

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Ramona J. Heim et al.

Author comment on "Fire in lichen-rich subarctic tundra changes carbon and nitrogen cycling between ecosystem compartments but has minor effects on stocks" by Ramona J. Heim et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-277-AC1>, 2022

We thank the Reviewer for the helpful and constructive comments which will help to greatly improve our manuscript. We are prepared to incorporate all raised points as suggested below and are confident that we can meet and successfully address all issues.

Anonymous Referee #1

This study presents an analysis of post-fire carbon (C) and nitrogen (N) dynamics using a chronosequence of tundra ecosystems in western Siberia. Results show minimal effects of fire on above and belowground nutrient pools, with the primary effect related to lichen declines and bryophyte increases post-fire. The research is well designed with appropriate analyses, and the manuscript makes an important contribution to the increasingly important topic of tundra fires for a region that is not well documented in the English language literature. Several aspects related to the framing and interpretation of the work may help to improve manuscript. Congratulations overall on a nice study.

Thank you!

Framing this work as contradictory to Mack et al (2011) doesn't seem entirely correct to me and raises a couple of important issues to consider. The paper by Mack estimates combustion losses one year post fire by using proxies to reconstruct the organic soil layer. In the absence of such data, the present study has a slightly less complete picture of soil C losses.

This is true. We will remove the text part that presents our results as contradictory to Mack et al.. Now we relate our results to Mack et al. by referring to the different time frames of the two studies. While Mack et al. 2011 is mainly about the combustion loss, we investigated the long-term fire consequences on C stocks. Therefore, we will now state right at the start of our manuscript that we do not look at combustion losses, but the long-term consequences. In the first sentence we will for example change "effects" to "long-term effects" and will maintain this phrasing throughout the manuscript.

The data provide good information on changes in C concentration, but I wonder if the depth increments are comparable. For example, could it be the case that 0-5cm in the burnt sites corresponds to 10-15cm in the unburned sites?

In contrast to Mack et al. we worked on rather fine-grained mineral soil without significant organic layers that could combust, which is typical for the dry and lichen-rich tundra type. A thick organic layer with accumulated litter, as described in Mack et al. 2011, was not present in our sites. Instead, the mineral soil was covered by living lichen and moss in unburned sites, lying directly on the mineral layer.

The 0-5 cm layer in our study is the upper layer of the mineral soil, which was in unburned sites found directly under the lichen layer. The mineral horizon was always clearly identifiable and could not be misinterpreted.

In a freshly burned site, which we also visited, the organic layer (lichen, moss and roots) was not entirely burned. But this likely corresponds to the rather small fire scar size in this case.

To clarify, we furthermore do no longer use the term "total stocks" but instead refer to soil and vegetation stocks.

These things are OK, but some more details and nuanced discussion would help. For example, what are the soils like at these sites, is there a well developed organic layer?

We will add to the discussion in l. 197:

"Thick organic soil layers, as e.g. reported for a site in Alaska (21.5 cm; Mack et al. 2011) may store particularly large amounts of C and N that are potentially lost if burnt. However, much shallower organic layers have been reported for North-Eastern Siberia (6.3 cm, Loranty et al. 2014) and were found in our study area (4.3 cm). Therefore, potential losses upon burning can be expected to be much lower, and, moreover, the importance of the more stable mineral soil carbon stock less susceptible to losses from burning relatively increases. However, a direct comparison of these studies is not possible, as we investigated the system during regeneration and not directly after combustion"

Would such combustion losses be possible, or are soils a less important fuel source in these systems?

In the subarctic dry tundra, as studied here, we would assume potential combustion losses to be generally smaller, as the soil organic layer is thin, and carbon stored in the mineral soil is much more stable. We discussed this point as shown in the answer to the next comment.

Additionally, regeneration was possible in all sites, in our study. What we show are the difference between burned and unburned sites after different times of recovery, this means new vegetation and hence a regeneration of the carbon stocks.

