

3-17-58

Momentary reactions to site situation.

The sudden fact is the termination of the Berkeley negotiations. This came by surprise, but we were not as disappointed or as startled as might be thought. I never had felt quite easy that the administration there would or could support an expensive program merely for the purposes of the Genetics Department, and there was always overhanging their own sense of uncertainty on the role and future of the dept. There were increasing troubles about what could be done about lab. facilities that would still preserve the opportunity of lab. contact with Stent et al. The red tape experience was already getting to be rather terrifying: it was boiling down to a serious question whether we would prefer to leave Madison when the time came for a decision. But there was a nearly overriding factor: summer climate which makes me useless and frustrated here. Other possible solutions to this: ? here-- aircondition the laboratories?

In any case, since Art Kornberg's first overtures about 6 weeks ago we have both been much keener on that proposition though it seemed inherently less likely. This is still full of uncertainty. When Art first approached, he thought that Kaplan and he had a megabuck in their pocket; though this has evaporated, he seems to think it may still be possible. But I have not heard from him since I spent a day with him at Chicago airport (Feb. 5).

3-19 Stanford did not seem so terribly attractive in the Christmas week, 1956 that we visited-- our first offer. At that time, it was quite uncertain whether the Biochemistry chair would be offered to Kornberg and I had every reason to doubt whether there would be enough strength in microbiology to train my graduate students. Berkeley was appealing on the general promise of more DNA biochemistry there, though in fact this boiled down to Stent and Schachman ± some odds and ends. The reputation of the Virus Lab. was none too encouraging. However, if Genetics had been able to set up a proper lab and status for me, it would still have seemed an inescapable opportunity at Berkeley. But the picture Art K. painted was much brighter. There would be the opportunity of close collaboration along precisely the lines of my deficiencies and interests; and the little extra scope of a medical genetics department appeals to me rather more than a genetics department still unsure of itself and its commitment to Agriculture/ Esther's position at Berkeley would probably be more attractive financially; it is hard to say whether it would confer more status. The housing situation was problematical: it covers a wider range than the PA opportunities, and it is at least conceivable that we could have gotten something superb and still not too far away. But this was uncertain, and we might have had to be contented with something not so satisfactory. PA probably appeals to E better on account of some possibility of driving there; I probably would prefer to live in Berkeley: if we could find the right place. On the other hand the academic opportunity at Stanford is probably better now, and in any case the issue is solved: if Berkeley or its Genetics department can't raise the ante, there is no point speculating further. The very hesitation and uncertainty were cause for concern-- validly as it turns out. It may be easy to rationalize--and I suppose it is not pleasant to be repudiated, if one wanted to put it that way, but I don't think Esther or I really feels badly about it, though it might be very different if Stanford hadn't come back into the picture. If that proves not to pan out either, at least we will have been let off in easy stages. As best I can judge now, I do not think that Berkeley's pending offer materially affected our decisions about Harvard or Stanford--I, though we would probably have searched more carefully into these.

Anyhow, the table is cleared now except for Stanford-II vs. staying here, and this is in itself something of a partial relief. This past year has been rather unproductive scientifically: protoplasts stopped paying off after a point; we were away at least half the time and sadly interrupted; the Med Gen Symposium has bitten hard into my time as did some of the organization issues (esp. Atwood) for the department. But I think the uncertainties about our future, still unresolved, and having lasted now for 15 months have something to do with it too! I badly have to concentrate on coli crossing again:

it has been difficult to maintain a position on the problems here without some perspective on the Jacobian experiments, and we have simply never gotten around to these. I am looking forward to Lucas's arrival tonight, and hopes of a fresh start. Probably we share this feeling about coli crossing.

I have a nasty backlog of paperwork: the Harvey lecture and papers with Iino to write very soon, and some piddlings like an abstract, and finishing up 'cosmic microbiology, which has been very instructive for me (to look into geochemistry for example) but again distracting from research. Hemophilus has been a nuisance lately, and Jackie misses my close direction. Just as well I don't have too many students this spring. Anyhow, I did get off a revision on Gus Nossal's draft on the antibody formation by single cells.

Mainada. problem: how to deal with Atwood. I've almost given up hope of getting him here, but have to try and at least get a clean cutoff. He's an impossible correspondent.