did not concentrate more effectively on the stated topic of gene enzyme relationships, for which Hershey and Hotchkiss seem to me out of place. Is it too late to translocate them? Don't misunderstand me; I'd go out of my way to hear Hershey under any pretext. And Hotchkiss gives a nice story, but I think it has only the color of biochemistry, because DNA is isolated by chemical procedures, while adding no more by itself to the story of genes and enzymes, than would any random example of genetic recombination, e.g., eye color in fishes. If it were the connecting pathway that was to be discussed, I would have to suggest Crick or Watson instead, for all of the speculativity of the latter's proposals. If there were than some more time for the 1st afternoon session, I would like to hear Yanofsky's story on tryptophane ~~desmolase~~ desmolase, which I hear is one of the most detailed analyses ever done on the enzymatic functions of a gene, and Itano on the genetics of the hemoglobins in man. If no one else brings up the latter, I may, from the point of view of how many loci (1 ?) are involved. On this last point, the literature confuses me—most of the inferences are drawn from the abnormal status of heterozygous compounds rather than proper recombination tests, and the reasoning may be circular.