

October 16, 1952

Dear Tracy:

I was very disappointed to read your letter, especially as it had a tone of discouragement beyond the entanglements of the book. You must realize that I could not possibly undertake the role that you suggest. Your statements about our relative facility at writing and familiarity with the field as a whole simply are not correct. Ten years from now, I might consider such an imprudence, but now it is out of the question. May I hope that your present lack of enthusiasm is temporary, growing out of distractions that will pass? I have simply no interest in an association, or a private effort, that did not enlist your initiative and collaboration.

Might we consider the following proposal: the book might be regarded as tripartite, consisting of 1) a general topical treatment which would be a more intimate joint effort, 2) a taxonomic survey of protozoa and thallophytes and 3) the same for bacteria and viruses. You have already outlined part 2) at least in a general way for your course outlines, so you are way ahead of me. I will work on 3) in a relatively casual way for the time being while we leave the whole question in abeyance. This will not be lost motion for me. There is a good deal of historical work I have to digest and organize, and this will not be superannuated by new research. If you would like to revive the collaboration, I will accelerate my own end of it. If worse came to worst, the three sections could be issued separately in the first edition. We could not do very well with 1) until 2) and 3) were in fairly definite form. I will try to avoid writing commitments, for silly reviews and such, and save such sporadic energies for this job. (This wouldn't be a bad idea, book or not!)

Mr. Freeman is paying a visit in a couple of weeks. I never have clearly understood what compacts you may have made with them. I like their way of doing business, not to mention their books, but have never gotten down to brass tacks with them. Nor do I intend to. He is bound to ask about our project, and I probably would want to tell him. If it's all right with you, can the arrangement of the previous paragraph be told to him? Of course, I will have to refer him to you for any definite statements.

Proofs just came in for the "Cell genetics and hereditary symbiosis". I wish I could have thought about the whole business a good deal longer. It was an education for me, but it may be a crime to inflict such a mass of over-implicit arguments and half-analysed conjectures on the public. Serves them right for asking me.

Sincerely,

Dr. T. M. Sonneborn
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Joshua Lederberg