

March 2, 1949

Dear Martin,

The primary purpose of this letter is to find out whether and when you are going to San Francisco for the A. C. S. meeting. If you are going, let me know the dates, since I am at present arranging to go there somewhere within the week of the 21st. It would be nice if we could either go together or at least meet there.

As you probably know by now, I had quite an interlude a couple of weeks ago, first with Mike, and hard on the heels of his departure came Beka. I managed to survive, but didn't get too much work done during that period.

Now a little bit of news. I have officially received the appointment of Associate Professor in the Department of Bacteriology, so it is okay for you to spread the word. This apparently, however, is not a stable point in my meteoric rise to success, since I have just been offered a full professorship at the University of Illinois. I am going down there next week to look the place over. The new head of bacteriology there is Doctor Halvorson, who as you know, is from here. He is one of the smartest and nicest guys I have met in quite a while, so the probability is that although they will probably meet the Illinois offer here, I will undoubtedly decide to move. I shall let you know definitely in about another week or two when the thing is settled. Actually, I may not make up my mind until I return from California, since Hamner at U. C. L. A. wants me to come out there and set up a Division of Microbiology in the Department of Botany, but I do not think that U. C. L. A. can come anywhere near the salary and working conditions that Illinois is in a position to offer.

I am very anxious to take Guest along if I go to Illinois. I have already discussed this with Halvorson, and he is in favor of it. I am writing Howard a separate letter about the setup, and I hope he has not as yet committed himself anywhere else.

Our work here is really going along full blast now. We returned to the old eighteen hour day schedule and are doing experiments like mad. This "long-term" adaptation stuff of Winge's is turning out to be a very fascinating phenomenon, and one which he completely missed the true significance of. We are using it in quite a few different ways, and one of them is as an assay procedure for adaptin preparations,

as well as of the inhibitor, which is always isolated in the first crude extraction. It has turned out to be an infinitely superior assay method both from the point of view of ease and sensitivity. We are now, as far as I am concerned, for the first time in a position where we can begin to make meaningful stabs at correlating the chemistry of our fraction with its biological activity. Meroy is at present in a manic phase since some of his experiments with the pink yeast have just crashed through with a bang and it looks as if that story may finally yield us the kind of data we would be willing to stick our necks out on. There are several other things, but we will, I hope, shortly have the opportunity to talk face to face, so I will save some.

Please give my fond regards to Ronnie and Reba. I find it difficult to think of anybody else.

Sincerely yours,

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