

October 2, 1954

Dear Francis:

Thank you for sending me the mss. I will read them as soon as I have the clear time and send them back to you. Unfortunately, it is too late for the particular purpose for which I originally asked to see them, as proofs have already been corrected for the two review articles.

We had an interesting and restful summer also, despite having been chased home from Woods Hole by "Carol". We did not manage to make contact with the Cavallis in New York on account of the disruption in communications on the Cape, but they did join us here and helped us move into the house we had just purchased. I doubt if we will ever completely "move in".

I have wanted to talk to you about the Affaire Moewus, which did create quite a stir at Woods Hole, beginning just about the time we arrived. Apparently there had been little critical discussion of any sort until at one evening seminar a Dr. Grell (from Hartmann's lab at Tübingen) got up to express the extreme scepticism that had developed there since their own attempts to repeat the sex hormone experiments. Grell referred to mss, of two papers in press, which he let us see. In endeavoring to read them, Esther thought she might as well do a proper translation, a copy of which is enclosed for your own interest. Moewus made some reply about the necessity of adhering closely to specified culture conditions in order to repeat the results.

You probably will get as many opinions and impressions as people you ask about the matter. In New York you can probably get a full account from Ruth Sager and Bernie Davis. However, like myself, they may feel some reticence about bringing up such a messy subject with you unless you take the initiative and ask them to speak frankly with you. I have to report to you what I have heard from several others that your own position appears to be highly ambiguous. On the one hand you are made out as Moewus' sponsor and willing to take the responsibility for it, especially as he appears to be continuing his work at Columbia under your aegis with no comment from you to date; alternatively, others have the impression that they should not go into the story themselves since you yourself must have the same intention as the basis for bringing him to New York. And to tell you the truth, I have not been able to suggest what I thought your own position was on either count (and suspect you may not have definitely decided yourself). But one way or another, I am afraid you have let yourself in for the unpleasant necessity of reaching your own responsible conclusions and not concealing them.

After Grell had quoted the criticisms from Tübingen, Ephrussi and I and several others thought it should be a simple matter simply to ask Moewus to confound the criticisms by making a demonstration. We had the impression that he had been using the materials in his course and would have no difficulty in it, and I can assure you our aim was to try to clear the air and not to embarrass him. Ephrussi and I were not so very confident of our background on Moewus' work, and this is why we wanted to consult with several interested people before even raising the matter with him, so we would have some notion of what would be a

worthwhile and convincing demonstration. ¹But before we were able to arrange this, another incident came up. Ruth does not want to be quoted on this herself, but the story is available from independent sources; a) the fellow who was with her, and b) Moewus' own admission as to the facts if not intent. Anyhow, they had gone to see Moewus for a demonstration of the ~~flag~~ cro⁻ mutant, supposed to be non-flagellated, and its restoration by crocin. On first examination, the stock proved to be quite motile. The possibility of reversion would be quite acceptable, of course, but this is not what was raised. Instead, he suggested making up a preparation in another sample of distilled water which he was more confident would be free of contaminating bacteria or crocin. This he did, and so successfully demonstrated a non-motile preparation, except that they were noticed to be dead. He later admitted to Tracy that he had made up this preparation not in water but in dilute iodine solution "in order to visualize the flagella". It turns out he has several unlabelled solutions on his bench, some distilled water, some visibly indistinguishable iodine. One could argue all day about fraudulent intent (or paranoid self-delusion) but we came to the conclusion that after this there would always be a residual doubt after any demonstration, and that conviction would only follow an independent, self-critical confirmation by someone (like yourself) of unquestioned reliability and critical insight. The stories were not concealed from Moewus and 'Phruss' and Tracy put the matter to him in just these terms. I am willing, pro tem, to hear his excuses for perhaps another year, but if by then he has not succeeded in making someone else repeat his basic findings (the ~~flag~~ series of sex hormones and the different sexual mutants), not many people are going to waste their thought on it. But I agree that if you are going into the matter at all, as I most urgently hope for your own sake, ~~as~~ as well as to gratify my curiosity, you will have to work on the premise that everything is ok, or will be when the bugs are straightened out. The confirmation will just about have to come from you so that he can show you how to make the experiments work. I don't think you can afford to let the matter pass. Among many of the people in other fields than genetics, at Woods Hole, there was the feeling that he had at least your implicit endorsement; on the other hand you will hardly be able to say anything except from your personal experience. After what has already happened, you will, unfortunately, have to be very wary of anything that has not passed exclusively through your own hands.

I have not gone into any of these questions of reliability with him, at least not directly, though I hope I made clear the necessity of independent confirmations. After the above, Moewus asked me if I would not like to see his material and responded affirmatively, almost enthusiastically to the suggestion of a demonstration. I asked him what he thought he could show, and he suggested the ~~gathe~~ mutants + wild type filtrates from the two mating types. He did not seem to balk when Tracy and I suggested a coded experiment, but he never did get around to setting it up. I won't deny that conditions at Woods Hole may not be optimal. What's fishy though is the seeming clarity and simplicity of the experiments according ~~to~~ to the papers (and the very bulk of them) in contrast to the way in which virtually every demonstration or confirmation has failed, albeit subject to some excuse.

I may not have been of much use as a first hand source and I am distressed to see one of my friends put on the spot in such a way. If you can get in touch with Harold Bold, Tracy or especially with Ephrussi you may be able to get more substantial impressions. Boris will be at Harvard most of the rest of this year, as you probably know.

Best wishes,

Joshua Lederberg