

in return for the specimens, & as some acknowledgment also of a handsome sum of money I had sent him (nominally to pay for postage & for his various publications sent by mail, but in fact far exceeding, as meant to, any such outlays) let me know it, before I put the species. Instead of that, in a short time comes a proof-sheet of a new memoir of his in the *Annuaire*, in which my ms. determinations are made to figure in comparison with his own & also criticised to such an extent, that one of my species is expanded into six, & another old species, as I understand it, into five, making eleven in all. This surprised me, but I knew his way, & that I had not guarded against this use of the specimens — & so, in responding, was very careful not to say much (writing the letter twice in short) & expressly to add that I had no right to blame him though it wd have been more agreeable had he waited till I had published the species.

His reply is exceedingly harsh, as if I had injured him beyond forgiveness: but as he added that he had "tribulations assez grandes pour que des nouvelles ne puissent m'être qu'excessivement décourageantes" I felt sure he must have trials, & have tried to make him feel better; even telling him of your kind interest & willingness to attract the attention of European friends to the importance of his work, in

which, I added, I should be glad, if anything opened, to join (mentioning Hooker and Thacker) and thank you for sending it to me. I had I thought, when I filled upon the vacant hour with the note to you I should have taken a little more time and satisfied myself at least, a little better. I am not surprised nor disappointed at Dr. Hooker's decision. I can look at the scientific work that the *Savon* in question now does as any real advantage to publish. His papers look well, and have spared of labour, and it is the labour of a man who ought to be more learned in what he writes on than any body else. But the new species (most many of them) are abandoned by the author himself as soon as he has

33 Charles St.
Boston
16 Feb 1864

I return the note of Dr. Hooker and thank you for sending it to me. I had I thought, when I filled upon the vacant hour with the note to you I should have taken a little more time and satisfied myself at least, a little better. I am not surprised nor disappointed at Dr. Hooker's decision. I can look at the scientific work that the *Savon* in question now does as any real advantage to publish. His papers look well, and have spared of labour, and it is the labour of a man who ought to be more learned in what he writes on than any body else. But the new species (most many of them) are abandoned by the author himself as soon as he has

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finished his brief description; or at least as soon as he arrives at a chance to revise them, in a new memoir; while old species suffer in a like way from various sorts of intermeddling with them, and changes upon changes of the Nylanderian idea. His first book was the resume of the French Lichens, & was an able criticism of The Lichen-Flora of Middle & Western Europe — marked indeed with his present defects of manner as regards the opinions of other writers — and a rather extravagant and hasty reduction^{or destruction}. He has since taken for species (as at p. 109 where 10 species of Koerber's Syst., are, in a note, and without discussion pronounced "names" of one species, and at p. 116, where 5 more with an "etc." are, in like manner, "denominations" of another — a good part of which^{15,} ~~x~~ ^{is} known to me, are now recognized, and not a few really doubtful ones added to them by the author himself) but, as a whole, I think, an interesting and instructive

application of modern views to the old system, which he appears to seek, not, as the sporologists to destroy, but only to reform. It was this tendency as I regarded it (it is important to say that the authors refused their credit, & claimed that of a founder of a new system, based on the Spermatogones & their contents, a way in which I never accepted his work) which made me a willing learner of all he wd teach me: so that, at last, he has come (in his Bot. Journal article, lashing me for differing from him) to claim me as a follower, which I never was, any more than of others.

But excuse this, which an awkward
cold makes, I fear, unpardonably dull.
Let me get add one more little narrative.
Having occasion, last autumn, to get the
remains of the determinations of Graph-
ides of the N. Pacif. Exped., which Mr.
H. had agreed to make — I sent a
small packet to him & included all
my new Lecideas of Cuba: with the
hope that if I had ^{made} mistakes, he would,



Tuckerman, Edward. 1864. "Tuckerman, Edward Feb. 16, 1864." *Edward Tuckerman letters to Asa Gray*

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