Thermal erosion patterns of permafrost peat plateaus in northern Norway
L.C.P. Martin et al.

We are grateful to Reviewer 2 for the time and effort dedicated to our work. We present below our detailed answer to each of the discussed points. The reviewer comments appear in black Times and our responses appear in brown Arial. *Quotes from the manuscript are in brown italic Times.*

Reviewer #2

Martin et al. use a combination of field measurements and modelling results to investigate spatiotemporal patterns of permafrost landscape erosion. Specifically, they look at the controlling effect of snow cover and its microscale distribution in such landscape evolution. This was an interesting and in general well-written paper. In particular, the authors demonstrate a novel model structure that is capable of explicitly simulating these processes dynamically and give an indication about how this surface scheme could be considered in large scale climate studies. This work represents a useful contribution to process understanding in permafrost environments in the Arctic and I recommend publication in TC subject to some minor comments.

General points

I think it would be useful to define thermal erosion. According to NSIDC glossary: "the erosion of ice-bearing permafrost by the combined thermal and mechanical action of moving water." I think this study is broader than that in terms of heat fluxes considered. You attribute 80% of erosion to "thermal erosion", presumably not just due to the action of water. Of course, you might not agree with the NSIDC def but perhaps state explicitly the processes considered right at the beginning of the text.

We acknowledge that our use of “thermal erosion” does not match with the definition from the NSIDC which relates to a rather specific process. To avoid ambiguities, we decided to replace all the occurrences of “thermal erosion” by “lateral thermokarst” or “thermokarst” depending on the context. We also define “ lateral thermokarst” in the introduction for more clarity:

« In this study, we use the term “lateral thermokarst” instead of “lateral erosion” to highlight that the lateral shrinkage of peat plateaus is governed by thermokarst processes.»

How does the forcing compare to observations at the meteorological station? In general, why not use the station data directly? When comparing to observations it would be good to have an appreciation of any potential biases in the forcing. A plot in the annex or at least some statistics to show how the downscaling performs would be important I think.

In first place, we want to remind that surface temperatures in the model are derived from surface energy balance calculation, which requires various climatological input data such as incoming long and short wave radiations, air humidity, wind speed and these data are not available from the station nearby the site, in Cuovddatmohkki (286 m asl, 7 km east from Šuoššjávri, which is 310 m asl).

Yet, we understand this concern and now provide some evaluation plots in Appendix C (reproduced below: Fig. R2-1 and R2-2). The nearby station provides temperature and precipitation data. Because we adjust snow depth in our modeling experience, comparing snow fall is irrelevant so we compared rainfall only (here taken as precipitation when Tair > 0°C). Temperatures are in very good agreement with similar mean annual values. The rainfall is overestimated in the forcing by 27%. We expect this to be of negligible impact for the ground thermal regime of the peat plateau and the mire for the reasons we develop below, in the answer to the last *General Point* of the reviewer. Air humidity and wind speed values used for comparison come from the Karasjok weather station, located 50 km east of Šuoššjávri and standing 180 m lower. Air humidity values are in good agreement. Wind speeds are higher in the forcing data but the two sites have different settings regarding wind. The Karasjok station is located in an urbanized area with a higher surface roughness that likely promotes lower wind speeds. Yet the comparison enables to see that the wind data take reasonable values and that real life windy events are represented.

Finally, we want to point that ground surface temperature integrates the thermal effect of the different climatic variables via the surface energy balance calculation. In this regard, simulations for which the ground surface temperatures match those of the temperature loggers (Appendix A) is a good indicator that the forcing data are realistic and do not bear any significant bias.

