Interactive comment on "Thermal erosion patterns of permafrost peat plateaus in northern Norway" *by* Léo C. P. Martin et al.

Anonymous Referee #1

Received and published: 11 January 2021

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 oover 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). 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, param-

eterizations 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 (uniformly, 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.

Detailed comments

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

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.

L42 14 million(s) square kilometers

L45 of (a) few meters

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

L51 microtopography result(s)

L64 North(ern) Hemisphere

L65 plateau(s) degradation

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).

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.

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.

L109 Bleu - blue

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). 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.

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?

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)?

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

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.

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?

L144 delineate

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?

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". 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.

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?

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

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.

L173 Processes

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

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

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

L201 Plateaus

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?

L211 we focus on modelling the

L212 climate forcing of the period with field observations

L214 over a three year period

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.

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.

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?

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.

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

L236 Rephrase.

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

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? Also is the unfrozen peat thickness based on measurements?

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?

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

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?

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.

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. . .

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

L292 1.2% (of) the . . .

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

L301 mineral soil(s) at . . .

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

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.

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

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.

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

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.

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

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

L363 phases show(s)

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

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

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

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

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

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

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.

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

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?

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.

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). 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.

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.

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 [. . .]"

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.

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.

L490 consistent(ly) with

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

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

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?

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

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.

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?

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.

L519-520 (used to) -> that simulate

L533 LSMs?

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?

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.

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.