

## ***Responses to reviewer***

### **Changing sub-Arctic tundra vegetation upon permafrost degradation: impact on foliar mineral element cycling**

I would like to congratulate the authors on the improved revised manuscript. Readability has benefitted tremendously from restructuring, especially in Results and Discussions section. Well done!

We thank the reviewer Jonathan von Oppen for his second careful reading of the document and helpful comments.

Conciseness also more concise now, though I see some potential to condense even more (e.g., in the very extensive study site description, or the summary (“Overall, ...”) after the summary (“In summary, ...”) in the two lower paragraphs on p. 13). I have included some additional suggestions for changes below, but would be happy to see the manuscript published after consideration of these points.

We have addressed this comment by revising and simplifying the study site description (section 2.2) and the discussion summary (L290).

#### **Materials and Methods**

L141/146: is the number of samples equal to the number of sampled leaves per species? I suggest to explain what exactly was sampled (as in L142f). For example, if  $n = 69$  is the total number of samples at the Gradient, Fig. 2c wouldn't make sense as it has more levels (81).

We have clarified the description of the foliar sampling at the experimental CiPEHR site (L141) and the Gradient site (L150) to specify that “The foliar sampling was performed as one bulk foliar sample per plant species per plot”.

Moreover, numbers indicated on the Fig.2a-b-c have been adapted for a better understanding.

L176ff: the  $\boxplus$  symbol is normally used for differences, not standard deviations, so I find its use here a bit confusing-.

The symbol  $\Delta$  has been replaced by the symbol  $\sigma$  according to the comment of the reviewer (L180 and L196).

L205: which variables were log-transformed?

Variables that required log-transformation are indicated in the Suppl. Mat (Table S.5). We have specified this in the text (L206).

L207ff: including model formulas could help with clarity.

This has been included (L217).

## Results/Discussion

L225f: the statement “potentially induced by...” is repeated in the following paragraph. It seems more meaningful there, so I suggest to remove it here.

We agree. The first repetition has been removed.

L248f: I don't think it's necessary to cite the same reference twice within this sentence and suggest to remove the first instance.

We agree. The first instance has been removed.

Fig. 5/6: I think the captions of the two figures are interchanged. Further, both would benefit from a more detailed description of what they are displaying in the first sentence, e.g. “foliar fluxes across treatments” (Fig. 5) vs. “between treatments” (Fig. 6).

We agree. This has been corrected now. We also added a more detailed description as suggested (here, and for the similar figures Fig. 3-4-5-6).

L. 481: “The wide shrubification across the Arctic is expected to increase by as much as 52% by 2050 (Pearson et al., 2013).” This is a very vague statement. For instance, what baseline is that increase compared to? Are you referring to increases in shrub cover and/or biomass? I would advise to carefully formulate such general statements to still keep them precise (and thus impactful).

We understand this was not specific enough. We have specified that the increasing shrubification by 2050 was referring to the shrub cover. This has been included L495-496: “The shrub expansion across the Arctic is an important and widely observed response of high latitude ecosystems to rapid climate warming, and the woody shrub cover is projected to increase by as much as 52% by 2050 (Pearson et al., 2013)”

L. 483: to my understanding, Mod and Luoto (2016) neither discuss albedo nor soil surface effects of shrubification. Consider removing this reference.

We agree. References have been adapted. This has been included L.497-498: “In addition to the multiple implications on surrounding ecosystems (e.g., changing the Arctic tundra albedo and soil surface roughness: Chapin et al., 2005; Sturm et al., 2001a; Weintraub and Schimel, 2005)”.

## Conclusion

L528ff: I suggest to make it clear that conclusions (i) and two (ii) refer to the community level, in contrast to the initial concluding statement at species level.

We thank the reviewer for this great suggestion: we have brought that precision in the conclusions. This has been included L542-545: “Our results at the species level showed that sedges (i.e., *E. vaginatum* and *C. bigelowii*) have relatively high Si, P, and Fe foliar concentrations, whereas shrubs (*V. uliginosum*, *B. nana*,

*V. vitis-idaea*, and *R. tomentosum*) have relatively high Ca and Mn foliar concentrations. As a consequence, the main conclusions for the plant community are: (...)" (L.542-545)

L548ff: I find the last two sentences a bit confusing. After only discussing micronutrients throughout the discussion, this reference to carbon cycling distracts from the main take-home message of the paper. If you want to include this point, I suggest to move it to the previous section (Implications for vegetation shifts).

We agree with this suggestion and the two sentences have been moved earlier in the discussion (section 3.4) (L.488-491).

Additional remark:

I would like to reiterate my suggestion of phrasing references to degradation stages at the Gradient site as "Minimal thaw area" instead of just the "Minimal area" (e.g. L519), and likewise for the other two degradation levels, to improve clarity for the readers.

This has been modified throughout the document.