

December 15, 1951

Dear Josh,

The ~~s/~~ story (ugh!) will be along shortly - it's still being checked by H K Mitchell. It's not genetic enuf for Genetics I think so J Bact might be the best place.

What I want to discuss with you is the possibility of working in your group next year. I mentioned in the previous letter that I did not think I'd stay here another year. There are definite reasons that I don't want to go farther of course. It's just that the interaction of Vandy and Nelly don't work out:

- (1) It is the expressed policy of VU (from the highest source, not departmental) that VU should strive to be a shining example of the liberal arts college and not emulate MIT, CalTech, Columbia, etc., so-called, and I quote: "technological and professional universities...with weak undergraduate colleges..." Ha! I haven't seen an undergrad here that carry a CalTech undergrad's books. With this in mind you see that it would be difficult to establish any sort of physical set-up for experimental work in microorganismal genetics. Classical biology - taxonomy - and ecology are OK. Of course I could get outside funds - there is apparently little in the U regardless of the campaign promises.
- (2) The atmosphere is hardly stimulating. Of course there are some good men here. H. C. Bold is an interesting chap (incidentally Moewus sent his strains and they are alive!) and turns out research, taxonomic. Wolf the fungologist you know - he's doing some research - inoculates minimal medium plus amino acids and vitamins with isolated spores to detect biochem mutants (no UV, etc.) - so far he hasn't gotten any. Iida McVeigh you know. Another gal is an ecologist. There is a vert morph and embryol who does no work that I can see and another new man - a chigger taxonomist. Deacon, the head, is a pleasant and interesting fellow but doesn't work either. Only two of the grad students know what's going on. The faculty spends most of its time discussing courses, students' grades, and every so often Delbruck (in the form of an Orwellian hate session).
- (3) I am the physiologist. However the ex-physiologist may come back. No one knows. That means that I may stay and teach bacteriology to engineers and nurses, Enough said.

However I've been sufficiently politic enough not to let my opinions show so I get along with everybody.

At the present I am doing a little bit of research - to the limits of time and material equipment: :

- (a) If bacterial cells fuse in syngamy the resultant zygote might (just might) be phage sensitive. Therefore, by crossing K12/2 X K12/4 and adding marked T2 and T4 to get multiple infection we would have the possibility of recombination of phage occurring within recombining bacteria. Why? Phage recombinants, if recovered, would therefore indicate the probable fusion of the bacterial cells. There are technical difficulties - I know.
- (b) The time of synapsis of the bacterial chromosomes might be pinned down by UV mixtures of syngaming cells. The UV will play havoc with cross over frequencies, if we may extrapolate from Drosophila.

I still haven't given up the idea of looking for a catalaseless or rather, catalaseintermediate (with a screwed catalase molecule, enzymatically speaking) M. lysodeikticus (catalase being crystallizable and 2% of the dry weight of the cell). The only trouble is the screening medium - some attempts have failed but I have other ideas.

Have you tried to cross diploids?

Lastly several s- types when intercrossed throw s/ progeny in part. I hope to find the s/ differentiating from the zygote by the use of TPT under a decent microscope. But I agree with you - the cytological demonstration is nice but not necessary. What happened to those slides of heterozygotes with 4 Robinow bodies?

I have one other iron in the fire. The Air Force has an operations research group and they wish me to apply for a position as an Intelligence Specialist (that's sticking their necks out). The pay is tremendous but (a) I don't know what they want me to do and they won't tell me either because it's all classified or else they don't know, and (b) there is the none too considerable problem of one's conscience as Weapons Maker.

Research from Cal Tech is slim. Leg Lieb writes nothing about her research except that my mutant hunt method is too much work.

There is no news from the rest. I may call Atwood when I drive east for an interview with the Air Force.

The Papers book is a marvelous idea. Let's hope it can be profitably repeated every 5 years.

Sincerely,

Tom Nelson

You have been identified by two of my genetics class as being "a German geneticist" and a third thinks you are a German working with eugenics. Of course this is the same group that think Muller formulated the chromosomal theory of heredity and that Trofim Lysenko worked out the genetics of Neurospora.