Our point-by-point replies (in black) to the question and comments by Dr. Reese (in blue).

The paper by Verdonen et al., is a study on degradation of palsas and permafrost plateaus over two sites in northwestern Finland, where Active Layer Thickness and ground elevation has been measured systematically and annually since 2007, and aerial photos exist from 1960 and onwards (including UAS images) to measure lateral degradation. Verdonen et al user linear regression to relate changes in palsa area and height changes to climatic parameters. This paper contributes to a better understanding of the changes in the sensitive palsa mires of Fennoscandia, and their association with climate variables. It's well written, and represents a further step in understanding palsa dynamics and associated influences, both climatic and otherwise.

We thank Heather Reese (Reviewer 1) for insightful comments and many good questions, and are grateful for her time and effort in providing valuable feedback. We believe that addressing the issues raised by Dr. Reese will improve our manuscript substantially and will add further insights not only to the permafrost and climate dynamics but also the methodologies used in the monitoring of palsas.

Main issues are marked with a *.

Questions and comments

1. Line 4 – It would be informative with a short form of the Latitude after the names.

We will add "68° N" after the site names as suggested.

2. Line 5 – I don't think that is true that your study focuses on the time period covered by your UAS data. It focuses just as much on your ALT measurements from 2007-2021 (albeit only within the non-degraded area of the palsas), as well as your RTK-GNSS ground elevation measurements, which to me is the more interesting data set since it should be more accurate. Even the next line in the abstract mentions using the longer time series of aerial photos. So I would take away this sentence ("The emphasis is on detailed change detection …").

Our initial idea was to focus more on detailed change detection during the last 6 years of the investigated period. However, the results of the ALT and RTK-GNSS measurements were more interesting and shifted the focus of this article. Therefore, we agree with this comment and will rephrase the beginning of the abstract so that there is no emphasis on the UAS data.

3. Line 8 – Mention that the ALT data are annual from 2007-2021.

Done.

4. L61 – name the years that your study is looking at, rather than using the more vague "the investigation period" phrasing.

Thanks to this comment we noticed that the investigation periods are presented only in the next paragraph. Therefore, we decided to rephrase the first research question to include the periods as follows: "(1) How did the lateral extent of palsas (1959–2021), palsas' height (2007–2021) and the active layer thickness (2007–2021) change during the investigation periods?"

5. L66 – It would be better if you indicated what kind of sensor you are using for data collection from the UAS platform. Is it an RGB camera, an NIR camera, and/or a Lidar sensor?

Thank you for the suggestion. Indeed, many different sensors have been used to collect data with drones at these study sites. However, here we used only the data collected using RGB-sensors. We will edit the text to mention it as follows:

"For the last six years (2016–2021), the UAS **RGB-**data have enabled capturing changes in palsas at ultra-high **(< 5 cm) spatial** resolution."

6. L91 – Describe the area of the two palsas in the same way and give their dimensions, as the shape of the palsa may affect how it reacts.

We will revise the descriptions of the two palsas so that their dimensions and particularly differences in shape and size are more clear. In the revised version, the dimensions of the Peera palsa will be described as follows:

"The palsa consists of a main body with a diameter of around 50 m, and the highest point of which was 2 m above the surrounding mire in 2021, and two lower "extensions", which increase the palsa's core-to-edge ratio. The area of palsa was around 1500 m^2 in 2021."

And the dimensions of the Laassaniemi palsa will be described as follows:

"The detailed investigation focuses on the southern palsa (hereafter 'Laassaniemi palsa'), which has over ten times smaller area (ca. 120 m^2) than the Peera palsa. The Laassaniemi palsa is more oval-shaped and it was around 18 m long and 7 m wide in 2021."

7. Figure 1 d and f – If these are 1 or 2 m high, are these palsas or peat plateaus? Or were these taller some decades ago? Just double checking, seeing as you made a point about the difference between the two.

We do not have information about the height of Peera palsa before the beginning of the monitoring in 2007, when its highest point was around 2.5 m above the surrounding mire surface. In Laassaniemi, the height of at least one of the palsa mounds was closer to 2 m based on a photograph taken in 1997, indicating that the whole palsa or peat plateau used to be higher than the ca. 1.5 m observed in 2007. Overall, it seems that all palsas in our study areas are disintegrated parts of larger peat plateaus.

