Transcript of letter from Robinow

New York City
January 12, 1958

Dear Joshua,

Many thanks for your welcome letter which caught me with one foot already on the train to N. Y. I am pleased to hear that you find mitosis in fungi worth investigating. Mitosis in growing hyphae of Neurospora crassa has been studied by my former student Dr. A. Bakerspigel who has a paper about it coming out soon in the A. J. Botany. The story is peculiar but its oddity is of the same order as that of the strange Euglena "mitosis" which Leedale described last year in Nature and probably not sufficiently exotic to rouse the interest of geneticists in general. As I remember it the course of events runs something like this:

The bouquet stage is much more chromosome-like than anything one ever sees in the Mucorales but it is still not possible to say precisely what it means. Experiences with Allomyces, of which I have a stack of Feulgen pictures with me, suggest to me that in the bouquet phase homologous chromosomes are paired. Presumably they divide afterwards but if they do B's preparations are not yet transparent enough to show this.

Allomyces has excellent countable chromosomes but again there is a very peculiar form of division. More like that in Euglena than like mitosis in the bean root. And of course none of it is visible in conventional iron hematoxylin slides.

In Schizophyllum, a basidiomycete one would think of as more sophistocated than Neurospora, B. has found that the division of the nuclei, which are comfortably large, is brutally direct. Thus:

One more point that might interest you is that the nuclei in the phialides (sterigmata) of Penicillium, which are quite reasonably large, do not even possess a nucleolus. They look like so many balls of chromatinic wool and they too divide by simple constriction. One sister nucleus going into the youngest conidium. - I find these observations fascinating because of the possible relationship between fungi and protozoa which they suggest, but I fear this information is not important to geneticists.

The puzzle of the behaviour of the chromatin bodies during fertilisation is constantly before me. I find nothing wrong with the photograph(s?) which you had in the J. Bact. except that it does not suggest to me that the nuclei participate in fertilisation. With suitable Hfr strains it ought surely be possible to obtain enough material for looking into this. It might be worth while to cause a contraction of the nuclei first with chloromycetin. If fertilisation still occurs under these conditions

participation of the chromatin bodies would be more or less ruled out.

I think it more likely, and see faint evidence of it in your photographs, that the small "centrioles" slip across and I hope you will keep a look out for this.

The "mitochondrial equivalents" of W. Niklowitz, Zbl. Bakt. I. Orig. 73, 12 (1958) might in reality be the same thing as the centrioles of the Giemsa and Feulgen slides. Their position fits and this idea would also explain why union is end to side. In other words, is there something like a micronucleus?

Wyile we are about it, can you tell me what happens to the chromatin of the donor cell after copulation? I realize that they may be hard to identify but perhaps the recipients could be lysed away by phage at the right moment, thus tagging the donors.

I should of course very much like to talk these matters over with you by the side of a laboratory bench. Perhaps we shall penetrate sufficiently far west next summer to make it possible. I have a talk to give in Ann Arbor in July.

With best wishes to both of you for the progress of your work and your general happiness in new surroundings

Sincerely yours,

Carl R.