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The Cryosphere Discussions

Interactive comment on "Effects of multi-scale heterogeneity on the simulated evolution of ice-rich permafrost lowlands under a warming climate" *by* Jan Nitzbon et al.

Anonymous Referee #1

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Artic permafrost is considered one of the key tipping elements of the Earth system. However, researchers face the problem that modelling studies and observations show that the dynamics in permafrost affected regions often depend on abrupt, non-linear processes that are locally very confined, while quantifying the resulting impacts on the global climate requires using low resolution models which do not account for these small scale processes.

Here, Nitzbon et al. propose a tiling approach that allows representing surface heterogeneities – namely the polygonal structures typical for many permafrost regions and low gradient slopes with a length scale of ∼100m – in the CryoGrid model. They use the model to investigate the effects of 21st-century warming (RCP8.5) and demonstrate that their approach is capable of capturing subrgid-scale variations in the resulting degradation of permafrost. Thus, the proposed approach could potentially facilitate the understanding of high-latitude processes and improve their representation in Earth System models.

In general, the study presents highly relevant work in an important field and, overall, the manuscript is well written. Especially the introduction-, discussion, -conclusion and outlook sections help the reader place the study in the context of previous and future research on permafrost-affected regions. However, while I think that the proposed tiling approach could present an important step in improving coarse-resolution models as well as our understanding of high latitude landscapes, the authors do not demonstrate this in their work. As it stands, the manuscript only shows that the approach adds to the model's complexity, but fails to provide compelling evidence that this results in an actual improvement of the simulations. Here, the paper requires major revisions before it can be considered for publication.

General comments

1) As stated above, my main concern with the manuscript is that the authors do not compare any aspect of their simulations to observations or to simulations with any other point-scale model that has been validated in the past. Here, the authors claim that they investigate a site on Samoylov and even state that there is a large amount of observational data available for the island that can be used to validate numerical models. However, they make no use of this data making it impossible for the reader to judge whether the tiling approach leads to results that are closer to reality.

2) It may be difficult to evaluate the model's performance even with the data that is available on Samoylov. However, in this case the results need to be described in a manner that allows the reader to understand how the newly implemented processes change the model's behaviour. In this way, the reader has at least the chance to judge

whether the behaviour of the model is plausible. In the results section, the authors merely present the landscape evolution for different setups without providing any details on the underlying mechanisms or explanations as to what causes the differences in the simulations. This is not only true for the more complex cases that involve subgrid-scale heterogeneities and later exchanges between the tiles, but even for the very basic one-tile setups. For example, while reading, I was always wondering why the permafrost degradation was so much faster in the poorly-drained than in the well-drained setup? Is it because of higher heat conductivity of water? Or is it an albedo effect due to wetter soils and due to the formation of surface water bodies? Admittedly, some details are provided in the discussion section, but this is nowhere near enough to understand what the model actually does.

3) If it is partly the aim of the paper to present the approach to large-scale modellers as a way of improving their parametrizations, it requires a better verbal description of the scheme's benefits. To exaggerate a bit: One could look at figure 3 and 4 and decide that actually the simple homogenous, well-drained setup does surprisingly well when compared to the complex polygon-landscape setup. In 2100, I find subsidence of roughly 1m, an active layer depth of about 1m and largely unchanged ground below, which is very close to what I get when I aggregate the three tiles of the complex setup. Admittedly, the simple scheme misses the water bodies (especially between 2025 – 2050), but they also seem to be quite small. The same is true when I compare 5a and b as well as 5c and d. There is very little in the paper that convinces the reader that the (overall) landscape evolution can't be simulated well with a single-tile setup with an appropriate set of soil parameters. I do believe that the scheme presents an important improvement, but that point needs to be made more clearly in the manuscript.

Specific comments

P.4, L.87 ff: Study area – To my understanding the study is more a demonstration of technical possibilities of your developments rather than an investigation that relies on the specific setup of Samoylov or on data from the island. Therefore I would suggest to leave out the entire description (subsection 2.2) of the study area, because it is a bit misleading in two ways:

A) Expectations – With such a detailed description of the site, one expects to find a comparison to observations from Samoylov at a later part of the manuscript. B) Technical capabilities of the tiling approach – The tiling approach is not really capable of representing specific (complex) heterogeneous landscapes. With no real information of the actual spatial distribution of the tiles within the encompassing grid box, any tiling approach only ever represents a well mixed setting which is not really the case for the island.

I think it is sufficient to state that the initial soil conditions, forcing data and areal fractions were chosen based on Samoylov and that stratigraphy represents a generic profile based on previous studies of the island, without giving more information on the site. However, the ideal way would be to use any of the available observations from Samoylov to validate your model – then the information you provide would be very welcome.