Note that Loranty et al (2014) report organic soil depth in this context, and that Jones et al (2013) also document organic soil accumulation after fire. If these sites have different soils it may be worth highlighting this and considering what this means for geographic variability in tundra fire impacts on biogeochemical cycling.

Good idea! We discussed the impact of geographical variability in soils on the biogeochemical cycling as follows after line 189:

"Our results support recent findings of He et al. 2021, who claimed that soil organic carbon loss through tundra fires may be overestimated because the soil

organic layer in many tundra areas is much lower (see also our findings) than in the area of the extreme Anaktuvuk River Fire with predominant histosols.

In addition to the thick organic layer which usually is combusted by fire(He et al. 2021) moist tundra ecosystems typically store much higher amounts of carbon per unit area because of a higher soil organic matter content in comparison to dry tundra ecosystems (Marion et al. 1997). Through increased temperature sensitive decomposition after fire (de Baets et al. 2016) burning can thus indirectly lead to higher decomposition rates and ecosystem carbon loss in moist tundra ecosystems. Depending on the tundra type, fire impacts can thus have varying degrees of fire impacts on biogeochemical cycling.”

Related to these points, the results or discussion don't really mention depth differences for the soils, particularly regarding the ^{13}C results. Could there be differences related to belowground biomass (root) dynamics? Alternatively, could differences in permafrost thaw depths or temperatures explain any of these differences?

We discussed our observation that ^{13}C differed between the fire scars and the unburned control plots in the topsoil layer in section 4.1.2 of the discussion. We reasoned that these differences may be related to differences in the decomposition rate caused by higher soil temperatures at the fire sites, not as a direct result of the fire, but an indirect due to the thinner vegetation layer shielding the soil surface less from sunlight. Since temperature decreased rapidly with depth, primarily the topsoil layer was affected. However, we do not think that this finding helps explaining the differences between the dry Tundra ecosystem in our study and the wet Tundra in Alaska.

Here again I think some site specific context would be helpful - could results from your other studies at these sites (e.g. soil temperature or thaw depth) help interpret these results?

We will add means of active layer depth and soil temperature for each fire scar and control plots to the study site description in lines 75ff.

A map and/or photos of the study sites could be helpful as well.

We will add a detailed image of the study site with the fire scars to the study site description.

This may be more personal preference, but I think the structure of the manuscript could be improved. Subheadings seem to be used instead of paragraph breaks. The Introduction should be broken into several paragraphs to help highlight main aspects of the topic. Conversely, there are places where the subheadings seem excessive - for example section 3.2 could be a paragraph or two.

We agree that this was not ideal. We introduced more paragraphs in the Introduction section, as proposed by your comments below, and we reduced the number of subheadings in the Results section.

Presenting these results in a bit more detail and narratively linking them can help provide a more comprehensive overview for the reader.

Thank you we agree and will revise the results and discussion in the revision focussing on improving the narrative.

L30: Perhaps start a new paragraph when switching from C to N.

Will be inserted!

L40: New paragraph?

Will be inserted!

L186: This discussion begins to address some of my points above. Note the study by Loranty et al had ~10cm organic soil layer relative to ~21cm reported by Mack et al., and in both cases the boundary between organic and mineral horizons is generally well defined. It would be interesting to know how the sites in this study compare, and whether differences in soil types between depth layers (i.e. 0-5cm, 5-30cm affect bulk density and nutrient concentrations.

We will add more detailed information regarding soil organic layer depth to the discussion (please see answer to comment above).

New references:

He, J., Chen, D., Jenkins, L., & Loboda, T. V. (2021). Impacts of wildfire and landscape factors on organic soil properties in Arctic tussock tundra. Environmental Research Letters, 16(8), 085004.

De Baets, S., Van de Weg, M. J., Lewis, R., Steinberg, N., Meersmans, J., Quine, T. A., ... & Hartley, I. P. (2016). Investigating the controls on soil organic matter decomposition in tussock tundra soil and permafrost after fire. Soil Biology and Biochemistry, 99, 108-116.