Response to Reviewer 1

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

We are very grateful to Reviewer 1 for the in-depth reading and thorough review we received. We present below our detailed answer to the discussed points. The reviewers' comments appear in black Times font and our responses appear in brown Arial font. *Quotes from the manuscript are in brown italic Times.*

Reviewer #1

Martin et al. (the authors) present a study on erosion of peat plateaus in a peat plateau landscape, northern Norway. They use two digital elevation models (DEM) acquired through two drone imagery surveys in 2015 and 2018. The DEM differences serve as a basis to determine in particular the plateau edge retreat, which is the major focus in this study. They come up with an index to describe whether the erosion is really affecting the edge as edge retreat or if the erosion is uniform over an area, expressed as HvsV (horizontal vs vertical) index. Alongside, they set up a CryoGrid3 (CG) model to investigate, first, if the model is able to represent the erosion pattern, and second, how important snow cover is for different erosion patterns. The erosion patterns of interest are initial slope adjustment, steady edge retreat, and plateau collapse (uniform subsidence of the plateau). The study is quite complex due to the various methods but they are able to provide interesting insights into the relationship of snow cover height and erosion patterns. The results are interesting and provide possibly valuable insights for a broader understanding of how low relief landscapes, representative for many permafrost regions in the Northern Hemisphere, correspond to variations in snow covers that are expected due to changes in climate. The multitude of approaches, however, comes at the cost of – sometimes severe - shortcomings that I will list later on in more detail. Shortcomings comprise the level of detail and provided information regarding individual working steps, chosen climate forcing for the model and, related to that, unassessed uncertainty related to climate forcing. As one of the authors' incentive is to provide a gain in knowledge that can serve to improve land surface model schemes representing permafrost landscape evolution, these shortcomings limit currently this goal. The multitude of working steps and reliance on e.g. downscaled reanalysis data might prevent addressing some issues with the desirable level of detail. However, in this case, the authors should provide more discussion of uncertainties related to these aspects and state these limitations more clearly, and carefully mention these shortcomings in the deductions. I think that with addressing these issues the work would make a worthy contribution to TC.

General comments

The paper is overall well-structured but some parts should be reorganized. The language is sometimes too colloquial and some sentences are grammatically and structurally wrong. Note that I am not a native English speaker myself. With the help of the numerous co-authors, it should be possible to identify many of the passages where the wording can be made more concise and avoid colloquial phrases. Examples are listed in the detailed comments (there might be more than listed, so I advise the authors to read through the entire text again).

We have implemented the suggestions listed in the detailed comment section. We also have improved formulations and English language.

While of good quality, some figures should show additional information or be reworked (details also follow). Information on the model is very sparse. It is clear that the entire model should not be explained again as the authors base their work on previous studies. However, parameterizations chosen and important details on snow redistribution should be presented; for repeatability as well as to allow the reader comprehend how the important methods work. How is the 1D heat transfer "extended laterally"? How is the snow redistributed (uniformely, based on a gradient)? It also remains unclear how the chosen 3 years of climatic forcing affect the thermal evolution. The authors say that the three chosen years are particularly warm and wet, yet they use these three years for a spin-up and the modelling. No information about the boundary conditions is given or how their model parameterization is maybe accounting for this extreme climatological forcing. Does it have an important impact on the finally derived conclusions on snow depth and importance of snow redistribution? In particular, if the model shall be used to find a parameterized simplification, does this choice limit the transferability? I am quite picky here because I know that even slight changes in climatological forcing can have severe impacts on the ground thermal regime. Using the same three extreme years for a spin-up and actual modeling might thus impose a strong bias. You should provide good evidence that your findings are sound despite this.

Following the reviewer's structure, we provide detailed answers to these different points in the *Detailed comments* section. Regarding the overall assessment of our modeling work, there are two important clarifications we want to make at this point. Firstly, to our best knowledge, no modelling framework has been demonstrated to date that can represent the transient evolution of peat plateaus, in particular the widely observed edge retreat. We do not claim that our study can deliver this, but we believe that it is a first, but important step in this direction. Our study is designed as a proof-of-concept to showcase the features and capabilities of the new model setup. For this reason, we have selected a simple steady-state climate forcing data set employed in a previous study (Martin et al., 2019), which at the same time allows comparing computed volume rates to observations. With the selected setup, we can demonstrate the threshold behavior of the edge retreat in the model, as well as volume rates depending on the applied snow forcing. The purpose of the comparison to observations is to show that modeled rates are in order of the observations (which show a large spread) when assuming realistic snow depths and ground stratigraphy. This has never before been demonstrated and in our opinion is clearly novel science that goes significantly beyond the state of the art in simulating peat plateaus.

At least two key steps are missing to realistically simulate the transient evolution of peat plateaus:

1. Model spin-up with long-term forcing data. It is widely accepted that the initialization of temperature and ground ice profiles is a key issue in permafrost simulations. This is generally achieved with a model spin-up, mostly by running the model with an initial forcing data set for a period sufficiently long period (generally at least several decades) that ground temperatures in deeper layers are in equilibrium with the applied surface forcing. For peat plateaus, a particularly long model spin-up is required due to the strongly insulating peat layer, which delays propagation of surface forcing to deeper layers. With the presented model setup, the spin-up is a significant problem, since the edge would already retreat during spin-up, when forced with readily available forcing data, such as the ERA-5 reanalysis. As a result, modeled retreat rates would be the mixed result of model spin-up and already active edge retreat which in our opinion precludes any meaningful interpretation. In our simulations, initialization has been accomplished with the zero snow scenario, in which the peat plateau is indeed stable, thus avoiding the problems during spin-up, so that we achieve a realistic ground temperature profile. In Scandinavia, peat plateaus have only been stable in the Little Ice Age, which means that model initialization should take place in this period. Such simulations should in principle be possible with the presented set-up, but preparing a bias-free forcing time series for such long timescales, including radiation and precipitation is a significant challenge and clearly beyond the scope of this study.
2. Simulations for three-dimensional geometries. In this work, we have modeled edge retreat as a two-dimensional process, which means that all fluxes (snow, water, heat) in the third spatial dimension are assumed to be zero. Mathematically, this corresponds to the assumption of translational symmetry in this third spatial dimension, i.e. there is no variability

in this spatial dimension and all information is contained in 2D-sections, as presented in this work. As acknowledged in our manuscript, the observations clearly show that there are strong three-dimensional effects at play, and future studies should focus on the role of edge geometry, e.g. the curvature, on the retreat rates. As the number of possible model configurations increases strongly in three dimensions (and so does the computational effort), we have left this aspect to be explored in future studies.

Secondly, regarding the choice of our forcing data, we understand the point of the reviewer regarding the representativity of the values we provide. We want to show that our model simulates a threshold behavior regarding the presence/absence of snow and a an edge retreat sensitive to snow depth that shows acceptable subsidence rates with respect to field observations. We do not claim that the relation between particular values of snow depth and degradation speed in our simulations should be expected for other setups or other climatic forcing. For these aspects of our approach to be clear, we now provide additional details about the climate of the year 2015-2016 and we added in the discussion a section explaining that we assume our sensitivity test shows a too strong sensitivity to small values and small variations of snow depths because 2015-2016 was warmer than average.