P.6, L.117 ff: You provide the units for density and specific heat but not for heat capacity and conductivity. Other parameters are also introduced without unit later in the text. In my opinion it wouldn't be a problem to leave out the units altogether, however if you are nice enough to provide them, this should be done consistently.

P.6, L.123: What does the "effectively" refer to?

P.6, L.125 ff: How does the model deal with the surface water? Is there a (water) depth dependant runoff-formulation and does the water evaporate? Or does it simply pool until it can infiltrate?

P.6, L.127: Is the field capacity the same for the organic and mineral soil?

P.6, L.145: Why this formulation for the effective h. conductivity?

P.7, L.164: is > are

P.7, L.171: I think the information that you are not treating meso-scale lateral heat fluxes should be a bit more prominent – e.g. in the abstract you say that your model captures lateral heat fluxes at the scales not captured by ESMs which implies that also the meso-scale heat fluxes are represented.

P.8, Figure 2: A very nice figure that gives a very intuitive overview over your setups. Maybe you could separate the vertical subplots more distinctly from the connection/network diagrams to make it clearer that these are two different aspects and that the vertical setups in a and b are also applicable in subplot c and d. Also the information with respect to dx and the ice content is slightly confusing because it is only shown in subfigure a – I think it could be left out from the plot.

P.9, L.181: What happens to the vegetation layer in the case that surfaces are inundated for longer periods?

P.9, Table2: Has the column "Water" been mentioned before?

P.10 Table3: The legend states that the average of the polygonal setup is equal to the single tile setup, however this does not seem to be the case for the Reservoir elevation (poorly drained).

P.11, L.219: What is a "repeatedly appended base climatological period" ?

P.11, L.224: Why only the two extreme cases for the single-tile setup? I think it could also be helpful to see the behaviour for a medium-drainage constellation?

P.12, L.244 ff: Why is the degradation rate so much faster in the poorly than in the well-drained setting? Heatconductivity/Capacity, albedo? A description of the underlying mechanisms would be very helpful.

P12, L.250 ff: Could you explain the cause of the diverging behaviour in the tiles, it appears to be similar to the differences between the well- and poorly drained single tile setups. Also why is the outer tile behaving very differently while the inner and intermediate tiles behave similarly? The figure seems to indicate that the same dynamics could be obtained with a two-tile setup (inner tile / outer tile).

P.16, L.302 ff. | P.17, L.310 ff: It seems as if around the year 2060-2080 the rate of active-layer deepening (rate of ground subsidence) increases in all setups, which you also note on page 17 l. 312 . What is the reason for this non-linear behaviour? Is this related to the forcing? If so could you maybe provide a timeseries of the forcing – e.g. 2m temperatures?

P.16, L.303: Which sub-grid scale interactions result in the active layer being deeper in the landscape simulation – at least until the year 2090? Based on the soil properties one would expect it to be a combination of the well- and poorly drained single tile setup?

P.18, L.358 ff: Maybe these explanations – at least between lines 358 and 364 – fit better in the results section.

P.18, L.371 ff: What does "become more involved" mean in this context?

P.19, L.382 ff: Again, this information may be better suited for the result section. As an afterthought on sections 4.1 and 4.2: maybe it is possible to disentangle the process description and the interpretation to how this relates to other studies? Because the descriptive parts would fit extremely well into the results section?

P.19 L.401: Here, one could argue that your results actually show the opposite: When including both meso and micro-scale processes, the results are actually fairly close to the single tile (well drained) set-up. Thus, setting the soil parameters adequately may already be enough for projections with large-scale models.

P21, Figure 6: A very nice overview figure. It would be nice though if you could explain the abbreviations in the caption.

P. 21, .L447: from > form

P.22, L.453: I wouldn't say that this is specific to northeast Siberia but rather a simple

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Discussion paper

test case.

P.22, L.462 ff: I am not convinced that you demonstrated that in this study: There is no comparison to observations or a detailed description of the subgrid-scale processes. Thus you present a new (and I do believe more suitable) approach, but you do not show how this helps reduce uncertainties.

P22, L.472 ff: Here, I do not agree with the authors' conclusion. On one hand they merely show that their approach increases complexity but not that this complexity improves the quality of the results and provides further constraints on projections of future permafrost degradation. On the other hand they do not show that their approach is suitable for the ESMs, i.e. that the approach can be scaled to the respective resolutions. With respect to permafrost-affected regions, one important issue would be that the dependencies in the model are sufficiently linear, allowing subgrid-scale heterogeneity to be represented by one (or a few) parameter set(s) that represent an average over large areas. Personally, I do believe that the scheme presents an important improvement, but that point needs to be made much more clearly in the manuscript.

P. 22, L.477 ff: I think it would greatly increase the quality of the study if the authors could provide some validation or evaluation of the model. There is neither a detailed description of the processes that lead to the results, nor have any aspects of the simulation been compared to observations or to simulations with any other point-scale model.

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