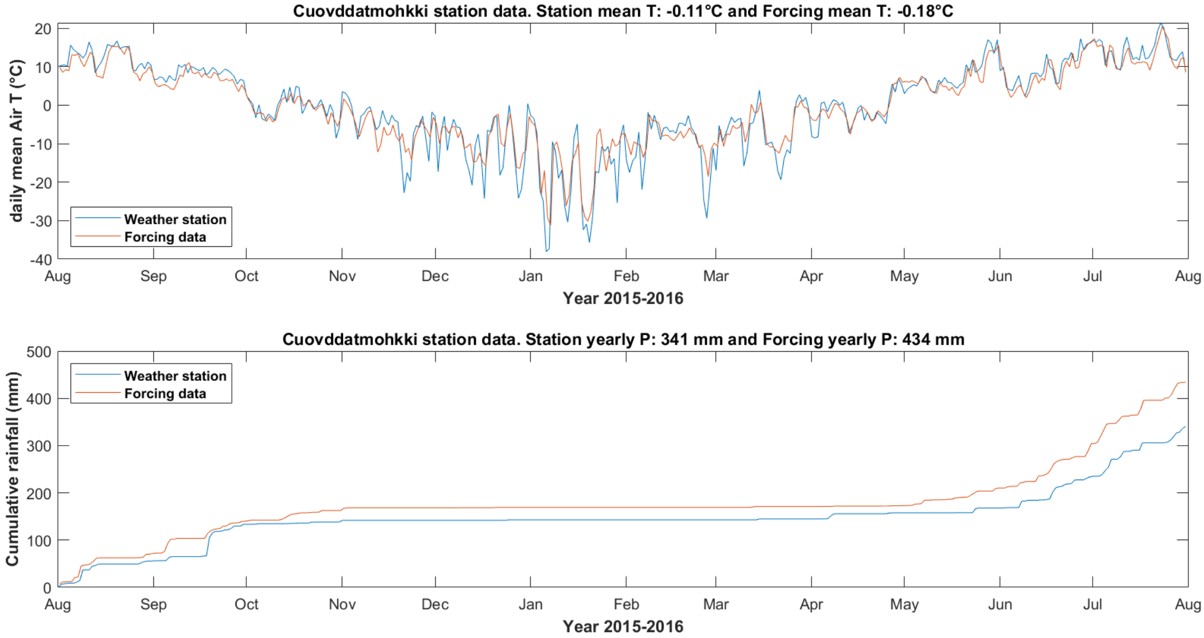

Figure R2-1. Comparison between weather station and forcing data (Fig. A3 of the main manuscript). Top: daily mean temperature of the air 2 meters above the surface. Bottom: cumulative rainfall. Rainfall for the station data is taken as precipitation falling when the air temperature is above 0°C. The station is Cuovddatmohkki station located at 286 m asl, 7 km east from Šuoššjávri (310 m asl).

