This manuscript describes a study on the controlling factors of soil temperature and thaw depth for arctic tundra in Alaska, with a focus on the role of mosses. The study collected data on thickness and composition of moss layer (green/total), soil temperature profiles, soil water content, water table, thaw depth along two transects in a well-established research site. The study found that moss layer thickness, microtopography and soil moisture influence soil temperature profiles, and that near surface temperature controls thaw depth.

This study provides some insight as to how mosses and other environmental factors influence soil microclimate and can influence permafrost thaw. However, I feel the current manuscript in general lacks clarity. It is not clear to me what exact knowledge gap this study is trying to fill and how the analysis used is addressing this. I believe the manuscript can be much improved by looking more into how these different drivers interact and the mechanisms involved. I will give further details below.

We thank the referee for the detailed comments regarding how to improve our manuscript and what to consider before resubmission. We hope that, after addressing the comments of this and the other referee, our work and findings are communicated more clearly. In the revised manuscript we will more clearly specify the novelty of this work, and the unprecedented extent of field data collected for this study. A novelty in our results was highlighted by the reviewer as the lack of a species-specific effect on water retention seen in previous studies (e.g. Hrbáček et al. 2020). The methods and location of our study also provide a novelty. While some of the relationships explored in our study have been established in the past, they have not been studied in our region of study despite the heavy presence of moss nor have they collected datasets with data at a scale like ours. Studies such as Hrbáček et al., 2020; Gornall et al., 2007; and Porada et al., 2016 did not take separate measurements for the green and brown tissue (which our study can obtain by taking the difference between the total and green moss tissue thickness) but took one single measurement despite these two portions of the moss layer behaving differently and influencing active layer thaw in different ways. Our study also included a finer scale of moss mat thickness measurements which allowed us to observe breakpoints in our relationships which could not be explored with the limited measurements conducted in other studies. To our knowledge, past studies have depended on modeling with limited field data which have left gaps in our understanding of the full, quantifiable effect of the moss layer on active layer thaw. We will specify and better highlight these novelties in the revised manuscript.

The introduction can be improved by restructuring the current text and better introducing the exact research question this study is addressing. At the moment, the introduction is a bit repetitive as certain points are made multiple times and arguments that belong together are spread across paragraphs. I also struggled to identify what knowledge gap this study is trying to fill and how the study addresses this. From the introduction it seems like the role of mosses in influencing soil temperature and thaw depth is already well established. I believe this study could still provide additional insight by looking more into specific mechanisms (non-linearity of responses), and interactions between different factors and context dependency.

We will simplify the introduction to reduce repetition when preparing the manuscript for resubmission, and better specify the novelty of this study.

This study aimed to fill gaps in our understanding of the extent to which the moss layer reduces permafrost degradation. Our study aimed to accomplish this by providing a high-resolution set of quantitative data exploring previously understood but not thoroughly documented relationships. To our knowledge, previous research did not provide a dataset of the extent we presented here, and mostly focused on modelling with limited data for field validation.

As suggested by the reviewer, in the revised manuscript, we will specify mechanisms and interactions between different factors and better describe the complexity in these interactions.

The Methods section is generally clear but needs a couple of clarifications, see specific comments.

This will be addressed in the revised manuscript.

The statistical analysis and result section lacks clarity. Many predictor variables are used, but there is little reflection as to why certain variables are used. I personally also think that there are too many graphs in the results section, which adds to me losing track of what the authors really are trying to show.

This will be clarified when preparing the manuscript for resubmission. We will reduce the number of graphs and move some to supplementary information.

The authors mention the correlation between variables and that these are not used together in multiple regression analysis. However, they do not provide clear information as to which variables were included in multiple regression and why.

The variables selected for the multiple regression were those that did not present collinearity with each other. We will make sure to include a section that details what variables were selected and be clearer about why these were selected in the revised manuscript and include a table listing the variables collected.

In the analysis, a number of temperature parameters are used, both as predictor and response variables. However, there is little explanation as to why these different parameters are of importance or why they may have different drivers. This lack of context makes it hard to see the relevance of these different analysis and because of the many different temperature variables it becomes hard to follow and confusing.

