

***Interactive comment on* “Biochemical and structural controls on the decomposition dynamics of boreal upland forest moss tissues” by Michael Philben et al.**

Anonymous Referee #2

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This is an interesting and novel paper that tries to dig into the reasons behind the relatively well documented low decomposition rates of bryophytes that has a huge impact on biogeochemical cycles in the boreal, which as the authors point out, is frequently not taken into account. I think the question that this paper is addressing is important and novel.

I have a few concerns about the paper however that in my opinion would have to be addressed before it should be published. 1) The bryophyte species were not included as a variable in this test, but they did vary between regions. Bryophyte species, even beyond the true moss/sphagnum split, are far from being homogeneous. I suspect that

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many of the differences found between the two regions has to do with the different species that were included in the mesocosmes. This point is not addressed anywhere in the text. There is considerable literature showing that the nature of the decomposing matter is one if not the most important factor in determining decomposition rate (e.g. Lang et al. 2009 Journal of Ecology). Unfortunately the latin names of almost all the species are misspelled. I feel that including acknowledging this factor and including the associated literature will strengthen this paper considerably. 2) The methods are not clearly enough described. In the annotated manuscript I have highlighted several places where more details are needed to clearly understand the methodology - mostly in the field aspects. Similarly, I am uncertain about the use of the Philben et al. 2006 approach as "green moss" from a stream is taken as equivalent as a variety of mosses from boreal forests. Can more justification be provided? 3) The results could be more clearly presented. I am uncomfortable with a table made up only of p values. It would be much better to have F values and N for the different tests. There also seems to be a contradiction between the table (effect of temperature on mass remaining), the figures (not really) and the text (there was none). Also the figures were not always clear as information was lacking from the legends. I do wonder if all of the figures are required, perhaps Fig 6 could be an annex?

In conclusion I think this is an interesting paper with a lot of potential. With a little refinement I think it could have a lot of impact.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-290/bg-2018-290-RC2-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-290>, 2018.

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