To assess the characteristics of the 2015-2016 year in the light of the last decade, we used the data from the Cuovddatmohkki climate station (located 7 km away and 25 m lower than the Šuoššjávri peat plateau site) (Fig. R1-1).In this perspective, the hydrological year 2015-2016 has the warmest temperatures, but 2011 and 2013 have a similar mean annual temperature and 2015-2016 is by no means an abnormal year. Regarding precipitation, although this year is wet in comparison to the normal period 1960-1991, its annual value of 472 mm is consistent with the mean value of 453 mm computed for the last 10 years. The selected year is therefore broadly in line with the climate of the last decade. To make these additional information available, we modified Figure 1 in the main manuscript (Fig. R1-1), Section 2, Section 3.3.4 and Section 5.2.2 which now state:

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). »

Section 5.2.2:

« 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). »

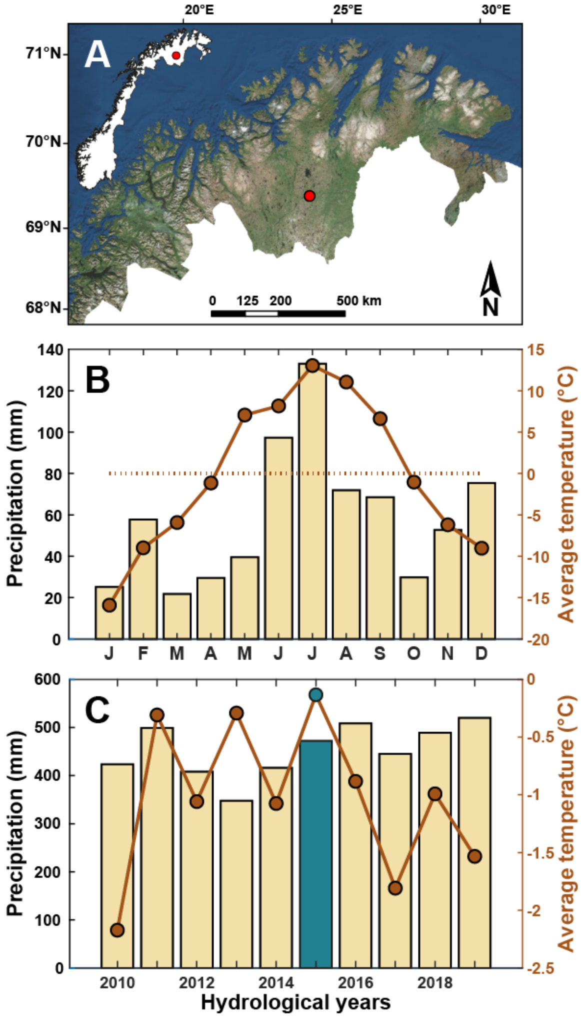

Figure R1-1. Reproduction of the new Fig. 1. A. Location of Šuoššjávri in Northern Norway. B. Monthly data of the model forcing to simulate the hydrological year 2015-2016. C. Yearly data from the Cuovddatmohkki station located at 286 m asl, 7 km east from Šuoššjávri (310 m asl) for the last decade. The green bar and point indicate the hydrological year 2015-2016. Panel A is modified from Martin et al. (2019).

Detailed comments

L5-6 ".. of surface ground thermal regime " – the surface or ground thermal regime or the interface. Please clarify.

We rephrased:

« … of thermal surface conditions, affecting the ground thermal regime ».

L14 Is the cubic meter per year and per meter of plateau circumference the common way to present such results? I am a bit baffled here. Should the normalization by length rather refer to length of erosion feature? How do you determine the circumference of an edge and is this really what you use to derive the value? Would it not make more sense to report the erosion as volume per year and feature length? Also, make sure to use the same writing of the unit throughout the text. Sometimes you use m3/m/a and sometimes m3/a/m.

Our study uses drone based photogrammetry to quantify lateral thermokarst degradation of the peat plateaus at submeter scale. Therefore, the type of results we provide is original and we have defined novel metrics to characterize the observed changes. We chose to quantify the microtopographic changes along time as a volume change per year and per linear meter of retreating plateau edge as it has a simple and intuitive physical meaning and it is directly connected to the raw data (e.g. derived only from the difference between two digital elevation models). At the same time, it can be directly computed from our idealized model setup. Nevertheless, we notice that we did not choose our words accurately because the idea the reviewer expressed is the one we wanted to convey. The volumetric rates (m^3 yr^{-1}) are indeed normalized by the length of the erosion feature in the horizontal direction that is orthogonal to the retreat direction. The word *circumference* seems particularly misleading so we replaced its occurrences by *« retreating edge (orthogonal to the retreat direction) »*. The metrics is now defined once in the abstract and once in the methods and later consistently referred to as « normalized annual volume changes ». The consistency issue in the units is now fixed and all results are now given in m^3 yr^{-1} m^{-1}.

L25 ff You only test snow depth rather than "snow pack characteristics". From this sentence one would expect various parameterizations of the snow pack tested. Also, the usability of the micro-scale findings are somewhat limited to be directly implemented into coarse resolution land surface models (as you mention later in the study yourself). You identify that your results will provide a basis to test parameterized models if they can represent the modelled patterns. Because you do not compare a CryoGrid version with and without snow redistribution the statement "[. . .] these results [..] highlight [. . .] the benefit of improving snow representation in numerical models for permafrost degradation projections." lacks a foundation. Rephrase please to match your results.

We understood the concern of the reviewer regarding the limited transferability of our work and removed the last part of the abstract containing the problematic phrasings. The new abstract now ends with more cautious opening perspectives:

« As snow depths are clearly correlated with ground surface temperatures, our results indicate that the approach can potentially be used to simulate climate-driven dynamics of edge degradation observed at our study site and other peat plateaus world-wide. Thus, the model approach represents a first step towards simulating climate-driven landscape development through thermokarst in permafrost peatlands. »

L42 14 million(s) square kilometers → done

L45 of (a) few meters → done

L46 low()lands (you use "lowlands" later on). Make expressions consistent and according to guidelines of TC. → done

L51 microtopography result(s) → done

L64 North(ern) Hemisphere → done

L65 plateau(s) degradation → done

L71-83 As you are advertising for implementation of snow redistribution into other (larger, integrated) numerical models it might be worth to mention such studies that rely on non-redistribution schemes. Note that this is not my research field so I cannot provide suggestions here. The only study on large-scale effects of snow cover I am aware of is "Effect of snow cover on pan-Arctic permafrost thermal regimes" (Park et al., 2015).

To our knowledge, no largescale permafrost study implements snow redistribution in a process-based way. We understand the point of the reviewer and added such a precision in the introduction when we describe the general challenges of permafrost modeling tied to microtopography:

« In models, the representation of this feedback between small-scale fluxes and dynamical topography is still in its infancy and large-scale permafrost modeling studies usually lack these processes (Park et al., 2015). »

L85-86 Here is the first time you mention the two years (2015 and 2018) that are used for the photogrammetry. It is not mentioned (here or later on) why you only use these two years. Have you not made surveys in the other years? Did you consider integrating them into your work? An elaboration on that point could be included in the methods/data section. Right now it is baffling to me why only the two years are selected.

These two years are the only years for which we have successfully created DEMS at the required accuracy.

Figure 1 is somewhat a replica of already published work, and shows monthly data for the climate diagrams. However, lines are drawn as some smoothed curves even though the data is (I assume) point-wise data. Replace the polynomial fit by points (plus connector line if need be) to actually represent the data that builds the basis for this figure (same applies to the min/max envelope). This also avoids some potential copyright issues.

This figure was entirely reworked and does not include smoothed polynomial fits anymore.

L109 Bleu – blue → The new figure comes with a new legend.

Figure 2 would benefit from the profiles that are used to calculate the HvsV index to appreciate the method and to derive a better idea of how someone would need to fit the profiles (it is a manual method so it requires some expert knowledge). → We added the profiles to the figure.

The points where the snow measurements were taken could be included as well. One of the questions that arise later on is if the snow depth scenarios in your model are representative for the actually observed values. It would be interesting to see where the measurements were made to give the reader the possibility to comprehend the snow depth variability related to the location in the landscape.

We added the snow measurement points to the figure.

L126-127 While the focus is clearly on the strong degradation of plateau edges in the range of tens of centimeters and more, some statistics on the precision/error should be reported. This information could be reported in the supplementary. Is the “arbitrarily” chosen 5 cm threshold within or outside the uncertainty?