8. *In general – I think it would be better if you used the terms Digital Terrain Models (DTMs) and Digital Surface Models (DSMs) instead of the umbrella term DEM, particularly since your article refers to both kinds of elevation models. Or at least conscious use of the terms. Your RTK-GNSS created DTMs while your drone images will create DSMs.

Thank you for pointing this out. Our reasoning to use only the umbrella term "DEM" was to avoid confusion by using too many similar terms. We acknowledge that the result was the opposite, and it is better to state clearly whether we refer to DTMs or DSMs. Therefore, we will change the RTK GNSS-based DEMs to DTMs throughout the text. We will also add information about UAS-based

DEM processing in the first paragraph of Section 3.2 and change UAS DEMs to UAS DSMs throughout the text.

9. *L120 – Do you mean that if there was lateral degradation in any year from 2007-2021 that you did not include this in the ALT measurements used in the regressions? If so, that should have some effect on your result (and maybe this is why you don't see strong relationships between ALT and climate parameters at the larger of the two palsas). Can you motivate your choice and make clear how using only the "Top of Palsa" mean ALT measurement can affect your results in the Discussion section. It seems like you would be missing the bigger changes. You can see the points you are missing when looking at Fig 2.

This is a very good point, and we agree that the effect of using only top-of-palsa (TOP) ALT values could have been motivated and discussed more in the manuscript. Only the values within the delineated TOP-areas were included in the calculation of the mean ALT and regression analyses. Our hypothesis was that we could better capture the relationship(s) between climatic parameters and annual variations in the ALT by excluding the areas most affected by lateral thermal fluxes. Preliminary analyses of all ALT values ≤ 1 m also showed very similar trends to the trends using only the TOP-values, which further justified focusing on only the most "stable" parts of the palsas.

To avoid speculations in the discussion regarding the effects of using only the TOP values, we ran the same regression analyses with the mean ALT values calculated from all ≤ 1 m measurements. The results showed similar temporal trends as TOP-ALT, but with a higher standard deviation. A comparison with climatic parameters showed less correlation with temperature-related parameters and better correlation with precipitation-related parameters when all ≤ 1 m values were used. As we want to refer to these results in the discussion, we decided to include the analyses of all ≤ 1 m values in the manuscript, either as part of the main text or as an appendix.

10. Section 3.2 – More details are needed on the sensors and specifications used to create these data. A Table could be useful here. What camera? What scale (or GSD- ground sampling distance) are the original images taken at? What full date? Which photo dates were the panchromatic, and what were the others? With the UAS, what platform (since this helps indicate which GPS was used)?

We compiled a table indicating the details of the aerial data used as suggested. This table will be added to the Supplementary materials.

While looking for more details about the aerial photographs we found a mistake in the acquisition year of the first photographs. The correct year is 1959, as opposed to 1960, which was indicated in the web platform where we accessed the data. We have corrected this throughout the manuscript and in the calculations of the annual area loss rates and the results of the regression analyses shown in Table A2.

11. *L145 – You listed a number of issues that you ran into. In addition to this, the UASbased data result in elevations that include vegetation heights (DSMs) and are therefore not completely reliable for showing accurate elevation from year to year, and therefore subsidence and volume changes over time. How tall is the vegetation on the palsas? In any case, this should be a primary reason why you can't calculate reliable volume changes from these data. I would reword this section so that this is acknowledged. However, the orthophotos are useful. It will be much better when you get the UAS-Lidar data for calculating volume changes!

Vegetation height varies from zero centimetres (bare peat surface) to ca. 50–60 cm. We agree that the effect of vegetation cover on the UAS-based heights and volumes should be addressed more in the manuscript. Therefore, in the revised version, we will add the uncertainties due to the vegetation cover along with the issues regarding georeferencing (see our reply to the next comment) to the description of the UAS DSMs processing. And yes, including UAS-LiDAR data will (hopefully) improve our ability to detect changes in palsa topography without vegetation issues.

12. L145 – I think you should also indicate how you geo-referenced your UAS data. You mention problems with the equipment.

    Since you have RTK-GNSS data taken annually, couldn't you calculate an RMSE for elevation of the UAS DSMs, indicating their potential error? Then again, that would mean you are comparing DTM and DSM. But still, you might be able to observe systematic errors across the UAS DSM. When you mention in results that you see a trend from southwest to northeast (Line 225), I wonder if it is due to a tilting of the UAS DSM, which can easily happen when good georeferencing isn't possible.

