

handwritten transcript

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Dear Josh:

In my old age I'd just about forgotten I'd written you about the biographical piece on Ed. Yes, I favor a joint effort and because you knew Ed better than I and have a much better recollection of events than I, you should be the senior author.

I read your recollection notes with much interest for they give me a much clearer view of role in the whole remarkable story.

Perhaps you know well just how Ed + I got together and how Neurospora came to play such a significant role. Ephrussi and I were convinced from the Drosophila eye transplants that it was highly probable that genes controlled chem. reactions in a one-to-one relation of genes and enzymes. After our joint efforts at Paris (where Monod was a frequent visitor) Boris continued in Paris and I at Harvard for a year (collab. with Thimann) then at Stanford with Ed. I had tried to get one of one of Hans Clarke's students but with three jobs he didn't want to come for less total income. Next I tried for Wachtendonk (sp?) and almost got him. For some lucky reason he didn't come with me but worked with C V Taylor at Stanford. Then I heard of Tatum's availability and we tried to work out the eye-color chemistry.

Neurospora came about in the following way. Ed. was given a lecture course in biochem in the Chem Dept + I sat in on the lectures. When he described Nils Fries's nutritional studies of filamentous fungi, it came to me that Neurospora [which I'd first learned about at Cornell when Dodge came to give a seminar not knowing about 2nd div seg. of gene pairs in meiosis. As grad students we enlightened him.] might well be the organism to try by inducing mutations for nutritional requirements. You know the rest - luckily biotin concentrates had just become available commercially.

Sometime along the Neurospora work, I remembered the paper by John Gowen in which he reported attempts to see if he could detect recombination in appropriate genetic mutants (organism just now escapes my recollection). I suggested to Ed that this would be an excellent opportunity to see if with nutritional mutants recombination could be detected. It was my thought that for Ed's future it would be desirable that he have a line of inquiry of which he was the major investigator.

As you know, the first mutants were isolated by Gray, a MA grad student. You of course know the story far better than I. I don't know how many attempts

[Marginal note]

Gray made to detect recombination. I recollect few or none.

I enclose a reprint of a little recollection for the 1974 Ann. Rev Biochem that recounts much of this including the visit of Ed's father who reprimanded me for getting Ed in a position where he was neither geneticist nor biochemist and thus had no visible academic future. My attempts to reassure him were quite unsuccessful.

In our non-scientific personal relations Ed and I were never really close, far less so than Norm Horowitz + I.

For personal material on Ed, I should think Howard would be a good source.

Regards

Beets

P.S. I'm writing Corner that we can't make the yearbook in press but hope to do the next one.