We now include additional details on this point in the methods and use them for discussion. We also completed the discussion to further discuss uncertainties in our topographic reconstruction.

Methods (Section 3.1.)

« The average elevation difference between ground control points and the DEM are of 2.6 cm. To guarantee a meaningful subsidence signal, we only considered subsidence values exceeding 5 cm in this study. »

Discussion (Section 5.1.)

« Based on dGPS measured ground control points, the vertical accuracy of the drone-based DEMs is estimated to 2.6 cm (Sect. 3.1), but shadows, changing cloudiness or strong reflectance contrast near water bodies can create artefacts in the acquisitions, which locally might cause larger deviations. When comparing elevation difference between two DEMs, vegetation growth, the presence or absence of leaves and water level variations can add noise to the results. To account for these possible flaws when computing elevation differences, we only considered variations higher than 5 cm, which is double as the mean difference between the elevation of the ground control points (measured with a dGPS) and their counterpart on the DEMs (2.6 cm). This value finds good consistency with values from the literature (Forlani et al., 2018; Jaud et al., 2016). In comparison, our results show that actively degrading zones of the plateau are associated with subsidence values higher than 20 cm, than can reach 1 m and more. These values are significantly higher than the 2.6 cm average discrepancy

between the DEMs and dGPS measured ground control points, so that the DEM accuracy does not affect the volume changes strongly (Table 1). Yet the evaluation of elevation accuracy derived from this technique will benefit from additional studies producing similar results. »

L128 ff In Martin et al., 2019 in situ measurement locations are provided. Please indicate here also where measurements were made. This could be implemented in Figure 2. The reader wonders at this point how many measurements were taken and where. Is the number representative to capture the variability in snow cover (particularly on the plateau)?

Following the Reviewer's suggestion on Figure 2, the location of snow measurements was added to the figure. The variability in the snow cover over the plateau is shown in Martin et al. (2019). For the present study, we looked at points which clearly corresponded to peat plateau flat tops during three years in a row to match with our modeling setup, which reduced the number of observation points to six. While this does not capture the full distribution of snow depths on the plateau, we believe it provides an reasonable assessment of snow depth variability.

L132-133 I assume the dGPS measurement accuracy shall highlight that measurements of different years were made exactly at the same position. Clarify.

We added *« in all years »* in the sentence, which now reads:

« Snow depths were measured in the end of March with an avalanche probe at same points in all years, using a dGPS system to define the locations within 5–10 cm accuracy. »

L135 As mentioned earlier, I wonder why only two DEMs were used? What is with the DEMs of 2016 and 2017. Are they produced but not used? Would they not provide additional information and validation data for CryoGrid? Please clarify.

As detailed above, DEMs for these years are not available.

L140-142 It is the first time I read about this extension of an erosional unit to circumference. I assume there is a reason to do this and not use the "traditional" height or volume per area unit. Please specify. Is the aim to have an erosion value that is normalized by the feature length? How is the circumference of the - I assume - peat plateau calculated? Is circumference a good word to use here? Was this unit introduced by someone else (ref) and that is the reason why the word circumference is used?

We previously discussed this point in response to the comment L14. We have replaced the word "circumference" by "retreating edge (orthogonal to the retreat direction)".

L144 delineate → done

L147 "Small structures like palsas tend to sink entirely from the edge to the top [. . .]". Do you mean bottom or subside uniformly? How can it sink otherwise?

We used "uniformly" instead of "entirely". The sentence now reads:

« Small structures like palsas tend to sink uniformly from the edge to the top, while peat plateaus show stability of their top part and pronounced lateral retreat. »

L150 ff I had some problems understanding the concept because the connection of the text and the figure lacks some details. I think it would help a lot if you include the symbol "z" and "Δz" in the figure. The Δz could for example be included as cross-hatched area, and z in the bars. A y-axis labeled with height would also help. Right now, the reader has to infer that the bars represent height through the word "vertical motion".

We included the suggested modifications to the figure (Figure R1-2):

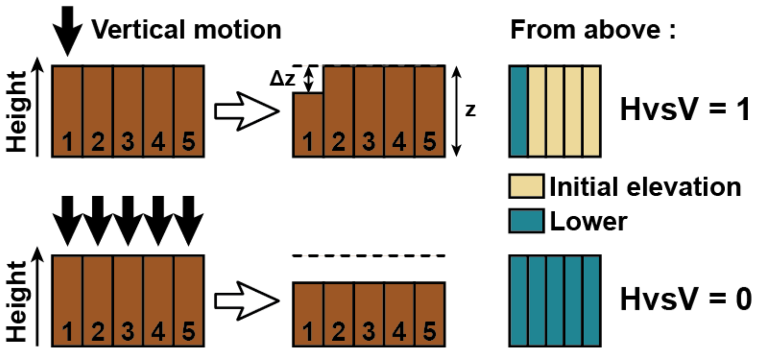

Figure R1-2. Presentation of the HvsV index used to quantify the observed and simulated degradation types. Reproduction of the fig. 4 of the main manuscript.

It would also greatly help to include the manual profiles in Fig.2 (panel B maybe). The method is not automated, so this information should be provided.

Following the suggestions for Figure 2, the profiles were added to the figure.

L160 ff Are here 10 bins used instead of 5? Edge retreat and slope adjustment will show the same value depending on bin sizes. Is this accounted for, and if yes, how?

The number 10 refers to the ten meter length of the simulated profile we used, not to the number of bins. To eliminate possible ambiguities, we wrote “meters” in full letters and added a reference to the 5 points later. The paragraph now reads:

« For the simulation results (Sect. 4.2), a 10 meter long window was used to capture the topography from the base of the plateau to its flat top. For these 10 meter profiles, the five required points were determined and the HvsV index was computed over three-year-long time periods. »

Figure 3 and the description had me struggling very hard to understand the concept of the HvsV index. Indicate “z” and “Δz” (e.g. cross-hatched area).

Following the Reviewer’s suggestion on Line 150, we included the modifications in the figure.

The index based on a max function contributed to that, rather than normalizing e.g. over the maximum elevation range in the studied area.

The max function is used so that the index values cannot go below 0. We displayed the function differently for more clarity:

$$HvsV\ shape\ index = \begin{cases} 1 - \dfrac{\Delta z_4 + \Delta z_5}{\Delta z_1 + \Delta z_2} & if\ \Delta z_4 + \Delta z_5 \leq \Delta z_1 + \Delta z_2 \\ 0 & otherwise \end{cases}$$

L168ff You say that the heat transfer is modelled in 1D but you also state in line 189-190 and 195 that tiles exchange heat between each other. How does that fit together with the 1D heat transfer? Please explain.

We added the following sentence to the paragraph:

« As described in detail in Nitzbon et al. (2019), the lateral heat flux calculation is based on the temperature gradient between neighboring cells of different tiles. If topographic differences expose the side of a tile, lateral heat fluxes between the tile and the atmosphere are not taken into account. »

L173 Processes → done

L179 Add information to the “bucket scheme”, e.g. water drains based on a recession coefficient or any other more scientific description.

We added the following lines:

« Computation of dynamic soil moisture is accomplished with a bucket scheme (Martin et al., 2019; Nitzbon et al., 2019), in which each grid cell can hold water up to its field capacity, while excess water is moved to the next grid cell, until a water table on top of the permafrost (or bedrock layer) is reached. »

L182 Does "natural porosity" refer to the measured porosity? It is unclear to me.

Natural porosity refers to the porosity of the soil without excess ice, observable when the soil is fully thawed (Daanen et al., 2011; Hopkins, 1949; Seguin and Frydecki, 1994). We added this information in the main text, section 3.3.1. (The CryoGrid3 model):

« The subsidence calculation is based on soil stratigraphy, in particular volumetric ice contents and natural porosity, i.e. the porosity of the soil matrix in unfrozen conditions. »

Additionally, we now use the definition of excess ice from the National Snow and Ice Data Center in the introduction:

« In many cases, the microtopography results from the presence of excess ice in the ground, i.e. the volume of ice which exceeds the total pore volume that the ground would have under unfrozen conditions (NSIDC glossary), so that permafrost thaw results in surface subsidence... »

L200 Replace (cf Introduction) with appropriate references. I would assume that the references of line 76-77 are meant but they focus on microtopography.

