

What you think properly, to read the proof before sending them to me. I would be by far the best and I have much time. Then, do that. In case of important correction, I should write to you, should I?

Now about your queries on *Sphagnum*. I can not be very precise. I have no copy of my mss. of *Sphagnum*. All the documents were sent to James who began the Manual. I should have corrected it but I did not think I had time, on the whole worth no demand for the printer.

S. fimbriatum should be described and I think I had described it.

S. compactum of the Yosemite valley Bolander is *S. rugosum* var. *compactum*.

S. mollicornis, Oregon, Humboldt? I do not know that. Where have you seen it?

S. subcundum, Marjor. has been *S. Loderi* Heller. Mon. U.S. but *pedunculatum* is *S. subcundum* again. That species ^{*S. Loderi*} having been crossed by Heller and in his *Scenes*. p. 15.

Juncus *S. cribrum* Hook. & var. of *S. macrophyllum* and will describe it (it is not already described so?) as a var. *filoides* Auct. and quote *S. cribrum* as syn. *Sord.*

I have still another new species *S. Fitzgeraldii* named by two French Bryologists one of them Pensacola made a good match for me and also a good description. The place is after *S. Loderi*. I send that as a specimen. As perhaps it will be in time for the printer. Very truly and thankfully yours, S. Loderi

Cambridge, July 31st 83.

Prof. J. Watson. Cambridge.

My dear sir.

Your very valuable letter of the 17th is received and for it, I give you my best thanks. It gives me some information and directions which I was anxious to get. I should have come to you (in letters) already many times, but I ^{feared} to trouble you and to intrude, taking for my own advantage your precious time for reading my long letters and answering them.

What you say about the beginning of the first page of the Manual is all satisfactory. I shall not correct again the No. of the Genera nor make any change to their relative position now; only replace in the text the descriptions of the Tribes which I cut out. I really think that these descriptions are valuable for the student. Short descriptions of the tribes with also names and very short descriptions of the genera of each tribe will be also good as a kind of key in the Compendium. I could not in any way find fault with your arrangement and distribution of Tribes and Genera in the Bot. Calif. On the contrary, I wrote to Dr. Gray that it would not be done better.

and as you will see my own systematic arrangement differs little from yours. I have a few more tribes. But it is quite a matter of course, as a large number of genera of the manual, some of them necessitating a separation under peculiar tribes as not found in the California mones. — You say that the English botanists do not mention tribes, and other divisions in the text. That is true essentially for Hooker, at least in his *Synopsis filicum*, a work which for me is very dry and of difficult access by the necessity of running over it, sent by the author from a division to another, from a genus also to another, untill I can find what I need to know. For contra the Bryologist. Wilson and especially Muller have carefully exposed their systematic arrangement, the by a compendium at the beginning and then, for Muller, at least by repeated description of the tribes and of the genera in the text, even with the arrangement and short diagnosis of the species after each genus. Briethwaite is a disciple of Lindberg and follows it from the beginning, as far as he has gone untill now, at least. I have already written to you about it. Should we admit the modification proposed by Lindb, or admit the oldest known genera, what

should we have? You can see that in his museum dinariz is *Systemate novo naturalis* etc. where *Schizophyllum* for *Tridens*, *Astrophyllum* for *Mnium*, *Licharospheum* for *Subcomnium*, *Leersia* for *Encalypta*, *Mollia* for *Trichotomum* and *Amothecium*, *Isotria*, *Ditrichum* adopted by Prather. That revolutionary system may be good according to some opinions. But I am too old and therefore too conservative to admit it, though so that I should find as many reasons to admit that of Muller, or of Muller which also present some difference. The more one studies the mones the more he finds the impossibility to arrange them in some consistent natural system and also the difficulty of fixing precise generic divisions or rather of knowing which of the names given by different authors are the oldest or the most right. Hence I think best to stay with Schimper, Sullivant, Wilson, Muller and other authors whom I have followed for half a century. I have no time to make a new acquaintance and I truly believe that the American Bryologist will support my opinion in the matter. At least they have followed Schimper and Sullivant untill now, as you have done yourself. I do not say this to depreciate the works of Lindberg whom I have in high esteem and who is certainly one of the best bryologists now living.



Lesquereux, Léo. 1883. "Lesquereux, Léo July 31, 1883 [to S. Watson]." *Leo Lesquereux letters to Asa Gray*

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