    My main point here is not to re-do a lot of work or invest a lot more time in the UAS DSMs, because frankly these will always include uncertainty due to 1) including all surface heights and 2) poor geolocation accuracy if not fixed with RTK-GPS control points. I think you just have to admit and realize the weaknesses of that data set for accurately measuring subsidence.

Reviewer 2 also raised the question about the georeferencing and processing of the UAS DSMs. In addition to the lack of consistency regarding the equipment and UAS data collection, the comparison of the UAS DSMs is further complicated by the lack of stable and fixed control points within the study areas. We will clarify this in the revised manuscript. As recommended, we calculated the RMSEs between the RTK-GNSS elevation values and the UAS DSM values for 2016 and 2021.

We have checked the DSMs of the Laassaniemi palsa for potential tilting, as suggested, and compared them to the RTK-GNSS-based DTMs from the same years. The subsidence in the Laassaniemi DTMs is also highest in the northeastern corner and lowest at the southwestern end. Therefore, we ruled out a tilting effect. However, in 2021, the surface of the Laassaniemi palsa was surprisingly higher based on the RTK-GNSS DTM than derived from UAS DSM. This difference, the RMSEs, and further comparison of the palsa changes based on the RTK-GNSS DTMs and UAS DSMs will be presented in new Section 4.5 of the revised manuscript where we compare the changes in the palsas based on the UAS DSMs and RTK-GNSS DTMs.

13. L165 – Again this is a DSM and not a DTM (or DEM), with vegetation included. Finland has a national Lidar scanning – why didn't you use the DTM from that for the snow model (or even better, both)? Too coarse? Can this account for the rather large differences between modelled and the reference snow depth measurements (10-30cm difference)? Also, where was the vegetation classification from? Your own? In any case, what classes were there?

Yes. The 2 m DEM (or DTM) from the National Land Survey of Finland is too coarse in this case, as the idea was to model the snow distribution with considering small-scale variations in the topography.

Vegetation classification of the Peera palsa mire used by A. Störmer (2020) in his Master's thesis was performed by another student (Tomhave, 2018), who mapped the vegetation using UAS orthomosaics and whose results were validated in the field. This classification was then adapted into the classes used in the SnowModel, in which there are predefined vegetation types with associated snow-holding capacities (see Table 1 in Liston and Elder, 2006). Following SnowModel vegetation types were used with the classes from the classification by Tomhave (2018) in the brackets:

- Erect Shrub Tundra (Dwarf Birch)
- Low Shrub Tundra (Dwarf Shrub)
- Prostrate Shrub Tundra (Lichen)
- Arctic Gram, Wetland (Sphagnum, Peatland vegetation)
- Bare (Bare rock, Peat)
- Water (Water body)

These same classes were also used to classify vegetation types in the Laassaniemi palsa mire by A. Störmer for the use in this work.

To further address this comment and another reviewer's questions regarding the SnowModel, we will include a more detailed description of the SnowModel, the parameters used, and a summary of the results from the thesis by A. Störmer in the Supplementary materials of the revised manuscript.

14. *L170 – I don't think the explanatory parameters are clearly given. A table could help here, or else you could more clearly state it in the text. For example, did you not test any precipitation variable, besides snow?

The explanatory parameters are introduced in the second paragraph of Section 3.3, where we briefly mention that precipitation was calculated for different seasons. In the revised manuscript, we will add more information about the parameters related to precipitation, including total precipitation, rain, snow, and rain during spring/early summer (May-June), summer (July-August) and autumn (September-October). We will also add a reference to Tables A1 and A2, where full lists of parameters used are given.

15. Fig 2 – I found it hard to see the outline of the palsa. Maybe a little thicker. Also, you should mention what your image is in the background of the 2021 images, and what date it was taken.

We have increased the line thickness of the palsa outlines and added dates of the UAS orthomosaics used as the background as suggested.

16. Fig 3 – Very nice information! This figure raises a lot of questions for me, such as What happened in 2012-2014 to cause this change in ALT?. Also, why the divergence in responses between the two palsa sites after 2014? I interpret the large error bars on Peera to indicate the faster degradation in process, likely due to the small size of the palsa, and the high edge-to-core ratio. Do you think the 2014 ALT measurement is correct for Peera? What causes it to be the biggest thaw measurement in case it is correct?