We now refer to Gisnås et al., 2014; Martin et al., 2019; Sannel, 2020 and Sannel et al., 2016. Note that this paragraph has been relocated to follow suggestions from the other reviewer.

L201 Plateaus → done

L205-206 Explain how snow is transported systematically. Explain how the threshold depth of snow is determined. This is quite interesting also with respect to the slope adjustment. Is snow redistributed to the slope or directly to the mire, and is the total snow amount conserved?

We reworked the paragraph explaining the lateral snow scheme to fit the requirements of both reviewers. It now reads as follows:

« 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. »

L211 we focus on modelling the → done

L212 climate forcing of the period with field observations → done

L214 over a three year period → sentence reworked.

L215ff If you place the snow measurement results (L323-329) in the materials section, it will become much clearer what these lines are referring to and what "considerable spread" means. L323-329 feels anyways a bit out of place where it is located right now.

We have now placed the snow results in the Material and methods section. Because the snow measurements are described at the end of section 3.1. (section for field measurements), we have moved the snow results to this place. This modified the number of the subsequent figures.

L221 ff In order for anyone to repeat your modeling it requires to know the parameters. These could be provided in the supplement or a table. Did you change anything in the parameterization from your previous study? This might be expected in order to achieve steady-state conditions when using warmer and wetter climatological forcing.

We used the same parameters as in Martin et al. (2019). They are now detailed in a table in the Appendix D.

L225 I am not familiar with the term and meaning of "translational symmetry" in a model setup. Is it possible to describe this with simpler words or an addition to let readers further away from the topic know what this means and why it is important?

We rephrased and completed this part which now reads:

« As a fully three-dimensional simulation of these phenomena is beyond the capability of our model approach, we focus on the simplified situation of a laterally homogenous peat plateau edge, for which all fluxes in the third spatial dimension are assumed zero (translational symmetry). »

L226 Do you really mean "transects"? In Fig.2 I can only see the selected study areas. It would be good to actually see the transects in Fig.2.

We added the transect in Figure 2 as previously suggested.

L229 Is the "7 m" mineral layer displayed out of scale in Figure 4? This statement causes confusion.

We added to the figure an arrow oriented downward and mentioning that the silt layer extends over 7 m. We also added a sentence in the figure caption saying:

« The bottom part of the setup has been truncated because it consist of silt over 7 meters for all tiles. ».

L236 Rephrase.

The two sentences now reads:

« The peat plateau tiles are 0.3 m wide, so that the initial width of the plateau amounts to 11.1 m. They contain the same total amount of peat above the mineral base layer as the mire tiles, but include additional excess ice, which increases their surface elevation. In line with observations (Table 1), the initial excess ice content is adjusted so that the flat top of the plateau is located 2 m above the wet mire at 302 m a.s.l.».

L237 ff This reads awkward. Rephrase. How is the "constant" slope arbitrary if the other features have fixed widths?

We reworked the model setup description. Explanations about the initial slope are now in the caption of the setup figure and read:

« The surface elevation was linearly interpolated between 300 and 302 m a.s.l. over a lateral distance of 2.4 m to represent a typical geometry of peat plateau edge. ».

L242ff What is the corresponding total ice excess volume in this case? Can you put this into perspective of literature values and for future implementation into large scale models. Is the value reasonable? Or is it too high because soil erosion could account for a significant part of reaching the base elevation after complete melting and draining the ice?

For comparison purpose, we adapted a commonly used equation for soil samples to our model setup to calculate the proportion of excess ice. We do not have field data of the excess ice content and were unable to find some specific ones for peat plateaus in the literature. Yet, literature values for various type of soils and sediments range between 0 and 85%, from which we suggest our value is reasonable. We therefore added the following lines to the main text of section 3.3.3:

« Our setup leads to an initial excess ice content of 47% (volume of excess ice / volume of unfrozen soil) in the plateau, which is in the range of commonly reported field values (Bockheim and Hinkel, 2012; Kokelj and Burn, 2003; Lacelle et al., 2013; Morse et al., 2009; Subedi et al., 2020). *»*

Also is the unfrozen peat thickness based on measurements?

The point is explained in the Input Parameter section (3.3.3.). Similarly to Martin et al. (2019), the soil stratigraphy used in the model is based on field measurements of the peat plateau site of Iškoras located 40 km east from the site of the present study.

L250 ff For this we use the . . . State briefly what options were used. Do not expect to have the reader go through these publications in detail to find out the specific points. Clarify what was changed very briefly and why; what do the changes account for?

We developed important aspects of the atmospheric simulation. The reference to Martin et al. (2019) has been removed, but we kept the one to Aas et al. (2016), so that an interested reader can refer to the technical details of the model setup there. The paragraph is now as follow:

« As presented in Martin et al. (2019), we use model forcing for the hydrological year 2015/2016, that have been compiled by dynamical downscaling of the ERA Interim reanalysis (Dee et al., 2011) with the Weather Research and Forecasting model (WRF v.3.8.1; Skamarock & Klemp, 2008). The WRF model was run in two nested domains with 15- and 3-km grid spacings from August 2015 to July 2016. To generate the model forcing for CryoGrid3, we used 3-hourly output from the nearest grid point in the 3-km domain. The other model parameters for WRF were selected as in Aas et al. (2016), with the exception of slightly higher vertical resolution (45 model layers compared to 40) and excluding the CMB glacier module. ».

L253-254 Rephrase. Make it quantitative. Maybe merge with next sentence.

As developed in our response to the general comment, this part was entirely reworked.

L256ff The special conditions are surely interesting to investigate how the warmer and wetter climate affects the ground thermal evolution but they are certainly also difficult to use to achieve steady-state conditions in your spin-up. I wonder why you have not used the climate average as forcing, or tried to scale the 2015-2018 period to the normal. This setup makes it also difficult in the end to draw general conclusions. I am not aware of how much work the individual steps needed for the modelling involve. Would it be feasible to have a spin up test run with either WRF output of more normal years, or with adjusted values of your chosen period to match the normal?

We understood this is a sensitive point for both reviewers. In our response to the general comment of the reviewer 1, we provided a detailed answer on the role of this year of climate forcing on our results. Besides the production procedure of climate forcing based on regional modelling is computationally demanding. This year is the only one we have, and unfortunately we currently don't have the computation capacity to produce more years with WRF. We have selected the available year for consistency with Martin et al., 2019, because the present work directly builds on their simulations of and observations of the ground thermal regime.

L261 Do you use the 2015-2018 period as one block and cycle over this period, i.e. 33.3 iterations for both spin-up and actual run? Please clarify.

We use only one year, i.e. the 2015-2016 hydrological year, as forcing. As previously discussed, the section on climatic forcing was strongly reworked for clarity, in accordance with both reviews.

L268 Rephrase. The 3 m thick peat soil layer has 5% mineral and 15% organic material total volumetric content and a porosity of 80%. It is. . . → done

L279 The parameters should as well be listed with the other parameters.

Following a previous comment, the model parameters are now listed in the Appendix D of the manuscript.

L292 1.2% (of) the . . . → done

L295 ff How do you determine the edge? Please specify in the methods. This shall serve for repeatability by others.