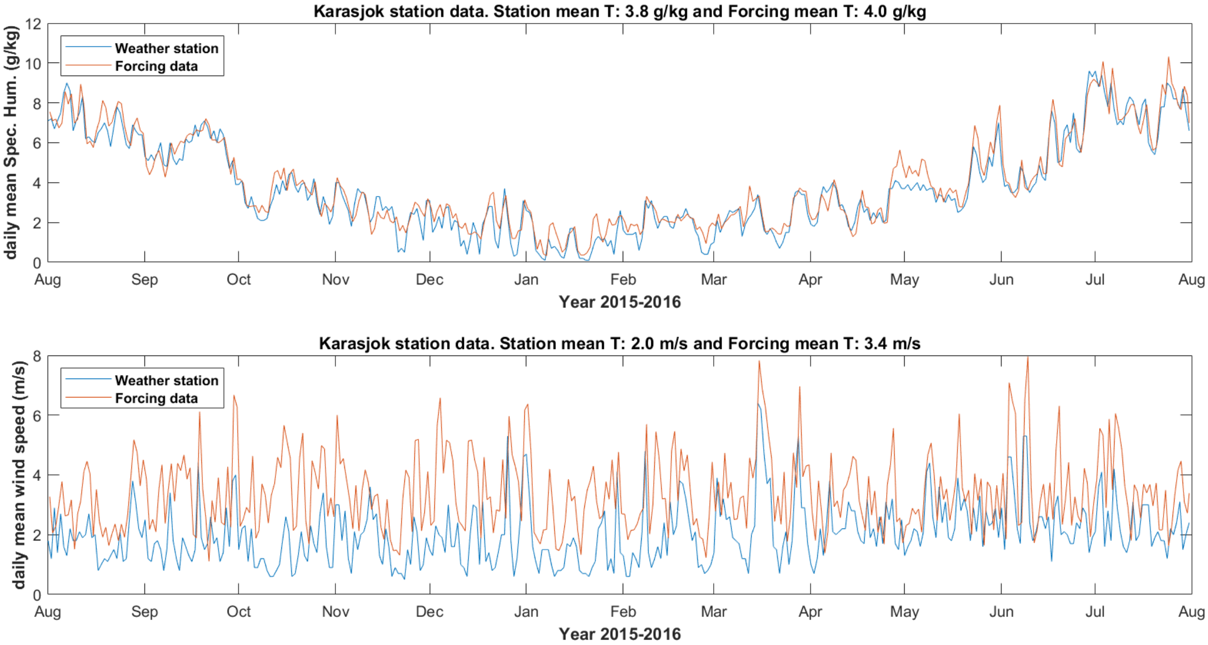

Figure R2-2. Comparison between weather station and forcing data (Fig. A4 of the main manuscript). Top: Specific Humidity (g of water vapor per kg of air). Bottom: wind speed. The station is Karasjok - Markannjarga station, 131 m asl, 50 km east from Šuoššjávri (310 m asl).

L.357: Why no subsidence with zero snow? I understand snow depth is important for thermal insulation and snowmelt is important for thermal erosion but wouldn't rainfall and melt due to positive air temperatures also generate subsidence, at least to some extent? Is the insulating effect of e.g. 10cm snow really so significant that it prevents significant winter cooling over the 0cm snow example? I would say this aspect of the modeling study needs to be explained in the text.

The absence of subsidence with zero snow and the difference in behavior with 10 cm of snow is not an hypothesis we make but a result we observe in our simulations. The absence of snow has a strong impact on microtopography in Scandinavia and artificial snow clearance in permafrost free area can result in the formation of palsas (Seppälä, 1982, 1995). Yet we do not want to say that all peat plateaus with 0 snow will be stable and all those with 10 cm will degrade at a certain speed. The insulation role of snow increases progressively with snow depth but in our case the climate forcing and thermal properties of the ground we used create this threshold behavior for a limited snow depth variation. Nonetheless, a peat plateau located in Northern Siberia where the mean annual air temperature may be 2°C colder or more could probably bear 10 cm of snow without showing sign of thermokarst degradation. In line with comments from reviewer 1, we added the following paragraph to the discussion related to the snow cover to clarify this point (section 5.2.2):

« Our simulations confirm the crucial role of snow on the ground thermal regime and peat plateau degradation. They shows that a stability threshold is crossed between zero (stability) and 10 cm snow depth (lateral thermokarst). Even though the absolute value of this threshold cannot be generalized due to our simplistic snow model and the interplay of climatic parameters, it is broadly consistent with field experiments of man-made snow clearance in permafrost-free mire areas in Northern Scandinavia, which resulted in the formation of new palsas (Seppälä, 1982, 1995). However, it is possible that our simulations slightly overestimate the sensitivity of edge retreat to snow depth variations, with the true stability threshold at higher snow depths. While measured March snow depths in 2015-2018 regularly exceeded 20-30 cm (Fig. 3), our simulations show higher than measured volume changes for the 20-30 cm snow scenario (Fig. 9). This behavior could at least partly be related to above average air temperature of the hydrological year 2015-2016 used to force the model (Fig. 1), which should be clarified with transient simulations in future studies (Sect. 5.3.2). »

L.219: I didn't fully understand the snow scheme I think some more details here would be useful. Specifically, how are the max snow parameters defined for each tile? By geographically matching with measurements? How do you scale the snow input in the model to generate the 4 snow height experiments? Do you apriori set a max/min snow height for each experiment somehow? And does this affect also liquid precipitation input?