Thank you for these thought-provoking questions. We checked the data again. Almost all 2014 ALT values were higher at Peera than in 2013, despite the earlier measurement dates in 2014. Therefore, we are confident that there are no errors resulting in higher ALT values compared to other years. The top-of-palsa area is 309 m^2 at Peera and 49 m^2 at Laassaniemi. Larger area and more variability in the vegetation cover allows larger spatial variability in the ALT at Peera, which could explain longer "whiskers" in Figure 3. Based on the regression analyses, the mix of warm summers in 2013 and 2014 and early snow onset in the autumn of 2013 could be the reason for the deeper thaw in 2014. In addition, based on the regression analyses, the ALT at Laassaniemi is less sensitive to the mean summer air temperature or the number of particularly warm (> +15 °C) days, which could explain the lack of similar deep thaw in 2014. After 2014, the trends in the TOP ALTs of the two sites were rather similar (- 1.5 cm per year at Peera and -1.8 cm per year at Laassaniemi); thus, the divergence seems to have been caused by this much deeper thaw in 2014 at Peera than at Laassaniemi. In the revised manuscript, we will add information about the size of the TOP-areas and include a table in the Supplementary materials showing the measurement dates, mean ALT, and number of observations (all ≤ 1m values and within TOP).

17. Line 191 – Give the R2 value of the few mentioned correlated variables in the text.

Done.

18. Line 212 – I find this paragraph to be confusing due to the mix of observing what I interpret you to mean lateral degradation as well as subsidence. It would be good to be clear here. The heading is about subsidence or volume change with the RTK-GNSS and the top of the palsa measurements. Otherwise, did you use RTK-GNSS to map the area loss (lateral degradation)? It is unclear, due to the heading, and then the mix of different vaguely worded "degradations".

We will add a following clarification in the first sentence of Section 4.3: "Degradation of permafrost **in the form of palsa area and height loss** is noticeable at both palsas... ". We will also edit this section so that it is clearer that we analysed height, area and volume within the areas covered by the RTK-GNSS surveys, not only top-of-palsa.

19. A thought: Since you have measurements in both places, what is the relationship between the RTK-GNSS measured annual subsidence and annual change in ALT? You wouldn't expect (intuitively) to see a fluctuation in ALT at the same time as you have a constant loss of palsa height. Would be a very good figure to include, since you have the data. (OK, I see in the Discussion you mention this, and try to explain it).

A comparison of the annual changes in the top-of-palsa ALT and annual RTK-GNSS-based subsidence did not reveal any correlation between the two variables. In other words, the active layer can be thinner or thicker compared to previous year, regardless of the degree of subsidence. The relationship between subsidence and ALT, on the other hand, is more apparent, especially at Laassaniemi, where subsidence has slowed since 2012. At Peera, the temporal trend in subsidence is the opposite. Further analysis of the annual subsidence is beyond the scope of our article in its current version, but we could add a figure in the results showing the temporal changes in the mean height and depth of the active layer within the top-of-palsa areas.

20. Table 1 indicates that your volume change measurements using your DTM from RTK-GNSS is based only on the "Top of Palsa" area. Good to make sure that is clearly stated in the methods.

This was not our intention. Therefore, to avoid confusion, we will edit Table 1 caption as follows: "Palsa mean height, area, and volume changes in 2007–2021, **derived from the whole RTK-GNSS measurement area of the years in question.** Note that the RTK-GNSS surveys cover only the western half of the Peera palsa."

21. Line 220 – Include in the sentence that this is height change measured by the UAS DSMs. Also, are you measuring only the "Top of Palsa" area, or what area are you using? To try to figure that out, I read back in methods, where it sounds like you have used the 2016 extent of the palsa, as

delineated from the very detailed orthomosaic, so it will be I guess, a different area than “Top of Palsa”. Do I interpret that correctly?

Yes. The areas covered by the UAS DSMs differ from the areas covered by the RTK-GNSS DTMs. Unlike the RTK-GNSS surveys, the UAS DSMs cover the investigated palsas completely, which provides better overview of the changes in the palsas, especially at Peera. We will add "**Based on the UAS DSMs**, ..." at the beginning of the section as suggested.

22. A thought: you would be able to confirm whether subsidence of 20 cm between 2016-2021 found using UAS DSMs corresponds with the subsidence measured by RTKGNSS from the same time period 2016-2021, given that you looked at the same area.