This point is detailed in section 3.2:

« Because elevation changes occurred in the mire due to water level variations between the two dates, we relied on an estimation of the elevation of the plateau edge inflection point (around 309.7-309.8 m asl, yellow color on Fig. 2B) to delineate the plateau from the mire and thus identify elevation changes associated with the plateau. »

L301 mineral soil(s) at . . . → done

L304 The explanation of normalization by structure length should come already in the methods

We rephrased the corresponding method part to be more explicit:

« Based on the changes of elevation and lateral extent of the plateau between 2015 and 2018, we used the eight edge transect areas (Fig. 2) to calculate the normalized annual volume change (the annual volume change normalized by the length of the retreating edge orthogonal to the retreat direction, unit: m^3 yr^{-1} m^{-1}). »

L306 Horizontal ground subsidence sounds strange. Consider using edge degradation (also used in the figures) vs uniform subsidence. Make sure to adjust other instances for consistency. → done

Figure 5 right panel could include the transects for the determination of the HvsV index as well.

Done

L323-329 As mentioned earlier, I think these results could be presented in materials. For Figure 7 it would be easier to have bar graphs next to each other for each class. At the moment, it is unclear if the bars are stacked or overlay – the y-axis suggests overlay.

The section and corresponding graph were relocated accordingly.

L332-333 Rephrase [. . .] as is observed in the DEM differencing. → done

L334-335 It is not clear in the methods section how the snow is redistributed. Is the total amount taken and evenly spread out in the mire? Please clarify in the methods.

Following the suggestions from both reviewers on this issue, the Method section on snow was reworked (section 3.3.2.). See the response to the comment L. 205-206 above.

L343 [. . .] during which the slopes gradually decrease over time. → done

L360-361 The unit should be explained sufficiently in the methods so that there is no need anymore to do it here.

We implemented the above mentioned terminology modifications following previous recommendations from the reviewer and removed the explicative parenthesis. We consistently refer to « normalized annual volume changes », or simply to « volume changes » if the term appear several times in the same paragraph.

L363 phases show(s) → done

L391 Normalized by structure width or structure length? Should be explained in the methods.

We replaced "normalized to structure width" by "per meter of retreating edge" to be consistent with the new terminology modifications suggested by the reviewer.

L401 Would it make sense to replace "chaotic behavior" with complex/heterogenous responses or similar expression?

We rephrased:

« This pattern highlights the highly complex and irregular behavior of ice-rich permafrost landscapes…»

L402 Include reference again here. Try to close the opened issues (references) from the introduction.

We added a reference to Osterkamp et al. (2009) and to Nitzbon et al. (2019). The first study describes this feedback from field observations and the second one is one of the first implementations of this feedback in a numerical model.

L406 Non-homogeneous ice content in the soil might be another big aspect?

We agree with the reviewer and included it at the end of the paragraph with the following lines:

« Finally, the distribution of the excess ice in the ground plays an important role for the timing and magnitude of subsidence. Heterogeneous excess ice distribution throughout the plateau may be an important driver of the observed spatial variability of the edge degradation. »

L407-408 Rephrase and maybe split sentence. Sentence is not clear.

We split the sentence and rephrased:

« Furthermore, heat transfer between the wet mire and the plateau is likely influenced by the geometry of the plateau-mire interface. As an example, zones 1, 2, 4 and 6 belong to convex features of the plateau edges and show particularly high subsidence rates. »

L414-416 It is not clear what you want to say here. Also, the ending of the sentence does not say anything. Clarify.

We agree that this formulation was not clear and have reworked the section. It now starts with a discussion on topographic uncertainties based on a previous point from the reviewer. The paragraph ends on a rephrased version of our initial point on the delineation of the plateau:

« Additionally, as described in Sect. 3.2, we acquired the volume changes for the plateau based on an estimation of the elevation of the inflection point of its edge, from which we derived its contour in the 2015 and 2018 DEMs. In case of high vegetation and uneven or gentle slopes, this method to delineate the peat plateau contours can introduce additional uncertainty. However, we carefully checked that this was not the case for the sections analyzed in Table 1. »

L417ff The whole passage on the method should be in the method section. Then you also do not break the reading flow in the discussion.

This paragraph was moved to Appendix E.

L439-440 Rephrase last part of sentence (field comparisons). I do not understand the sentence.

We rephrased as follow:

« Among the different degradation phases (Initial Slope Adjustment, Constant Edge Degradation and Plateau Collapse), the CED phase is most relevant for the comparison to field observations, as it is characterized by steady edge retreat in response to the steady state climate forcing, while the bulk of the peat plateau remains stable. On the other hand, The ISA phase is essentially an adjustment to the change in snow depth conditions from the no snow scenario used for initialization to the scenarios with non-zero snow depth, which are characterized by edge retreat. »

L446-447 I miss the context of the symmetry. What does translational or rotational geometry refer to and why is this important? For someone not familiar with your model this is very confusing. How does a translation (of what) relate to the modeling? Can you explain it in simpler words? Do you mean that heat transport on both sides of a palsas would need to be taken into account?

We have rephrased this paragraph for clarity. By exploiting natural symmetries in a system, it is possible to simplify the model representation in two dimensions, which strongly reduces the computational effort. With translational symmetry, we mean that the modeled peat plateau edge does not change in the third spatial dimension, which means that it can be described by the 2D-section presented in the manuscript. In nature, this would correspond to the idealized situation of a very long straight edge. However, rounded palsas, especially small ones, are better represented by cylinders, as done in Aas et al. (2019). Regarding terminology, we realized that "rotational symmetry" is not specific enough in 3D, and have updated the term to "cylindrical symmetry". The paragraph in question now reads as follow:

« As palsas are often small rounded peat bodies, the assumption of translational symmetry inherent in our model setup (Fig. 5) is not valid. For these features, simulations should be performed for cylindrical symmetry, which better describes the geometry of small palsas (as done in simulations by Aas et al, 2019). »

L452-456 Is the agreement only qualitative (coldest temperatures at lowest snow cover) or is it matching the actual values as well? Provide values to give more confidence in your results. This could be extended also. At this point there has not been any validation presented other than that the plateau is not degrading with low snow cover.

Temperature comparison between loggers and simulations for different snow cover are provided in Appendix A to establish the robustness of the simulations, with references to the appendix in the result part and in the previous part of the discussion (5.2.1). However, we do not have field observations that clearly show that high snow depths lead to a faster edge retreat, although the effect of the snow cover on ground surface temperatures (which propagate to deeper ground layers) is clear.

« However, the comparison between model results and observations clearly shows that the numerical model framework can capture the correct order of magnitude of the degradation processes, while also reproducing key patterns in the observed ground temperature regime (Appendix A, Fig. A1). »

L462-464 The higher snow depths at the transition between the slope and the mire are not modelled. This raises the question how the lateral heat transfer is affected. This extends the comment on how the heat transfer is modelled laterally (1D vs. tile interactions).

As answered to the comment on L168, more information is now provided on the lateral heat flux scheme, with details on this particular case (see above).

Are the maximum snow depths on the edge also associated with water drainage from the plateau top that would add yet another additional positive feedback for thawing? These pieces of information should be in the methods.

The mire near the plateau edge is already wet because it is hydrologically connected to the rest of the mire in which the water level is controlled by the hydrological reservoir (see model setup, Fig. 5, formerly Fig. 4). Therefore, water inflow from the plateau only has a marginal contribution to the water content of this part of the mire.

L468-470 References missing. Is this of significant importance though? You have not made any analysis of how this could affect the model results. If you list this, provide evidence why this would have an impact.

We follow these recommendation to modify the text:

« Furthermore, our model assumes a fixed snow density and thus snow thermal properties, while snow densities in reality vary with e.g. snow depth and time. A density increase from 200 to 300 kg m^{-3} may correspond to a doubling of thermal conductivity, depending on the snow type (Sturm et al., 1997). »

L472 What is kinematic metamorphism and why would anything kinematic be stronger with a thinner snowpack. Is this a reference from Domine 2016? I did not find the reference for kinematic morphism there. Please clarify, maybe simplify? Also, rephrase (less colloquial) "[. . .], notorious and a real difficulty [. . .]"

