

August 23, 1944

Dear Burr;

Received your letter about Hollander. I of course would welcome the opportunity to take part in a 'gene-cytoplasm' symposium. My ignorance can't be very much more profound than the other participants. However, if as you point out the geneticists are down on Lindegren, the situation has elements of embarrassment. ~~ix~~ Do you know of the detailed reasons for this attitude, i.e. beside the more obvious ones? In any case I am writing a letter to Hollander giving him some of the results we have obtained and telling him I would be definitely interested in participating.

I have been up to my ears in experiments trying to clean up the gene-cytoplasm thing with adaptation to melibiose fermentation. The last few backcrosses are coming through today and tomorrow. If they come through as expected we will drop it and publish what we have. It is financially impossible even for Busch to continue a more detailed analysis of the problem using melibiose as the substrate. We have two other strains which act the same way towards maltose and lactose. In addition to economy these strains have the advantage that one can adapt to maltose fermentation ~~which is the other way around~~ but not to lactose whereas the other one can adapt to lactose but not to maltose.

The story we have on melibiose has turned out to be very interesting. In the absence of the substrate during sporulation and copulation there is simple mendelian inheritance of a two gene character, both dominant and each situated on one of the two available chromosomes. In about 20 backcrosses to the double recessive we have always obtained a 1:1 ratio. If however the matings and sporulations are carried out in the presence of melibiose, after packing the cytoplasm full of the adaptive enzyme by continued growth on the sugar, the 1:1 ratio blows up in the segregants of the backcross.

Carl and I went up to Columbia and spent a weekend with Stadler talking over the data. He is convinced about the cytoplasmic effect even on our original data. Funny thing, on the basis of the talk you and I had on your front porch I reanalysed the whole thing and wrote it down in great detail and calculated the expectancy of various ratios and was convinced before I went to Stadler's that we had a simple Mendelian inheritance. I plugged that viewpoint during our conversations but the cytoplasm had caught their fancy. Then we got home I persuaded Jerry to start the whole business over again using high concentrations of melibiose in all the sporulation and copulation media and then the story started to come out. Stadler is more than willing to accept the paper for publication in the Proc. Unfortunately we didn't get a chance to talk about Stadler's corn material and Stadler made me promise to come up again in late September to give me an earful because he 'likes the way I think' (shades of Stalker's 401!). Anyhow coming from Stadler it sure made me feel good because I consider him one of clearest thinkers in biology that we have and so I can't help boasting to you a little about it.

I am finished with Bact. teaching for the next 6 weeks and with Bronfenbrenner's temporary blessing I am spending full time on the hill. The question of the locale of my research in the fall has not come to a head altho he has expressed his decided preference for the med school. I am letting that sleeping lie until I am surer of what will happen next year. Concerning this here is something interesting. I think I told you that Anderson mentioned to me during our

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desultory conversations that one of the big obstacles to the projected arrangement might very well turn out to be Woodson, 'who dislikes physiologists and won't have much if anything to do with them'. Well yesterday the botanical mountain announced that he was coming over to see the physiological Mohammed (no direct comparison intended). Woodson apparently is interested in using cartesian methods in studying variation in leaf morphology and wants some pointer on how to proceed. He is coming over this afternoon to talk it over. This may be something in the nature of a test. I don't know : will tell you more about afterwards.

After 15 consecutive days of temperatures above the thermal deathpoints of all but the most degenerate of living organisms the weather has turned mild (between 85 and 90). Our damned weather man persisted in issuing 'continued Warm' bulletins up to the 14th day when he finally broke down and called it hot. The Spiegemans weathered the weather by sticking close to our remarkable apartment. Willard is developing a devilish sense of humor. He spends a good deal of his time carefully observing his poor parents and hilariously imitating anything that seems funny to him. And the damndest things appeal to his humor. We have to be particularly careful in our pronunciation and if in our reading to him we make a slip or substitute a word he slaps his thigh and laughs and repeats the mistake using our intonation and this can go on and on.

Well I better bring this to a close. I am planning to go to Minnesota the second week in September. Viisscher through John asked me to try to come up then as he would like to see me and would not be in town after the second week. I am not planning to go to Cleveland after all.

Everything is running smoothly around the department. Victor is working pretty hard at the incubator course and they are getting a lot out of it. Jacobsen tells me that they are seriously thinking of making this into a permanent arrangement to be given in the Junior year of the premed work. Anderson has left after being laid up with boils very awkwardly situated. Stalker substituted for the last two genetic lectures. You no doubt have already heard of Roy's new job as instructor in Willier's department. He leaves in a few weeks for Baltimore.

Regards to everybody

If you run across Harriet take her aside and explain carefully that a crack in an agar plate does not have morphological significance for the identification of the organism growing in it.