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Oct 17 1946

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 know 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 Thomian) then at Stanford with Ed. I had tried to get one of one of Hines Clarke's students but with those jobs he didn't want to come for less total income. Next I tried for Wackenhack (sp?) and almost got him. For some lucky reason he didn't come with me but worked with CV Taylor at Stanford. Then

I heard of Tatum's availability and was
 tried to work with the eye-color chemistry.

Neurospora came about in the following
 way. Ed. was giving 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 might well be the
 organism to try by inducing mutations for
 nutritional requirements. You know the mesh-
 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 the 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 MIT grad student

which Ed
 first
 learned
 about at
 Cornell
 when
 Dodge
 came
 to give
 a seminar
 with me
 about 2nd
 year seg. of
 gene pairs
 in meiosis.
 As grad
 student,
 we enlightened
 him.

You of course know the story far better than I. I don't know how many attempts 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 & 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

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