Response to interactive comments of Reviewer 2 (bg-2018-290)

We thank reviewer 2 for helpful comments. Our responses to specific comments (reprinted in bold) are given below.

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 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 litterature 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 mispelled. I feel that including acknowldging this factor and including the associated litterature will strengthen this paper considerably.

We acknowledge that the study prevents the separation of effects of different moss species vs. regional effects due to the differences in moss species between sites. However, the main conclusions of the paper (low decomposition and Q10, little change in chemical composition or physical structure) arise from similarities between the two sites. The observation of these similarities despite contrasting climate, moss species, and N availability strengthens these conclusions. This will be clarified in the discussion of the revised manuscript.

The main difference we observe between the sites was in N dynamics, including changes in %N remaining, C:N, and amino acids. We maintain that these differences most likely arise from higher moss N concentrations at GC than SR because the changes are consistent with differences in N availability. The differences in moss N concentrations are likely due to site differences in N availability rather than species-specific differences because N concentrations of balsam fir needles follow the same pattern as the moss tissues (Ziegler et al. 2017). We will expand the section on differences in N dynamics between the site to acknowledge the possibility of moss species effects, including additional citations.

The spelling of the Latin names will be corrected in the revised manuscript

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.

The details highlighted in the annotated manuscript will be clarified in the revision.

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?

“Green mosses” in Philben et al. 2016 refer to the green portion of upland boreal forest moss tissues, separated from the underlying brown portion which was reported separately. The mosses in Philben et al. 2016 were collected from the same two forest sites as the present study and the same set of dominant species are represented. This will be clarified in the revised manuscript

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 contrediction between the table (effect of temperature on mass remaining), the figures (not really) and the texte (there was none).

Table 2 will be revised to include F values and N for each test.

The effect of temperature on mass remaining is significant, as indicated by table 2. The text in the results section (page 6 lines 14-23) also indicates that mass loss and Q10 were significantly higher in the 18°C incubations. Statistics and a reference to Table 2 will be added to these statements for clarity.

Discussion of a small temperature effect is based on low Q10 compared to vascular plant decomposition, despite a significant difference in mass loss between temperature treatments. This will be clarified in the revised manuscript.

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 annexe?

We prefer to keep Fig. 6 in the main text because it clearly illustrates the lack of change in bulk C composition during incubation, which is an important conclusion but not demonstrated in the other figures.

A legend will be added to Figure 6 and Figure 8 for clarity.

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.