We understand from this comment that the initial explanations on the lateral snow scheme and the use we make of it that are given in section 3.3.2 are insufficient. We reworked the text in depth and extended it to match the questions of both reviewers as follow:

Section 3.3.1 (The CryoGrid3 model) no states:

« The snow depth is a major control for the ground thermal regime (Gisnås et al., 2014; Martin et al., 2019; Sannel, 2020; Sannel et al., 2016). Strong wind redistribution of snow from the plateau to the lower-lying mire leads to a shallow snow cover on the plateaus (Sect. 3.1). In the laterally coupled tiling approach of CryoGrid3, wind drift of snow is not computed in a physically-based way. Instead, fresh snow is redistributed at regular time intervals between all tiles, based on the relative surface elevations of the snow covered tiles. Tiles gain/loose snow proportional to the difference between their surface elevation and the average surface elevation of all tiles in a mass-conserving scheme. Hereby, snow is redistributed between all the tiles, without taking their relative location into account. To represent immobile snow trapped by vegetation and/or rough surfaces, snow is only considered movable if its depth exceeds the "immobile snow height", which can be adjusted as a model parameter. In the setup used for this study, the elevation difference between the plateau and the mire leads to complete redistribution of snow that exceeds the immobile snow height from the plateau to the mire. The immobile snow height can be therefore used to adjust the overall snow depth on the plateau in our modeling experiments. »

And section 3.3.1. (Model setup) now states:

« While this is clearly an idealized setup, it is still possible to compare the magnitudes of modeled volumetric plateau degradation with field observations for sufficiently straight sections of the plateau edge (Sect. 3.2, Fig. 2). As field observations of snow depth show a considerable spread of snow depths on the plateau (that cannot be reproduced by modeling), we investigate model sensitivity towards snow depths on the plateau by adjusting the immobile snow height, using four different values within a realistic range. In each configuration, the same immobile snow height was applied to all tiles. During the simulations, the snow depth on the plateau varied within ranges of 5-10cm due to snow fall, snow drift and snow melt. Therefore, we named the scenarios based on their snow depth range, i.e. 0 cm snow, 5-10 cm snow, 10-20 cm snow and 20-30 cm snow. »

Connected to the above 2 points I assume rainfall has an important contribution to erosion - was this quantified or at least put in context with other factors?

We see two possible types of influence of rainfall on the ground thermal regime. On the one hand, during summer, the input of liquid water at a temperature higher than the ground temperature promotes positive sensible heat fluxes to the ground. On the other hand, variations in the soil water content affect its thermal properties (heat capacity and thermal conductivity) and the magnitude of latent heat fluxes during soil freezing and thawing. We do not expect the first contribution to be significant because of the climate setup of our site (Fig. R2-3) and of the region in general. Indeed, summer precipitation is not abundant. June, July and August typically collect less than 200 mm of rainfall in total and the monthly average temperature in this period neighbors 10°C. Therefore the amount and temperature of the water input in the soil can only drive very limited heat fluxes.