We apologize for erroneous terminology. We wanted to refer to kinetic metamorphism, not kinematic. The paper from Domine et al. (2016) mention depth hoar crystals which are produced by kinetic metamorphism although they do not mention this phenomenon directly. Therefore, we rephrased the sentence, added explanations and updated the references as follow:

« Measurements of snow density in Šuoššjávri showed that the snow on top of palsas is slightly less dense than in the mire. This could be due to a thinner snowpack leading to greater kinetic metamorphism (snow metamorphism driven by strong temperature gradient in the snowpack) and the formation of depth hoar crystals, which are characterized by high porosity and high effective thermal conductivity (Colbeck, 1982; Schneebeli and Sokratov, 2004)*. »*

L472-474 What would be the concrete implications. This is all quite vague. Could you pick up questions from the introduction about the motivation on generalization for large-scale models? This might be more interesting than listing related but not tested issues in thermal conductivity of snow. Or discuss how the uncertainties could affect your results.

This paragraph discusses the limitation of our approach regarding its ability to simulate the thermal insulation of snow given the natural complexity of this phenomenon. We want to point that in the nature several processes are at work and interacting (compaction, metamorphism…) which are challenging to represent numerically, therefore limiting the accuracy of our study. The paragraph has been reworked for more clarity (quoted in the previous comment and the references updated).

L486-487 The study demonstrates the impact of snow height. Any other thing is implied. If you had run the model with different climate forcing, e.g. mean climate, it would substantiate the claim of different climate forcings. As it is, I find it only justified to address the snow height and mention that this implies impacts of different climate forcing. As also mentioned in the Data section, spin-ups with different years (ERA Interim) would substantiate the other claims. This would probably be far off the scope of this work. But you should therefore also not make claims that are not substantiated by the present work.

We agree with the reviewer on this point. We do not claim any results regarding a warming climate and climate change. However, snow depth variations drive surface temperature variations, therefore our setup shows a sensitivity of the plateau degradation to surface temperature. We think it is a relevant indicator of the fact that our approach would be suited to study the impact of climate change on the plateau. Because it is not a results, we make appear such a consideration in the discussion. To make sure there is no ambiguity on this point, we have reworked the following sentences:

Section 5.3.1.

« Our experiment shows that snow depth <u>*alone*</u> *can drive important surface temperature changes and permafrost disappearance. This result illustrates that permafrost disappearance is not only a function of temperature (Chadburn et al., 2017) and that that plateau systems can react sensitively to different climatic parameters affecting surface temperature. »*

Section 5.3.2.

« In our experiment, the modelling scheme shows a sensitivity of the plateau retreat to different surface temperatures resulting from the different prescribed snow depths. Because other climatic parameters than snow depth can affect surface temperature, this indicates that our scheme may also be able to simulate the plateau response to a temperature increase, paving the way for climate change simulations.»

L490 consistent(ly) with → done

L491-492 New sentence. Moreover, this decrease . . . by a strong . . .

Done, we used "Nevertheless" instead of "Moreover".

L492 The last part of the sentence is unclear. Rephrase.

We changed "remain unclear" for "is difficult to anticipate".

L492-495 What are the implications of the 1D representation? The slopes are subject to lateral effects with decreased snowcover and thus higher thermal conductivity laterally. Can this be somehow addressed or estimated? Are there other studies available that can be used to infer the impact? Under the assumption that there is a strong impact how would that result in erosion?

This point is addressed in the next paragraph of the discussion, where we discuss how perturbations creating local snow accumulation can trigger massive permafrost degradations. Reference from the literature are given there to illustrate this idea (roads, fence experiments).

L496 "implementations" means snow redistribution or other things too? Be more specific.

We added "including subsidence and lateral fluxes" to the sentence.

L496ff Would it be feasible to include e.g. a figure that shows how the heat transfer in and out of the peat plateau changes if the snow is redistributed? It feels that a lot of the discussion is lacking a quantitative basis related to the changes in heat transfer with different snow cover heights.

On annual timescales, the impact of snow depth (and thus the effect of snow redistribution) on the mean annual ground surface temperature of the plateau is displayed in Figure A1 (reproduced below). It is thus a direct and quantitative graphical assessment of how spatially different snow depths drive vertical heat loss from the plateau. On shorter timescales, the effect of spatially different snow depths on the ground thermal regime is a complex question because of the seasonal dynamics of the snowpack. As such, it involves many factors such as the timing of the first snow fall in relation to ground freezing, the duration of the snow season, and the temporal distribution of snow fall during the snow season. All these questions need to be accounted for to discuss how heat transfer in and out of the plateau is affected by snow redistribution and for this reason we do not think that it can be represented in a simple graph. However, most of the mentioned processes are included in our snow model, so that our simulations of edge retreat offer a clear picture of the overall effect of the snow cover.

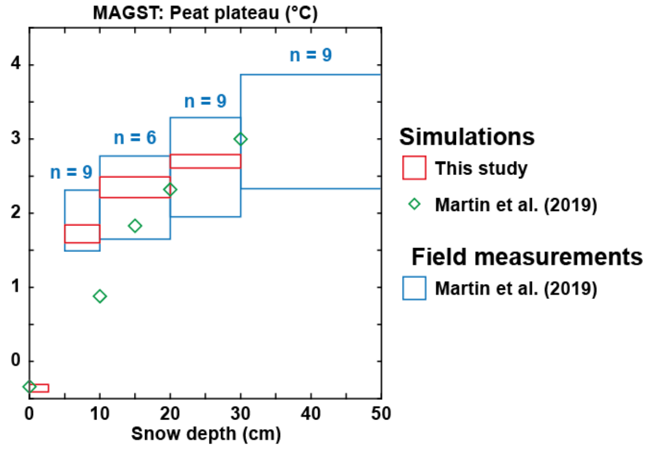

Figure R1-3. Extracted from Fig. A1. Mean Annual Ground Temperatures (MAGST) on the peat plateau as they are simulated in this study and in Martin et al. (2019) compared to the field measurements from Martin et al. (2019) for the same region. Values indicated with the letter n correspond to the number of field observations in Martin et al. (2019). 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 values span over the mean ± 1 standard deviation range for both observations (variability among observations) and simulations (variability among the tiles of a simulation).

L499-503 A lot of details for the general idea that snow heights affect the heat transport. Are there more specific details on lateral snow redistribution or generalized findings about lateral heat transfer that would more directly relate to your study?

As stated in the first line of the paragraph, these lines illustrate the positive feedback between surface subsidence and snow depth patterns caused by wind redistribution. As the surface subsides, more snow can accumulate, which insulates the ground surface more from cold winter conditions, which in turn increases excess ice melt and thus subsidence. The paragraph has been rephrased for clarity:

« The presented model approach (including excess ice and small-scale representation of lateral fluxes) is clear evidence of the importance of small-scale thaw feedback mechanism on permafrost degradation. The feedback between the dynamic microtopography and the lateral fluxes of water, heat and snow shows how a limited increase in snow cover (e.g. from the 10-20 cm to the 20-30 cm snow scenario) results in a strongly increased degradation rate. This sensitivity to small perturbation has been observed in a range of permafrost settings, when artificially increasing snow depth with a fence (Hinkel and Hurd, 2006)*, when building linear road infrastructures (Schneider von Deimling et al., 2020) or due to heavy vehicle traffic in Alaskan lowlands (Raynolds et al., 2020). »*

L507-509 You have not made any tests with different convexity/concavity and uneven edge outlines. Include a basis for these assumptions in form of a reference(s). In connection with a previous comment, maybe you quantify the heat transfer and snow distribution under different evolution phases (slope adjustment to plateau collapse) and use these results as a basis for these statements.

