Author response to comment submitted by both reviewers to the revised version of this manuscript

In the document below, the reviewers' comments have been copied from the original reviews and are shown in black font, while the author comments have been added in blue.

Comments by Reviewer #1

General comments

I believe the changes made by the authors since the last version have substantially improved the manuscript, most importantly on the statistical analysis and the description of the study sites and rationale for their choices. I only have minor comments/suggestions that would further improve this manuscript prior to publication, which I detail below.

Thanks a lot for this very positive evaluation!

1. A related preprint on the Pleistocene Park is under discussion in the same journal (Windirsch et al., https://doi.org/10.5194/bg-2021-227) and the corresponding author is also involved in that study. For further comparisons, it would be good to mention how the GR site relates to the sites in that study (it seems to be fairly close to DB-IN?), and to discuss the apparent contradiction between the observed effects on thaw depth in the two studies. The distinct "control" sites are certainly responsible for the different findings, but since Windirsch et al. observe a thicker active layer in both lowland and upland sites in absence of large herbivores, some discussion of this would be of interest to the readers.

The work presented by Torben Windirsch and colleagues is indeed closely related to the work presented in our manuscript. Parts of the field work for both studies was carried out together, and both sides supported each other in carrying out measurements, and/or taking samples.

As correctly noted by the reviewer, the site DB-IN (in the revised version of Torben's manuscript re-labeled to B3) is indeed located very close to the GR sites where fluxes were measured for our study. The horizontal distance between the position of our chamber frames and their soil sampling spot was approximately 12 – 15m. Windirsch et al. measured a thaw depth of 38 cm at a single location and time for this site, while the values in this manuscript vary between 39 – 58 cm over three different sampling spots and a period of 10 days.

The seemingly different values for this sampling site itself can be explained by sampling time: Torben Windirsch analyzed this site a couple of days before our first measurements, while we were still setting up. Given the steep trends in thaw depths, e.g. reflected in Fig. 1b in this manuscript, and also the spatial variability as reflected by the 3 sampling spots used herein, the values given in both studies therefore correspond well with each other.

The differences in thaw depth between this site and the respective references in both papers can be explained by wetness levels. In Torben's study, the B3 site is labeled as a 'wet area of the thermokarst basin', while the 2 grazed references sites do not carry the 'wet' label. Accordingly, while the reference sites in the Windirsch study are actually drier than B3, in our study the references (UGR) are permanently water-logged, and therefore much wetter than the GR sites. The effects of soil moisture changes on heat capacity and heat conductivity are discussed in-depth in our Section 4.5 already. In the paper by Windirsch et al., the potential impact of wetness is already mentioned in their methods section 3.1: 'Also, flooding regime (seasonal or occasionally) is different between our sites and might have some effects on the soils.' Within their discussion Section 5.2, they then mention that soil moisture may have a profound impact on soil organic storage, and a co-existing influence of grazing and soil moisture should be investigated more closely in follow-up studies.

To reflect the correspondence between the two related studies, we changed the last part of the second paragraph of Section 4.5 on 'Grazing impacts on soil properties' in our manuscript. We removed the sentence 'Barring differences prior to the onset of the experiment, these studies suggest that the differences in soil properties between GR and UGR may be predominantly attributed to grazing pressure.', and replaced it with this new passage:

"The important role of soil moisture conditions is also highlighted in the results by Windirsch et al. (2021), who investigated places with different grazing pressure within the same thermokarst basin in Pleistocene Park where our GR sites were located. The drier locations showed a deeper thaw depth in their study, even though the grazing pressure at these sites was actually lower. In accordance with Windirsch et al. (2021), we therefore conclude that both grazing pressure and soil moisture differences hold the potential to substantially influence the soil properties, and their co-existing influence needs to be tested in further experiments."

2. The thermal conductivity of minerals is a possible explanation for the altered thermal regime, however the reference chosen to support this focuses on soils with less than 3% organic matter and states that results could be drastically different for more organic soils such as peat. Considering such data is not presented in this study, one can use the DB-IN data in Windirsch et al. Fig 2 as a close data source, which shows 10-25% C in the top 50 cm, which would amount to ~40% organic matter. The statement at L429-431 is therefore not well-supported by the provided reference and I would suggest modifying or removing this statement.

We agree with the reviewer that the cited statement in Section 4.5 needed rephrasing. The hint at the thermal conductivity of soil minerals, including the chosen reference, may have been interpreted that the mineral content had been increased as a consequence of grazing. As shown in the results by Windirsch, this is not the case, instead the input of fresh organic material actually seems to have led to very high carbon content in the top soil at the most intensively grazed sites. We therefore changed the last sentence of this paragraph, formerly L.429 – 431, to "When this peat layer is trampled by herbivores, as observed at GR, the

soil thermal regime may be significantly modified, with effects on both thermal conductivity and diffusivity.", and removed the reference.

3. My earlier comment regarding soil and air temperature was mistakenly attributed to L408-410 in the previous version of the manuscript. It referred to the fact that the deep soil temperature difference (35cm) was on par with the observed difference in air temperatures (one degree, now mentioned at L248-249). Unless the difference in air temperature can be attributed to an effect of the grazing, it likely reflects spatial variability and observed differences in soil temperature smaller than this variability may not be relevant. This is now clearer with the changes to Fig1 paneling/y-axis and does not necessarily need addressing in the text.

OK, thanks a lot for the clarification.

L185: "Calculations were conducted using R" Please provide the version of R used. In addition, this should be moved either to the beginning or to the end of the Methods section, considering that R packages are already mentioned in earlier paragraphs.

The R-software is first mentioned at the end of Section 2.2 when referring to the R-package 'LakeMetabolizer'. We therefore moved the reference to the R-software to this place, rephrasing to "Calculations with this statistical package, as also for the other R-packages listed below, were conducted using R Version 4.1.1 (RCoreTeam, 2021)."

Figure 2: Please refer to Table B3 in the legend when mentioning pairwise comparisons. In addition, please double-check the post-hoc significance letters: I did not check them all but for instance it seems that for Reco GR1 should not share a letter with GR-3 according to Table B3.

According to the reviewer's suggestion, the first part of the caption of Figure 2 was changed to "Overview on C-fluxes at all chamber plots from July 7th to July 21st. Differing letters indicate significant differences between plots ($p<0.01$, please see also Table B3 for details). ..". The typos in the significance letters shown in Figure 2 have been corrected.

Comments by Reviewer #2

General comments

In this revised version of the manuscript, the authors excellently managed to clarify their study design for comparing carbon emissions and uptake between grazed and ungrazed Arctic tundra sites.

Detail additions in both introduction and methods will help readers to understand the study's intention and limitations. These limitations are picked up again in the discussion, and discussed in sufficient detail.

The additional work put into graphical design improves readability of the graphs and understanding of the "read thread" drastically.

Thanks a lot for this very positive evaluation!

Specific comments

Please consider making the data accessible via a scientific data repository.

Upon publication of this study, we plan to publish the data on the Open Research Data Repository EDMONT operated by the Max Planck Society (https://edmond.mpdl.mpg.de/imeji/).

Technical comments

Line 90: There's a missing space between 15 and km.

The space has been added.

Table 2: There is still some inequality in spacing of the asterisks in the table description.

We changed the formatting, so the asterisks now look even.

Line 741: There's a typo in the reference to Myers-Smith et al., where the I in "Macias-Fauria, M." should not be capitalized.

The respective reference has been corrected accordingly.