

March 16, 1953

Dear Ralph:

Thank you for your letter of the 11th. Had I known you could be provoked into thinking about these questions, I would have done either of two things a) sent you the ms. beforehand, to help avoid error, or b) thrown some more in (purposely) for piquance.

Seriously, I think you will agree that the whole subject is quite murky, (and I am rather out of my depth in the phycological aspects of it), but that some kind of generalization has been needed in order to help organize more interest in it. I trust my mistakes will not have led anyone seriously astray. You will have to keep in mind also that I have had to quote many assertions at their own value, without necessarily forming my own definite conclusions (e.g. your #1).

As to 2) phagotrophy is not the full answer; it does show how an endosymbiont can get inside the outer limiting wall. I can appeal only to the reestablishment of the chlorella-Paramecium bursaria symbiosis as, perhaps, supporting this kind of view.

3): I do not disagree. The presentation of these as likely symbionts, following Pascher, seemed the line of least resistance; it is by no means unqualified. I am interested to hear you have actually worked with Cyanophora. Do you have this in culture? Is it easy enough material to handle to warrant using it in this kind of study?

4) My slip! What would you have recommended as a correct term for my obvious intent?—microflora doesn't sound quite right.

5) I didn't invent these usages of "chromogenic"—both are reasonably well established— but it hadn't occurred to me that I had used both. (Cf. J. Bact. 60:381; JBC 170:391; Lindegren, CSH 1946). Of course, the nouns are chromogen and chromogens, respectively, so there should be less trouble with these.

6) Englesberg and Davis are, if not ischnotrophic, irrepressible, while Horowitz is, at least, prototrophic.

Sincerely,

Joshua Lederberg