To our knowledge, modeling studies have not yet investigated this point. Yet it is supported by our observations, as discussed earlier in section 5.1. To make this point clearer we rephrased:

« In addition to small-scale variations of ground stratigraphy, excess ice contents and plateau heights, our observations suggest that the irregular plateau outline with both concave and convex shapes affects the lateral fluxes of heat, water and snow, which in turn exert a control on the edge dynamics (Sect. 5.1). While computationally demanding, our multi-tile approach could be embedded in an ensemble framework to represent a range of edge geometries and other critical parameters, yielding a range of different degradation scenarios and therefore capture the high spatial variability of subsidence at the plateau scale. »

L519-520 (used to) -> that simulate → done

L533 LSMs?

We agree that Land Surface Models are the models that represent the surface in Earth System Modeling frameworks. Because we want to give the general context of ESM in our sentence we rephrased:

« In ESM frameworks, implementing a multi-tile approach for the land surface scheme is challenging due to its complexity and computational demands. »

L519ff Do you mean land surface models that are used for climate models? Landscape evolution models (Landlab etc.) include 2D representations but do not account for 3D (depth) effects. How could microtopography be parameterized? Even if a two-tile approach is chosen, this would still require a significant high spatial resolution. What is the basic idea to derive a parameterization from a multi-tile super high resolution (microtopography) approach?

Yes, we mean LSMs as used in ESMs and not landscape evolution models, as developed in the previous lines. L519ff ends on a reference to Andresen et al. (2020, The Cryosphere) "Soil moisture and hydrology projections of the permafrost region – a model intercomparison". The intercomparison compare "broadly used land models with permafrost processes" and none of them implement thermokarst, as stated by the paper. We think this is an important context for our study. To be more complete, we added a reference to Burke et al. (2020, The Cryosphere) which also present a LSM intercomparison dedicated to permafrost physics in which none of the models include thermokarst. Further discussions on possible implementations for the future are also discussed. We reworked the paragraph which now reads:

« Over longer timescales, on the other hand, this process leads to the reshaping and finally the complete collapse of the entire peat plateau. In ESM frameworks, implementing a multi-tile approach for the land surface scheme is challenging due to its complexity and computational demands. Yet, parameterized approaches could eventually be developed, based on sensitivity tests with future generations of higher-complexity multi-tile frameworks (Sect. 5.3.2). In particular, future studies should investigate to what extent the two-tile approach demonstrated by Aas et al. (2019) can emulate the results of a multi-tile model, especially when not only applied to single sites, but to the entire sub-Arctic where peat plateaus occur today. However, our multi-tile setup clearly produces a different thaw dynamics at the scale of individual sites, which might affect the modeled carbon balance. To investigate this issue further, a multi-tile model coupled to a carbon cycling scheme would be required. »

L536-538 Why is the average output of interest here? This connects to previous comments on your use of three years rather than the climatic mean conditions also. Could it be of higher interest to explore first the capability of the model to capture the landscape response for a wider range of climatological forcing? Would that not lead to more confidence in the capability of the model at first, before testing and training an empirical model to a "half-validated" physically-based one? As you said, you are focusing only on three years that are rather on the extreme side of climate (wet and warm). Before looking for a simplification in form of an empirical model that would act as a surrogate for the complex one, the first one should be well-validated. The discussion could pick up on these aspects a bit more. Depending on effort and available downscaled ERA Interim data it could be worth having a look at how a model run performs with colder years and how snow depth is then affecting the ground thermal evolution.

The limitations of our approach have been outlined in the previous sections (5.2.1, 5.2.2). At this stage of the manuscript (the last lines of the discussion), we provide general recommendation on possible ways to improve large-scale predictions, which have been made more concrete in future studies. In the approach by Aas et al. (2019), the entire plateau subsides as a whole, when a climatic threshold is exceeded so that the edge cannot retreat gradually over time as in our simulations. For large-scale applications with many grid cells located across a climatic gradient, there is at least a possibility that the approach produces reasonable dynamics for a warming climate. First, the warmest plateaus would subside, gradually followed by colder ones, which could provide a smooth curve for the subsided volume over time, similar to what we obtain from our more sophisticated model. Modifications were brought to the discussion in this regard (Sect. 5.3.3):

« Yet, parameterized approaches could eventually be developed, based on sensitivity tests with future generations of higher-complexity multi-tile frameworks (Sect. 5.3.2.). In particular, future studies should investigate to what extent the two-tile approach demonstrated by Aas et al. (2019) can emulate the results of a multi-tile model, especially when not only applied to single sites, but to the entire sub-Arctic where peat plateaus occur today. However, our multi-tile setup clearly produces a different thaw dynamics at the scale of individual sites, which might affect the modeled carbon balance. To investigate this issue further, a multi-tile model coupled to a carbon cycling scheme would be required. »

We agree that the idea of exploring other (colder) years in the same steady state approach was not mentioned as well as the idea that our presented work is a new step in an ongoing process. To correct this, we renames section 5.3.2 « Future model improvements », listed a number of important improvements and added the following sentence at the end of the first paragraph:

« Further sensitivity tests with steady state climate forcing should focus on the role of air temperature (colder/warmer), total precipitation and excess ice contents on peat plateau stability and lateral thermokarst patterns. »

L553 Steady state climate forcing, using warmer and wetter years than the normal. The generalization requires that you show it does not matter that your climate forcing is rather extreme.

As discussed previously, thanks to the reviewer comments, we identified the importance of discussing the peculiarities and representativity of our forcing. As previously developed, we think that that the relation between snow depth and degradation speed our simulations produce are climate- and setup-dependent. Therefore, we gave more context on the year 2015-2016 in the light of the last decade and discussed the potential impact of a warm year on the results in Section 5.2.2.

Section 5.2.2:

« 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 (see Sect. 5.3.2) in future studies. »

References

Aas, K. S., Dunse, T., Collier, E., Schuler, T. V., Berntsen, T. K., Kohler, J. and Luks, B.: The climatic mass balance of Svalbard glaciers: a 10-year simulation with a coupled atmosphere–glacier mass balance model, Cryosph., 10(3), 1089–1104, doi:10.5194/tc-10-1089-2016, 2016.

Aas, K. S. K. S., Martin, L., Nitzbon, J., Langer, M., Boike, J., Lee, H., Berntsen, T. K. T. K. and Westermann, S.: Thaw processes in ice-rich permafrost landscapes represented with laterally coupled tiles in a land surface model, Cryosph., 13(2), 591–609, doi:10.5194/tc-13-591-2019, 2019.

Andresen, C. G., Lawrence, D. M., Wilson, C. J., McGuire, A. D., Koven, C., Schaefer, K., Jafarov, E., Peng, S., Chen, X., Gouttevin, I., Burke, E., Chadburn, S., Ji, D., Chen, G., Hayes, D. and Zhang, W.: Soil moisture and hydrology projections of the permafrost region – a model intercomparison, Cryosph., 14(2), 445–459, doi:10.5194/tc-14-445-2020, 2020.

Bockheim, J. G. and Hinkel, K. M.: Accumulation of Excess Ground Ice in an Age Sequence of Drained Thermokarst Lake Basins, Arctic Alaska, Permafr. Periglac. Process., 23(3), 231–236, doi:10.1002/ppp.1745, 2012.

Burke, E. J., Zhang, Y. and Krinner, G.: Evaluating permafrost physics in the Coupled Model Intercomparison Project 6 (CMIP6) models and their sensitivity to climate change, Cryosphere, 14(9), 3155–3174, doi:10.5194/tc-14-3155-2020, 2020.

Chadburn, S. E., Burke, E. J., Cox, P. M., Friedlingstein, P., Hugelius, G. and Westermann, S.: An observation-based constraint on permafrost loss as a function of global warming, Nat. Clim. Chang., 7(5), 340–344, doi:10.1038/nclimate3262, 2017.

Colbeck, S. C.: An overview of seasonal snow metamorphism, Rev. Geophys., 20(1), 45, doi:10.1029/RG020i001p00045, 1982.