We did consider adding the comparison of palsa changes based on the RTK-GNSS DTMs and UAS DSMs early in the process of preparing the manuscript, but left it out from the final version. Based on this comment and the comments of Reviewer 2, such comparison should be included. Therefore, we will add a new Section 4.5, as mentioned earlier in the reply to the comment #12, showing the differences in the mean top-of-palsa heights derived for 2016 and 2021. In this section, we will also include the RMSE for the elevation and height values between the RTK-GNSS and UAS DSMs.

With this comparison, however, we cannot confirm the 20 cm subsidence found in the UAS DSMs as they cover larger areas than the RTK-GNSS surveys (as mentioned in the previous reply). Within the TOP areas and within the overlapping areas, the comparison shows that the mean height change of the UAS DSMs is double that of the RTK-GNSS DTMs. We will add these results and discuss them in the revised version of the manuscript.

23. Line 229: Well, you can’t measure the internal permafrost with the RGB images which only show the surficial extent of the palsa. Also Line 294 you refer to how UAS data can lead to overestimation of permafrost. The aerial photos, or any surficial representation of the palsa is only that – the representation of the geomorphological form of the palsa. To find the permafrost, which is an internal characteristic, so far the ALT measurements are needed. Also in Line 294 – it wouldn’t be only UAS, but also any aerial photo, or even Lidar that would “overestimate permafrost”.

That is true. Our intention was to highlight the difference in the extent of the Laassaniemi palsa derived by the two methods and explain the reason for this difference. We will rephrase this paragraph so that the emphasis is on the fact that we used the ALT values to delineate the palsa extent from the RTK-GNSS DTMs but did not use them for palsa delineation from the UAS data. The revised paragraph will read as follows:

“Over 30 % larger area of the Laassaniemi palsa as delineated from UAS DSM (122.2 m^2) compared to RTK-GNSS and ALT measurements (82.5 m^2) is caused by the difference in how the extent of the palsa is defined using these two methods. In the RTK-GNSS and ALT approach, information about the active layer affects the delineation of the palsa edge, especially in areas where the palsa edge cannot be distinguished based on topography alone. For the UAS-based delineation, the ALT-values were not used, and the palsa extent is therefore only based on the information about the surface topography and vegetation cover.”

We agree that it should be clearer that delineation of palsas from optical aerial data is not the same as delineating the actual permafrost extent, and we will edit the Line 294 to incorporate this point.

24. Line 239/240 – Include in the sentence that this measurement is derived from manual delineation of palsa area from the aerial photos from 1960, …2021.

We will add "**Based on the manual delineation of palsa area from the aerial photography**,…" at the beginning of the paragraph.

25. Line 240 – that's quite a sad loss of area…

Indeed. At these loss rates we will not have our monitored palsas for much longer.

26. Fig 6 – Legend text is pretty small. Also, I was confused about which legend belonged to which square. Maybe better to make the figure a little bigger, and clearly divide the two sides of absolute and relative change maybe with some lines or column names and Legend heading.

We will rearrange Figure 6 so that the absolute and relative changes are now more clearly separated and enlarge the legend text for better readability.

27. Fig 7 – Nice map, I like this a lot. Is there a way to make it larger in the publication?

Thank you! We will increase the size of Figure 7 in the revised manuscript.

28. Line 259 – Much better description of the results is needed here. What was the R2 of the most correlated climatic variables? Without proper description of the result, it is hard to have any discussion, and hard to compare to other studies (eg Olvmo et al. 2020).

We will add the R^2-values of the highest correlations and rephrase the paragraph so that it states more clearly the lack of significant correlations and that the regression results are rather indicative because of the low number of samples. We will also move Table A2 to the main text, as mentioned in the reply to the next comment.

The revised paragraph will read as follows:

"Because of the low number of samples (only four periods), the results of the linear regression analyses between the annual area loss rates and climatic parameters are only indicative. None of the correlations were statistically significant at the 95 % confidence level (Table A2). At Peera, three parameters related to the winter air temperatures had the highest correlation coefficients, and p-values < 0.1. These parameters were MWAT ($R^2 = 0.88$), √FDD ($R^2 = -0.88$), and the number of days with air temperature < -10 °C ($R^2 = -0.87$). At Laassaniemi, the area loss rates had very little correlation with the climatic parameters; the lowest p-values were only around 0.3 for snow cover onset ($R^2 = -0.44$) and snow cover duration ($R^2 = 0.46$)."

29. Why not have a figure similar to Fig 4 for your area loss? If not, then I think you should at least bring Table A2 into your main text, as I think it is more important than Table 3 and Fig 9.

