

## Authors' reply to RC #1 on bg-2021-227

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Author's reply to RC #1 on "Large Herbivores Affecting Permafrost – Impacts of Grazing on Permafrost Soil Carbon Storage in Northeastern Siberia" by Torben Windirsch et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-227>, 2021

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Authors' replies are formatted in blue.

This manuscript presents observations of soil cores from different landscape units and disturbance histories, and aims to answer an interesting and relevant question, "does grazing by large mammals impact permafrost carbon storage?" Unfortunately, the experimental design is fundamentally flawed, making any conclusions about the impact of herbivory on soil carbon storage impossible.

Thank you for acknowledging the importance of the research topic. We should initially have clarified that this was a pilot study. We will do so in the revised manuscript, and adjust the study's aim accordingly.

The main issue is lack of replication – the study relies on a single soil core for each combination of environment (drained lake basin or upland) and grazing (intensive or no grazing), which is insufficient given the variability of soil composition and the presence of confounding variables.

This lack of replications is a result of the pilot study character. Also because of logistical constraints we designed this study as small as possible. A random sampling design would be best, yes, and would be a next step. We aim with this study to identify differences between sites that could result from herbivory. This will be clarified.

We know that soil core properties are highly variable in permafrost environments due to cryoturbation, so any variation from one site to another could be due to natural spatial variability or the variable of interest, herbivory. Without replication within sites to account for spatial variability of permafrost soils there is no way to discern between those two possibilities.

We will adjust the paper and especially the discussion accordingly, stronger emphasizing that natural disturbances and variability are equally likely to cause the discovered differences. We will also include a discussion on the scalability of herbivory impacts

Additionally, soil moisture is a confounding variable that cannot be accounted for without additional samples in a wider range of environmental conditions. The authors showed that soil organic carbon varied with water/ice content and mentioned that the grazed sites in the drained lake basin flooded seasonally, while none of the other sites flood regularly. This means that patterns in soil organic carbon may be due primarily to variation in soil moisture rather than herbivory, because soil moisture and herbivory covary.

Thank you for clarification. However, the pattern of much higher carbon content in the active layer is consistent across two different landscape types, which hints on a process present in both landscape types. So there is a possibility that herbivory could be the driver here. Of course, we will clarify also the other influencing processes, and definitively soil moisture and hydrology are of utmost importance here.

Another potential confounding variable is the site history. The authors mentioned that the non-grazed drained lake site was cleared of forest a few years prior to the study while none of the other sites underwent the same treatment.

This clear-cut was done 4 years before the study, while the other sites did not feature any forest vegetation. We selected this site since changes from woody plants - although not trees but shrubs - towards grasses are associated with herbivore activity, making this site something like "ground zero" for vegetation succession with no animal activity yet.

While the underlying soil core data are sound and could be used to describe some of the variability of the site, the flawed study design makes it impossible to disentangle the effects of spatial heterogeneity, soil moisture regime, site history, and herbivory. Therefore, I suggest that this manuscript be rejected and the authors reconsider the scope of question that can be answered with these data for a new submission.

Thank you for this detailed comment on our study design. We agree that replication as well as random sampling would indeed provide more certainty.

We designed our study as a pilot and general proof-of-concept to look if the general idea of effects of herbivory on soil carbon storage is possible. We disagree that it is impossible to draw conclusions on the influence of herbivory with our dataset. For the revised version we will make this clearer and discuss the effects of spatial heterogeneity, soil moisture regime, and site history in more detail.

However, due to the consistency found in the differences in both active layer depth and organic carbon content between different grazing intensities across two landscape types, we would argue that herbivory - most likely combined with effects of hydrology - is still a likely explanation.