Daanen, R. P., Ingeman-Nielsen, T., Marchenko, S. S., Romanovsky, V. E., Foged, N., Stendel, M., Christensen, J. H. and Hornbech Svendsen, K.: Permafrost degradation risk zone assessment using simulation models, Cryosph., 5(4), 1043–1056, doi:10.5194/tc-5-1043-2011, 2011.

Dee, D. P., Uppala, S. M., Simmons, A. J., Berrisford, P., Poli, P., Kobayashi, S., Andrae, U., Balmaseda, M. A., Balsamo, G., Bauer, P., Bechtold, P., Beljaars, A. C. M., van de Berg, L., Bidlot, J., Bormann, N., Delsol, C., Dragani, R., Fuentes, M., Geer, A. J., Haimberger, L., Healy, S. B., Hersbach, H., Hólm, E. V., Isaksen, L., Kållberg, P., Köhler, M., Matricardi, M., McNally, A. P., Monge-Sanz, B. M., Morcrette, J.-J., Park, B.-K., Peubey, C., de Rosnay, P., Tavolato, C., Thépaut, J.-N. and Vitart, F.: The ERA-Interim reanalysis: configuration and performance of the data assimilation system, Q. J. R. Meteorol. Soc., 137(656), 553–597, doi:10.1002/qj.828, 2011.

Domine, F., Barrere, M. and Sarrazin, D.: Seasonal evolution of the effective thermal conductivity of the snow and the soil in high Arctic herb tundra at Bylot Island , Canada, , 2573–2588, doi:10.5194/tc-10-2573-2016, 2016.

Forlani, G., Dall'Asta, E., Diotri, F., Cella, U. M. di, Roncella, R. and Santise, M.: Quality Assessment of DSMs Produced from UAV Flights Georeferenced with On-Board RTK Positioning, Remote Sens., 10(2), 311, doi:10.3390/rs10020311, 2018.

Gisnås, K., Westermann, S., Schuler, T. V., Litherland, T., Isaksen, K., Boike, J. and Etzelmüller, B.: A statistical approach to represent small-scale variability of permafrost temperatures due to snow cover, Cryosphere, 8(6), 2063–2074, doi:10.5194/tc-8-2063-2014, 2014.

Hinkel, K. M. and Hurd, J. K.: Permafrost destabilization and thermokarst following snow fence installation, Barrow, Alaska, U.S.A., Arctic, Antarct. Alp. Res., 38(4), 530–539, doi:10.1657/1523-0430(2006)38[530:PDATFS]2.0.CO;2, 2006.

Hopkins, D. M.: Thaw Lakes and Thaw Sinks in the Imuruk Lake Area, Seward Peninsula, Alaska, J. Geol., 57(2), 119–131, doi:10.1086/625591, 1949.

Jaud, M., Passot, S., Le Bivic, R., Delacourt, C., Grandjean, P. and Le Dantec, N.: Assessing the Accuracy of High Resolution Digital Surface Models Computed by PhotoScan® and MicMac® in Sub-Optimal Survey Conditions, Remote Sens., 8(6), 465, doi:10.3390/rs8060465, 2016.

Kokelj, S. V. and Burn, C. R.: Ground ice and soluble cations in near-surface permafrost, Inuvik, Northwest Territories, Canada, Permafr. Periglac. Process., 14(3), 275–289, doi:10.1002/ppp.458, 2003.

Lacelle, D., Davila, A. F., Fisher, D., Pollard, W. H., DeWitt, R., Heldmann, J., Marinova, M. M. and McKay, C. P.: Excess ground ice of condensation–diffusion origin in University Valley, Dry Valleys of Antarctica: Evidence from isotope geochemistry and numerical modeling, Geochim. Cosmochim. Acta, 120, 280–297, doi:10.1016/j.gca.2013.06.032, 2013.

Martin, L. C. P., Nitzbon, J., Aas, K. S. S., Etzelmüller, B., Kristiansen, H. and Westermann, S.: Stability Conditions of Peat Plateaus and Palsas in Northern Norway, J. Geophys. Res. Earth Surf., 124(3), 705–719, doi:10.1029/2018JF004945, 2019.

Morse, P. D., Burn, C. R. and Kokelj, S. V.: Near-surface ground-ice distribution, Kendall Island Bird Sanctuary, western Arctic coast, Canada, Permafr. Periglac. Process., 20(2), 155–171, doi:10.1002/ppp.650, 2009.

Nitzbon, J., Langer, M., Westermann, S., Martin, L., Aas, K. S. and Boike, J.: Pathways of ice-wedge degradation in polygonal tundra under different hydrological conditions, Cryosph., 13(4), 1089–1123, doi:10.5194/tc-13-1089-2019, 2019.

Osterkamp, T. E., Jorgenson, M. T., Schuur, E. A. G., Shur, Y. L., Kanevskiy, M. Z., Vogel, J. G. and Tumskoy, V. E.: Physical and ecological changes associated with warming permafrost and thermokarst in Interior Alaska, Permafr. Periglac. Process., 20(3), 235–256, doi:10.1002/ppp.656, 2009.

Park, H., Fedorov, A. N. and Walsh, J. E.: Effect of snow cover on pan-Arctic permafrost thermal regimes, , 2873–2895, doi:10.1007/s00382-014-2356-5, 2015.

Raynolds, M. K., Jorgenson, J. C., Jorgenson, M. T., Kanevskiy, M., Liljedahl, A. K., Nolan, M., Sturm, M. and Walker, D. A.: Landscape impacts of 3D-seismic surveys in the Arctic National Wildlife Refuge, Alaska, Ecol. Appl., 30(7), 1–20, doi:10.1002/eap.2143, 2020.

Sannel, A. B. K.: Ground temperature and snow depth variability within a subarctic peat plateau landscape, Permafr. Periglac. Process., 31(2), 255–263, doi:10.1002/ppp.2045, 2020.

Sannel, A. B. K., Hugelius, G., Jansson, P. and Kuhry, P.: Permafrost Warming in a Subarctic Peatland - Which Meteorological Controls are Most Important?, Permafr. Periglac. Process., 27(2), 177–188, doi:10.1002/ppp.1862, 2016.

Schneebeli, M. and Sokratov, S. A.: Tomography of temperature gradient metamorphism of snow and associated changes in heat conductivity, Hydrol. Process., 18(18), 3655–3665, doi:10.1002/hyp.5800, 2004.

Schneider von Deimling, T., Lee, H., Ingeman-nielsen, T., Westermann, S., Romanovsky, V., Lamoureux, S., Walker, D. A., Chadburn, S., Cai, L., Nitzbon, J., Jacobi, S. and Langer, M.: Consequences of permafrost degradation for Arctic infrastructure - bridging the model gap between regional and engineering scales, , (September), 1–31, doi:10.5194/tc-2020-192, 2020.

Seguin, M.-K. and Frydecki, J.: Semi-quantitative geophysical investigation of permafrost in northern Québec, J. Appl. Geophys., 32(1), 73–84, doi:10.1016/0926-9851(94)90010-8, 1994.

Skamarock, W. C. and Klemp, J. B.: A time-split nonhydrostatic atmospheric model for weather research and forecasting applications, J. Comput. Phys., 227(7), 3465–3485, doi:10.1016/j.jcp.2007.01.037, 2008.

Sturm, M., Holmgren, J., König, M. and Morris, K.: The thermal conductivity of seasonal snow, J. Glaciol., 43(143), 26–41, doi:10.1017/S0022143000002781, 1997.

Subedi, R., Kokelj, S. V. and Gruber, S.: Ground ice, organic carbon and soluble cations in tundra permafrost soils and sediments near a Laurentide ice divide in the Slave Geological Province, Northwest Territories, Canada, Cryosph., 14(12), 4341–4364, doi:10.5194/tc-14-4341-2020, 2020.