We do not think that a figure showing the relationships between annual area loss rates and climatic parameters is necessary, because the number of samples is very low and the lack of statistically significant correlations. We can move Table A2 into the main text, however.

30. Table A1 – Put that the ALT and RTK data are annual from 2007-2021 in the Table Text.

Done.

31. Table A2 – Put that the area loss data is from 1960-2021.

Done.

32. Line 299- The palsas in Olvmo et al 2020 are also larger than those in your study. Would be good to put the size of the palsas from Olvmo et al in the discussion. As you write, Borge et al (and I think Seppälä too) talks about the importance of the morphology in relation to degradation.

That is a good point. We will rephrase this sentence so that the difference in the palsa area sizes is clear.

33. Line 315 – are they "more important" than climate? Or merely "also important factors"? I think the latter.

This is a very good question. After thorough consideration, we concluded following:

Based on our results, it does seem that other factors are more important than climatic variables in regulation of the seasonal thaw at Laassaniemi. However, more data and comprehensive statistical analysis are needed to confirm this statement. Therefore, we decided to reword Line 315 of the original manuscript from "… are more important factors regulating the seasonal thaw…" to "… have strong effect on the seasonal thaw…"

34. *Also, do you think your use of only "top of palsa" area measurements of ALT has led to a lack of a strong correlation with climatic factors, particularly in the larger of the two palsas you study?

As mentioned above in the reply to the comment #9, we have checked the correlations with the climatic parameters using all ALT values ≤ 1 m. The results showed less correlation with the temperature-related parameters and better correlation with precipitation-related parameters when all ≤ 1 m values were used. However, the improvements in the correlations were not high enough to change the p-value from > 0.05 to ≤ 0.05, for any of the parameters at either site. Thus, we conclude that using only TOP-ALT values was not the reason for the lack of significant correlations.

35. Line 351 – rather than say "the permafrost area in 2021 was less than 25% of that in the 1960s" I would say that "the palsas in 2021 have shown a lateral degradation of 75% *(or whatever the number is…)* the 1960 areal coverage", since that is what you really assessed that with the aerial photos. What area exactly the permafrost is (an internal characteristic that you aren't seeing with the images), isn't necessarily the same as the extent of the palsa at the time you image it.

We agree, that palsa extent is not the same as permafrost extent. We will rephrase this sentence as follows: "At the Peera and Laassaniemi palsa mires, **the extent of palsa area degraded over 75 % between 1959 and 2021.**" In addition, we will make sure that we do not use 'permafrost extent' when we mean 'palsa extent' in the revised manuscript.

Corrections and text improvements

36. Line(L) 17 – "its extent" is vague. Replace with a better geographical noun – whether " the Arctic" or "the Arctic permafrost region".

By "it's extent" we mean that the permafrost degradation is observed not only in the Arctic, but also in the mountain environments, the Tibetan Plateau, and non-glaciated areas of the Antarctic. We will edit this sentence to include the list of the regions with permafrost.

37. L23 – Write so it is more clear… "The main difference between peat plateaus and palsas are in…"

Done.

38. L31 – mires'

Corrected.

39. L49 – "ALT varies from a …"

Corrected.

40. L146 – "UA system settings" should be "UAS settings"

We will add "the" in front of "settings" to make it more clear that we mean variations in the UA systems and the settings used in the data collection.

41. L86 – palsas'

It is true that the surrounding vegetation complicate delineation of palsas from aerial data and DEMs in general. However, in this case, "palsa's edges" refer to the edges of the Laassaniemi palsa. Therefore, we do not change this as suggested.

42. L132 -aerial

Corrected.

43. Fig 5 should appear before Table 1, according to the earlier reference to it in the text (at Line 212).

Thank you for pointing this out. We will edit the placement of figures and tables to follow the order in which they appear in the text in the revised manuscript. Their final placement in the article depends on the typesetting process, however.

44. L332 – "…in which November …" Delete "the". Or, do you even need this clause?

We will remove the clause about November being included in the winter months.

45. L333 &359 – Arctic (I think it should be capitalized when used as Arctic region)

Corrected.

46. L334 – …ground's thermal…

Corrected.

References

Liston, G.E. and Elder, K. A.: Distributed Snow-Evolution Modeling System (SnowModel), J. Hydrometeorol., 7, 1259–1276, https://doi.org/10.1175/JHM548.1, 2006.

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