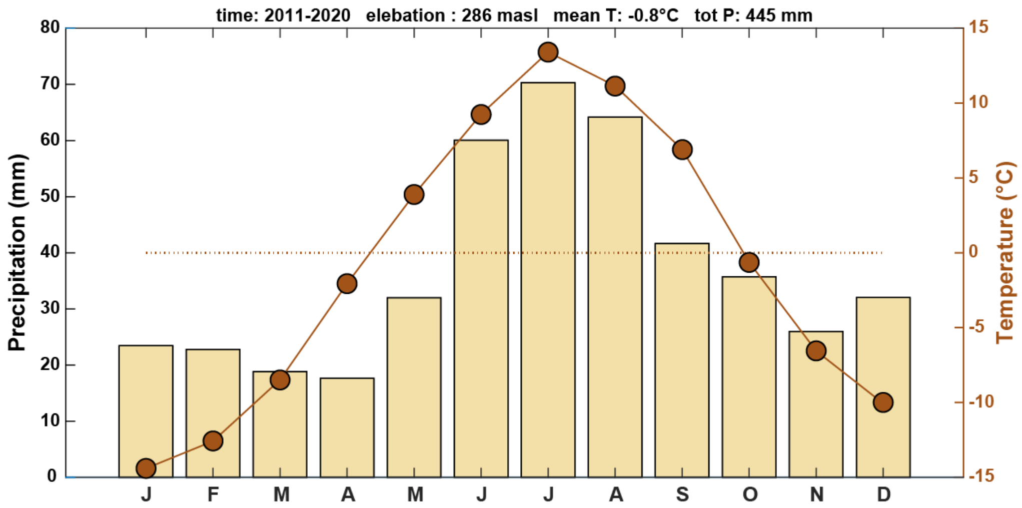

Figure R2-3. Monthly averages for precipitation and temperature over the 2011-2020 period at the Cuovddatmohkki station.

Now regarding the second point, in the real world, the wet mire is usually saturated and the plateaus are usually well drained because they can drain towards the mire. Yet we don't have observations to quantify this phenomenon that we observe in the field. In our model, this translate in the vertical water transport in the soil by the bucket scheme and the lateral transport between tiles from the plateau towards the mire via the lateral water fluxes. These combined fluxes make that the upper part of the plateau is most of the year at field capacity while the mire remains saturated. Provided that the precipitation are not scarce enough to dry out the plateau or abundant enough to flood it (extreme cases), this situation makes that our setup would exhibit similar soil water contents for the plateau for moderate increase or decrease of annual rainfall. For this reason, we think that the 30% overestimation of rainfall in the forcing data have negligible influence on our results.

I would recommend a reread to catch instances of poor grammar or so. Some are detailed below but likely not all.

All the points below were implemented. We also thoroughly re-read our manuscript to copy edit it.

Detailed points

• L.51: result > results → done

• L.61: I wouldn't call a threshold of 800mm "limited precipitation" → replaced by *«and precipitation below 800 mm yr^{-1}»*

• L.64: Northern Hemisphere → done

• L.65: plateaus 'degradation > plateau degradation → done

• L.68: "33-71%" seems like quite an uncertain result can you explain it a little bit? → this is a difference observed between different sites in Borge et al. (2017). Explaining this difference is a delicate problem that the authors do not fully elucidate even though they suggest key parameters such as the geometry of the plateaus. Small peat bodies and plateaus with complex palsa/mire interfaces and high perimeter/area ratios tend to degrade faster. We think that this type of consideration do not belong to this part of the manuscript. Yet, in order not to let this wide range of degradation value unexplained we added the mention *«(depending on the site)»* after it.

• L.74 "to the understanding of.." → done

• L.81: could > can → done

• L.109: "bleu" → done

• L.110: cited serial number should be assigned to some authority to give it a meaning. Is it from the national met service? → We fully modified the figure and caption and removed the serial number.

• L.122: Aeriel surveys were conducted... → done

• L.199: strange indent → We believe this ident is identic to the others for the same level of title.

• L.249 why ERA-Interim and not the latest gen of reanalysis ERA5? → The forcing data were produced before ERA5 was available.

• L.259: Is wetter future foreseen in climate projections for the region? If so maybe make this link explicit that it is a future analog to some degree.

As mentioned in the answer of Reviewer 1, following the questions of both reviewers, we conducted further analysis of the climatic data from the Cuovddatmohkki station located nearby the Šuoššjávri site (Fig. R2-4). In the light of the last decade, the hydrological year 2015-2016 still appear warm, even though 2 other years (among the last ten) have a similar mean annual temperature (2011 and 2013, Fig. R2-4). Regarding precipitation, even though this year appeared wet in comparison to the former normal period of 1961-1990, its annual value of 472 mm is now consistent with the mean value over the last 10 years of 453 mm. Regarding the question of the reviewer, we are unsure of the anticipated regional climate for the next decades/century but the impact of this particular year on our results is now further discussed in Sect. 5.2.2 (see quoted new text in response to comment on line 357 above).

