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CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

April 26, 1948

Dear Josh,

I envy you your ability to get to the East on frequent occasions. This is one of the few bad things associated with life in California--the great distance from New York and environs. Of course, it would not be too bad if one were able to afford frequent plane flights.. but with the round trip fare well over \$200 per person, I am afraid it is pretty much out of the question.

Your ~~working~~ seems to be progressing at a promising clip.. It is of course unfortunate that you have not been able to land any students yet, but this of course is understandable in terms of the general shortage of graduate students, and their tendency to flock to the old established names in their science. Don't fret, your students will appear on the horizon.

Regarding your particular problem: It is important that you not concern yourself with the apparent snub by Delbrück. He is undoubtedly a hotshot at his bacteriophage work, but I regard him as very much of a cold potato. He has demonstrated evidence of his deep concern with his own problems, but doesn't impress me as being inclined to strain himself with other peoples' troubles. I do know that he feels that you ought to be obtaining further proof concerning the phenomenon of apparent genetic recombinations in bacteria, rather than assuming this now to be a fact. It is probably this which accounts for his abruptness.

Probably your best bet for the "philosopher-biologist" is Norman Horowitz. He is apt to be rather severe with tentatively-advanced hypotheses, but I think he would be fair and patient, if not sympathetic. I mentioned your statements to him, and he said he'd be glad to hear from you, although, to quote him, he would not be happy to enter into an extended philosophical exchange. I would give it a try, at any rate. I might say that outside of Beadle himself, the man I most admire in the Neurosporology set-up is H.K. Mitchell.. However, I must say that I don't think he'd be particularly competent to deal with the philosophical aspects of your Lactose- situation.

I dropped in to quiz Sterling Emerson regarding the OP mutant that his wife has obtained. He said that there is as yet very little information on it, except some general growth studies of this mutant compared with wild type at various concentration of substrate. It seems to be a single gene difference which renders the mutant unable to grow in media of the normal osmotic concentration. It does, however, grow vigorously in more dilute media. Nothing of its physiology has as yet been worked out.

Our studies on growth and differentiation are proceeding along some quite promising pathways. We are now concentrating on ~~the~~ an attempt to understand the physiology of plant response to minute quantities of light (such as are involved in photoperiodic responses, prevention of etiolation, etc.) So far we have found the following: Etiolated pea material can convert tryptophane to nicotinic acid (via kynurenine and hydroxyanthranilic acid) provided that it receives some fairly intense illumination. In the absence

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of light, the tryptophane does not get converted to nicotinic acid; rather it shows up as indoleacetic acid. This probably explains why roots result when you feed tryptophane to pea epicotyls in the dark (an IAA effect), and why no roots result, but rather bud growth occurs in the light (a nicotinic effect). We have also found that adenine will act as a bud growth factor in peas, substituting quite well for nicotinic... Nicotinic seems to be involved in adenine accumulation somehow, because feeding nicotinic to illuminated plants always results in adenine accumulation, but the reverse is not true.

We have also discovered that light will alter the responsiveness of plant cells to auxin, even if the auxin levels within the cells are not at all altered. We are now trying to get some further evidence on this question... we'd like to know what metabolic system is involved in this response..... Another interesting finding is that peas are evidently just like the temperature-sensitive adenine mutant of Neurospora. They grow beautifully at 30°C, but don't grow at all (under our complicated conditions) at 35. If, however, you add 5 gammas per cc of adenine, they grow beautifully at 35. This is a good hot lead which we are pushing hard now... The temp. response may help to shed some ~~light~~ revealing data on the light-response.

We have also been dabbling with anti-auxins, because of our belief that low auxin levels are necessary for photoinduction in short-day plants. So far, the most promising ones seem to be (a) 2,4-dichloroanisole (the decarboxylation product of 2,4-D), and (b) a naturally occurring anti-auxin in Xanthium leaves, which I believe may have something to do with florigen. Incidentally, I'm surprised at your statement that you hadn't heard triiodobenzoic acid mentioned before.... I just send you a reprint of an article I published on this subject!

You could really do some good spying for me if you wanted to.. At the Chicago meetings last Christmas, it was rumored around that R.H. Robert, of the Hort. Department at Wisconsin had been able to get an extract from leaves which would cause vegetative plants to flower. The implication was, of course, that he had succeeded in getting florigen out of the plant. Up to now, I have heard no further information on this rumor. Is there any way you could scout out the truth and send it on to me? I'd be very grateful if you could.

Things are going pretty well on the home-front. The big news is that the Galstons are "expecting" to complete their family circle about the middle of October. So far, Dale looks and feels fine.

Please send our very best regards to Esther.... and to our friends in Botany.

Sincerely,

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