

# INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF ZOOLOGY

October 28, 1952

Dr. Joshua Lederberg  
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Dear Joshua:

Thanks for your very kind reply to my letter. I was very discouraged when I wrote the letter, but only with respect to writing, not anything else. I seem to require all of my time and energy to keep my group going, to keep myself informed about what I need to know at the moment, and to keep up my research, my teaching, and my various other duties. Writing always gets pushed aside. My own research papers are so far behind, that I get more and more discouraged with ever getting anything written. I write very slowly and revise repeatedly. When I realize how difficult it is for me to write a single paper, I shudder at the idea of a book. Moreover, I feel very much as you do, that ten years from now I might have a sufficient grasp of the field I wish to see included in the book, that I could undertake to write it. But I do not feel up to it at present.

The three parts of the book you suggest would certainly be called for. However, it was my impression that, after our discussions, we had both agreed we wanted to write a cellular genetics rather than a genetics of microorganism. I see no room, in the outline you propose, for discussion of cellular genetics in materials other than microorganisms. Perhaps this was an oversight on your part, or you intended to relegate all such material to the general section. I am not nearly sufficiently well versed in those subjects to undertake writing on them at the present time.

Something else now comes to mind. When I originally thought of collaboration as a division of labor, this seemed to me like the only present feasible arrangement, in view of the big gaps in my own knowledge. However, when I try to visualize how things would proceed, if we ever reached the time when our respective portions were written, then I suspect that we would both want to be able to go critically and constructively over what each other had written. What this really means is that we both must in effect be fairly familiar with the whole field. The book then would be a real joint effort all the way through. In any case, I have to become familiar with all the subjects in order to present them to my class. This spring, for example, I shall have to go in detail into the genetics of viruses and bacteria, ground over which I have travelled only to a very limited degree in past courses.

It is not that I want to take this part out of your hands, but that I must master this field and I should be able to make suggestions

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and evaluate conclusions in the part that you are going to write. I would hope that you would give the same to me, in the parts that I would write.

Now if this is the kind of thing you have in mind too, and I suspect you do, then I don't see how either one of us can undertake this book until we both are really much better acquainted with the subjects than either one admits to being at present.

The most I could do now, if I had the time to do it, would be to write a preliminary draft of the material covered by your subject No. 2, that is, on the protozoa and thallophytes, with preliminary notes, perhaps, as to some of the general questions to be dealt with in what you proposed as section No. 1. I presume you would mean for the latter to come near the end of the book rather than at the beginning.

As I write this letter to you, I feel again the urge and desire to write, but I do not see when. I don't feel that I can put off much longer the technical papers which are so long overdue from my laboratory. There are really many of them which should have been published long ago and haven't yet been brought to light. My students all complain bitterly about the fact that so much of their own work follows from and depends upon results of mine, which have remained unpublished for many years. They have had access to my data, of course, and they know what I have; but they can't refer to it anywhere except as "Sonneborn unpublished." This not only embarrasses me, but really causes some trouble, because until the data are thoroughly worked out and in publication, one can never be quite sure what will be specifically said about the data. This is a heavy obligation on me and I am trying to do something about it, in the little time available, from week to week. Also I have had on my mind now for several years the obligation of writing for the National Academy a biography of Jennings. This weighs me down. I want to do a good job of it and yet it means lots of study and reading in his diaries, his letters, publications and the general field of science in his day.

Well, I have just really been talking out loud to you to give you some idea of the difficulties I face with regard to the possibility of any immediate progress on the book. By all means talk with Mr. Freeman as freely as you wish when he visits you. I have a note that he will be coming here early in November, and he will undoubtedly pump me. I don't know what he'll drive me to say, but I do know that I cannot give him any commitments as to time. I would like to do what I can on this book and I would like to do it with you, but that is about as much as I can say at the present. I don't want to keep you from going ahead alone (or with some help from me) if you can do the job sooner.

I wish I could have answered your letter otherwise, but I just can't and still be realistic.

As always, my very warmest personal regards to you and Esther and to the Skaar's and my other friends at Wisconsin.

Cordially yours,

  
T. M. Sonneborn

TMS:jb