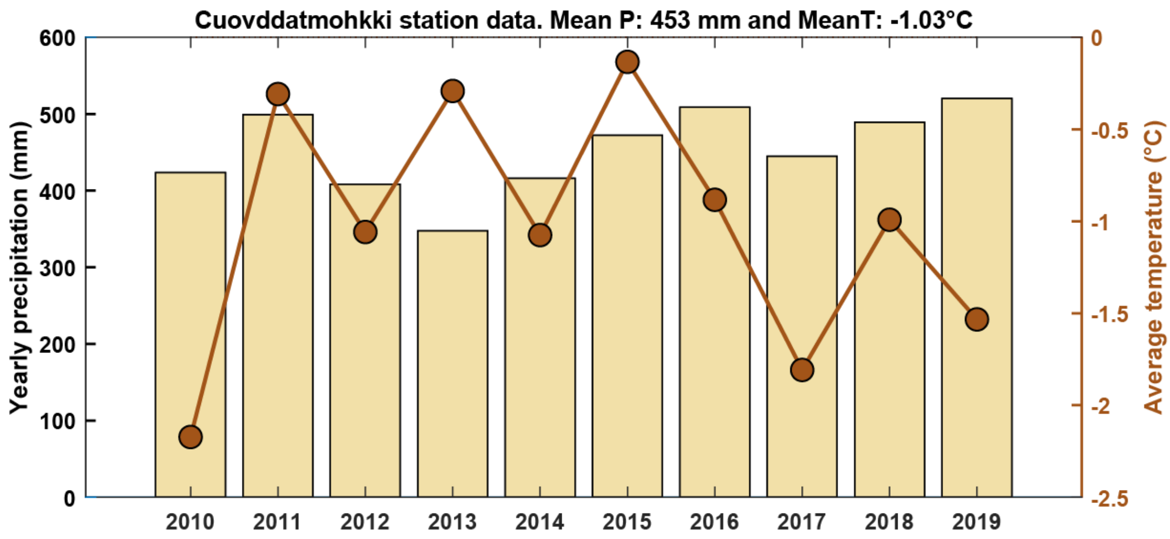

Figure R2-4. Mean annual precipitation and temperature at the Cuovddatmohkki station. The mentioned years are hydrological years. The one used as a forcing for the study is the 2015 one.

To precise this idea, we modified Fig. 1 in the main manuscript which is now as follow (Fig R2-5) and modified Section 2 and Section 3.3.4 (Steady state climatic forcing and model spin-up) which now states:

Section 2:

« The climate of Finnmarksvidda is continental. The Cuovddatmohkki station nearby the site shows that in the last decade, mean annual air temperatures ranged from 2°C to 0°C, with yearly precipitation from 350 to 500 mm (Fig. 1). Average air temperature is of -2.0°C for the 1967-2019 period, of -1.0°C for the 2010-2019 period and of -0.1°C for the 2015-2016 hydrological year (year used for modeling in this study). Average yearly precipitation is of 392 mm for the 1967-2019 period, 453 mm for the 2010-2019 period and 472 mm for the 2015-2016 hydrological year.»

Section 3.3.4:

« As shown on Fig. 1, the hydrological year 2015–2016 has been relatively warm. It is 0.9°C warmer and 4% wetter than the decadal average from 2010 to 2019 (Sect. 2).»