We will make sure to add additional context to clarify the importance of the variables and why they were selected for the analyses in the revised manuscript.

The authors use deviation form mean elevation (dz) as a predictor variable, which indicates that there are raised plots and hollows which have distinct environmental conditions and also influence moss thickness. However, the interrelatedness does not really come forward until later.

We will make sure to discuss these concepts earlier in the revised manuscript.

I also wonder whether the moss dominance varies between hollows and raised plots?

We will test to see if there was a difference in moss dominance based on microtopography and report any significant differences in the revised manuscript.

Hrbáček et al. 2020 found species-specific effect moss on ground surface temperature and active layer depth due to differences in water retention capacity and structure. The authors find no difference in the influence of moss genus, which to me is an interesting point and could be further discussed. Are the dominant species in your site very similar in structure and water retention capacity or could your results be confounded by topography.

We will highlight these interesting results better and explore possible explanations for our findings in the revised manuscript.

In my opinion, the analysis needs to better substantiated and should better reflect the interrelatedness of these different factors as illustrated by the visual summary. A structural equation model may be a more appropriate analysis which can show these dependencies. In a structural equation model air temperature, PAR and net radiation could be used as general climate conditions that influence soil temperature. Soil temperature can further be modulated by soil moisture content and moss thickness, which in concert regulate thaw depth. The present analysis is not able to tease apart the effect of moss layer thickness from environmental conditions and the resulting effect on thaw depth.

We will test if structural equation modelling is able to better explain the results reported in this study.

The general readability of the manuscript can also be improved by splitting up long sentences.

This will be addressed in the revised manuscript.

Specific comments

Instead of a picture of the datalogger, a picture of the fiberglass probe would be more illustrative of the research method.

This will be added to the revised manuscript.

Line 110 Can the authors include specifics on the graminoid species present?

We will include these details in the revised manuscript. Our team performed this species identification in a previous study (Davidson et al., 2016; https://www.researchgate.net/publication/303712736_Vegetation_Type_Dominates_the_Spatial_Variability_in_CH4_Emissions_Across_Multiple_Arctic_Tundra_Landscapes).

Line 120 states that moss and soil temperature were measured from 1cm below the surface? What do the authors mean with below the surface? Does it include temperatures in the moss layer as is alluded to? Or is it only soil temperatures starting from soil surface? This will matter for the analysis.

Below the surface refers to any point below the green, photosynthetically active section of the moss layer and includes the organic soil layer composed of mineral soil and the brown, photosynthetically inactive section of the moss layer. We will specify this in the revised manuscript.

Can the authors more clearly state the frequency of measurements of the different variables.

Vegetation characteristics including the dominant genus, total moss layer thickness, and green layer thickness were collected once across the week of the 6th of July in 2021 (n = 124). During the week of the 8th of July in 2022, we conducted a random sampling (n = 20) of the plots to minimize the disturbance to the tundra. These random samples involved once again measuring the total moss layer thickness and green layer thickness and noting the dominant genus.

Belowground temperature data, thaw depth, water table level, and volumetric water content at the 124 plot points were collected on a weekly basis across both field seasons. Data was collected across 4 weeks in 2021 (July 8th – July 28th) and 4 weeks in 2022 (June 23rd – July 14th) for a total of 8 weeks of data. This will be clarified in the revised manuscript.

Where measurements done in a randomized manner? Otherwise, one can expect diurnal patterns to influence the soil temperatures measured at specific locations.

We attempted to collect these measurements consistently at similar times of the day. We will specify this in the revised manuscript.

I suggest using a correlation matrix to illustrate the correlation between various parameters and the significance. This will provide a much clearer overview than the current supplementary table.

We will include a correlation matrix in the revised manuscript.

Taken together I believe this manuscript needs extensive revisions to provide clarity by clearly defining research questions and how these are addressed. I also believe the authors need to rethink their analysis, so it better matches the inter-relatedness of their predictors. I therefore suggest the authors to thoroughly revise this manuscript before a potential resubmission.

We want to thank the referee once again for the detailed comments and hope that our revisions based on the comments provided will improve our manuscript's clarity. We feel that by including the reviewer's comments, the novelty of our study will be clearer, and considered appropriate for publication in Biogeosciences.