Geoscientific Model Development Discussions

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Comment on gmd-2021-285

Anonymous Referee #2

Referee comment on "Explicitly modelling microtopography in permafrost landscapes in a land surface model (JULES vn5.4_microtopography)" by Noah D. Smith et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-285-RC2, 2022

In this study, Smith et al. introduce new implemtations into the JULES land surface model for the representation of micro-scale heterogeneities in permafrost landscapes. They put a focus on ice-wedge polygons which are common in lowland continuous permafrost and on palsas in the discontinous/sporadic permafrost zone. The authors describe in detail the novel implentations which are based on the concept of laterally copuled tiles which was previously introduced for these kind of landscapes by Aas et al. (2019) and Nitzbon et al. (2019). The new model implementations were evaluated using field observations from four sites, and parameter and process sensitivity studies were carried out to assess model uncertainties.

While the scientific concepts the study builds on are not novel, their implementation into JULES, the description of caveats in doing so, as well as the thorough analysis of model sensitivities are valuable contributions for improving the representation of permafrost in Earth system models. This justifies the publication of the article in GMD. However, the article in its present form has several (mostly minor) weaknesses which the authors should address before the article can be accepted for publication. These concern primarily the presentation of the findings and are listed below.

General comments

- The abstract of the article is quite long and contains a lot of details which are not necessary to be included here. The authors should condense the most important points and keep details for the main text. Similarly, the introduction section is rather exhaustive and should be reduced. For example, the discussion of abrupt thaw processes in [l.99ff] could partly be saved for the outlook section, and the description in [l.121ff] are not necessary in this detail in the introduction.
- I see that the authors aimed for a thorough evaluation of the tiled model configuration by comparing the results to various observational data. The study lacks, however, an

evaluation of the model‘s capability to realitically simulate the thawing of ground, i.e. how the seasonal thaw front proagates and how deep the active layers are. Due to the crucial importance of the thaw depth for a range of other processes in permafrost ecosystems, it would be desirable to also assess the model‘s capability to simulate thaw depths, and to discuss how these are affected by the tiling scheme. Observational data of active layer depths should be available at least for some of the study sites.

- I do not quite understand why the snow scheme is evaluated in terms of the „climatology“ of snow depths (Figure 7). As the observational data are available for specific years, why are these not compared directly with the respective simulations? The authors write that „the simulation has the correct depth of snow on the rim, but around 20 to 30 cm too much snow on the centre“, but this is not visible from Figure 7. Also, there seems to be an issue with the observed „snow depth“ in Samoylov during the summer months which is >0cm. Maybe the sensor measured vegetation, but the data should be corrected such that the snow depth is 0 when there is no snow.
- The authors should invest some effort in improving the quality of the figures (e.g. increase size of axes labels, consistent use of background grids, etc.). In my view, Figure 6 is particularly problematic and – in its current form – fails to provide a good overview of the results which is probably its intention. The entire figure should be thoroughly and carefully revised in order to be insightful. This concerns the placement of the panels (Why not plotting observations and modeling results for each landscape tile next to each other?), the aspect ratio (the five panels in each row have four different aspect ratios which is very irritating), and the design in general (sometimes the plot for the low tile is shifted compared to the high tile, sometimes not...). If the authors like to keep this figure, it would be consistent to provide the respective panels also for the other sites.

Specific comments

- The methods description is the longest part of the paper, which is understandable for a model description paper. However, to my opinion it contains some very JULES-specific information which are possibly not relevant to a broad readership (essentially, section 2.4). I therefore suggest to move these parts entirely to a supplement or an appendix.
- The model setup description is very comprehensive regarding the parameter variations and configurations. However, some information are missing: How ist the subsurface stratigraphy set up (ice contents, organic contents, soil texture etc.)? How is the snow represented and how were snow-specific parameters chosen?
- With few exeptions, I found the figure captions in the results section too long as they not only describe what is displayed but discuss and interpret the data. Such interpretations should be provided in the main text.
- At times, I found the language quite technical and loaded with modelling „jargon“. Examples: [l.398f] „This does however suggest that l_soil_sat_down = false is in general the more physically realistic scheme.“ [l.646ff] „In order to directly attribute the effect of different model processes on soil relative saturation, a series of runs were performed with individual processes switched off in turn with tiled no qbase as the base configuration, the ‘subtractive process switching’.“ Such sentences are very hard to understand in isolation and I suggest to revise the language to more verbal descriptions.
- [Figure 8] This figure is quite loaded and it is hard to distinguish between the individual lines. In particular, the lines for the simulations and observations for Samoylov and Iskoras are hard to distinguish due to the dark colours. In addition, I was confused about the label at the x-axis. To me, the liquid water content refers to the fraction of

liquid water in a given soil volume, which is something else than the fraction of saturation which is the fraction of the pore space filled with water/ice. This should be clarified and the authors should ensure that they are comparing the correct quantities here.