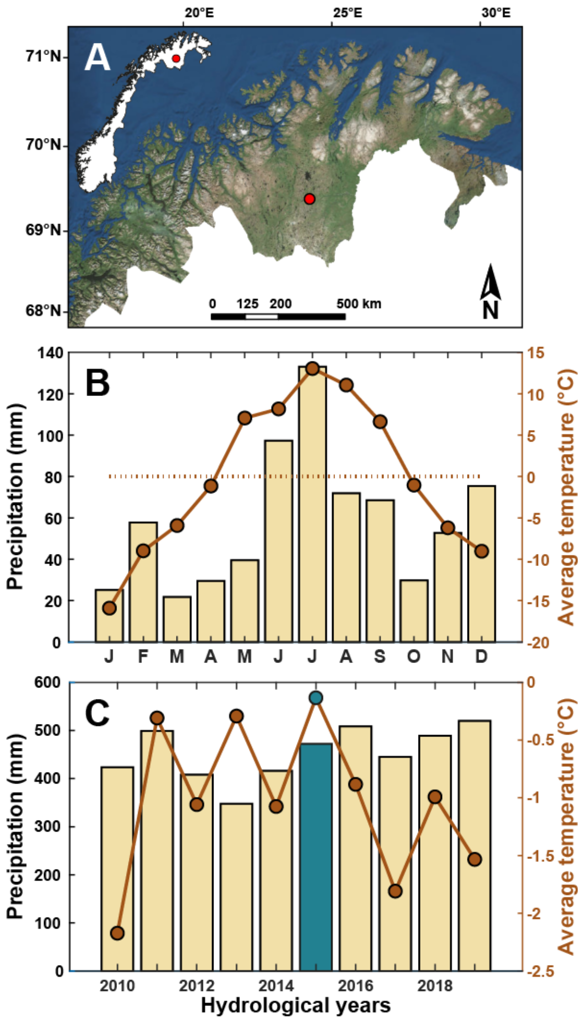

Figure R2-5. Reproduction of the new Fig. 1 of the main manuscript. A. Location of the Šuoššjávri site in Northern Norway. B. Monthly Precipitation and Temperature data of the forcing file used to simulate the hydrological year 2015-2016. C. Yearly precipitation and Temperature data from the Cuovddatmohkki station located at 286 m asl, 7 km east from Šuoššjávri (310 m asl). The green bar and point indicate the hydrological year 2015-2016.

• L.277: "where the edge.." → done

• L.Fig A1 Do these boxes represent the absolut range in both x and y → The horizontal ranges correspond to the snow ranges we are working with in this study, the vertical range correspond to the standard deviation of the y values. We clarified this in the caption of the figure:

« The snow ranges on the x axis are those used for the modeling work of the present study. Observations from Martin et al. (2019) have been distributed in these ranges for comparison. Vertically, MAGST and ALT values span over the mean ± 1 standard deviation range for both observations (variability among observations) and simulations (variability among the tiles of a simulation). »

• L.594: you say the sim is in perfect agreement with the obs, while the sim is within the window of the obs it seems to me the variability is very much lower in the simulation. If you plotted this as a scatter plot I suppose it would look different? → We acknowledge some information were missing, therefore we added the following text to answer this point:

« Overall, our simulations show good agreement with field measurements. However, they feature a smaller variability than the observations because the variability of the simulations is diagnosed for one idealized peat plateau profile, whereas the variability in the observations is derived from individual points distributed over the plateau which each feature different overall conditions (e.g. snow cover build-up, drainage regime, etc.). As discussed in Sect. 5.3.2 ensembles of simulations exploring different geometries and parameter sets would be required to match the variability of the observations. »

• Figure 8: can you locate this profile on one of the overview maps and refer to it so the reader can understand the spatial context. → Results in Fig. 8 are simulation results. They are based on the setup presented in Fig. 4 and correspond to an idealized peat plateau profile which is not supposed to represent one precise profile of the site (Sect. 3.3.2. Model Setup). We added the following sentence to the caption of the setup figure (now Fig. 5) to clarify this point:

« [This setup] does not aim at representing one particular natural setup of the edge transect areas detailed in this study. »

• L545: "Thermal erosion of the plateau edges is the main process through which thermal erosion occurs and accounts for 80 % of the total measured subsidence" and what accounts for other 20%? Where you able to quantify that? → In section 4, we compared the subsidence happening over the outermost 2 meters of the plateau and the subsidence happening over the whole plateau. We found that 77% of the total subsidence (affecting the whole plateau) was occurring over these outermost 2 meters. We assume that the reviewer got confused about this 80% value because we rounded it from the initial 77% without further explanations. Therefore in the conclusion, we came back the 77% value and indicated it was coming from section 4.

References

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