- [Figure 9] In order to save space and at the same time facilitate a better comparision amongst the three setups, I suggest to revise the way the data are plotted: Instead of showing four separte boxplots for the three setups, the same information could be shown in one (wide) boxplot where the data for the three setups are plotted directly next to each other, i.e. three boxes for runoff/on, three boxes for qbase, etc. In this way, there might be even enough space to include the results for the other sites (Figure B3) in the main text. Other than that, it seems that units provided for the y-axis (mm / m^2 / year) are wrong. I suspect this should just be (mm / year).
- [l.36] Writing „Consequently,...“ suggests that methane emissions would be determined mainly by soil temperatures, but an improved representation of saturated parts of the landscape would also affect methane emissions (e.g. from wet polygon centres).
- [l.63] A reference should be provided.
- [Figure 1] A scale for reference should be included in both pictures. If possible, hte picture for the palsas should be replaced by an aerial image in order to motivate the assumption of „repeated patterns“ (e.g., https://www.norgeskart.no/ provides high-res imagery).
- [l.111ff] Here, the works of other permafrost modelling groups, e.g. conducted witihin the scope of the NGEE Arctic project, could be worth mentioning as well.
- [l.407] I think „ice segregation“ would be the correct process as frost heave is a more general phenomenon.
- [l.415] The formula for seems to be for the polygon centre area in Figure 5 (A_2 instead of A_1. But considering the explanations in ll.421ff, the approach was to determine A_1 and A_2 independently, and to obtain Δl based on these.
- [l.416ff] It is not clear how the parameter Δx was determined. If is is calculated based on the areas and/or perimeters, a formula should be provided.
- [l.486] „Work to improve ...“ I find such statements irrelevant for the present work.
- [l.487ff] I appreciate the discussion on how to model baseflow as it addresses general issues arising when representing micro-scale processes within large-scale LSMs. However, there is no real-world motivation provided for the „twin qbase“ scenario, and I think it was not discussed explicitely in the Results section. The authors could consider discussing these scenarios in the Discussion section, especially in the context of changing hydrological connectivity, e.g. through ground subsidence.
- [l.507ff] The lateral landscape-scale drainage would most likely also be influenced by the meso-scale topography [e.g. Nitzbon et al., 2021].
- [l.591] The daily external water fluxes stated in the model of Martin et al. (2019) are only applied when the ground surface is unfrozen. Hence, the calculated annual fluxes are only „potential“ fluxes. The actual fluxes „per thawing season“ would be considerably smaller. This should be clarified as well as revised how these number compare to the annual qbase fluxes in JULES which they are compared to.
- [l.747] The authors write: „[...] explicitly modelling microtopography can increase modelled methane fluxes [...] in some cases.“ It should be noted that these cases are exactly those sites where recent permafrost degradation is strongest, i.e. regions which are transitioning from permafrost- to non-permafrost conditions.
- [Figure A1] To my opinion, these plots are not necessary, as long as the depths and times for which model and observations are compared are stated clearly in the main text or the main figures.
- [Figure B1/B2] I found it confusing that for the soil moisture splitting, the subtractive/additive process switching results are shown in the main text/appendix (Figure 10/B1) while it is the other way around for the soil temperatures (Figure 12/B2). Even if the results shown in B1/B2 are not discussed in detail, it would be more instructive and allow for better comparision to provide these as part of Figures 10/12 in the main text, e.g. as an additional row for each site. Instead, the tables with the

specific numbers could be moved to the appendix as the most important ones are mentioned in the text.

- [Figure B3 and B4] As mentioned above, the figures could be condesed to one panel/plot per site if the respective fluxes for the five setups are plotted next to each other. Otherwise, the axes labels and captions are much too small to be readable without zooming in. Please revise and increase the label sizes.

Technial corrections

- [l.22] should be „... the model‘s sensitivity to its parameters."
- [l.136] „the gridbox average values": Please specify which average is meant here.
- [l.386] delete „the"
- [l.489f] „we also qbase set to zero for ..." Please correct.
- [l.510] correct typesetting for fd1 and fd2 to be consistent with other occurences.
- [l.663] Please write „standard" instead of „std".
- [l.813] „choose" instead of „chose"
- [l.865] I think this should be „through" instead of „though".