

We sincerely thank the reviewer for the thoughtful and constructive comments, which will help make the manuscript more valuable. We have addressed all the comments below and will improve the manuscript as suggested. The reviewer comments below are numbered with “RC#”: and shown in bold letters, while authors’ comments start with “AC#”.

RC1: line 46 You generalize that all current ESMs omit thermokarst, but coauthors have published papers using CESM/CTSM to do so. Reword to be more precise.

AC1: Thank you for pointing that out. We will rephrase the sentence to be more precise.

“..., yet most ESMs still lack an explicit representation of permafrost microtopography and the thermokarst processes associated with it. Earlier versions of the CLM5, include several explicit representations of permafrost processes, including excess ground ice and the resulting surface subsidence (Lee et al., 2014), as well as sub-grid representations of excess ice (Cai et al., 2020) and laterally coupled tiles (Aas et al., 2019). However, not all of these features are part of the standard model configuration used in coupled climate simulations, and no current ESM dynamically represents how subsidence alters the lateral exchange of heat, water, and snow between landscape elements.”

RC2: line 70 Cite CLM5, CESM, and NorESM

AC1: This is a good point. We will include the following citations for these models.

- CLM: «Lawrence, D. M., Fisher, R. A., Koven, C. D., Oleson, K. W., Swenson, S. C., Bonan, G., Collier, N., Ghimire, B., van Kampenhout, L., Kennedy, D., Kluzek, E., Lawrence, P. J., Li, F., Li, H., Lombardozzi, D., Riley, W. J., Sacks, W. J., Shi, M., Vertenstein, M., Wieder, W. R., Xu, C., Ali, A. A., Badger, A. M., Bisht, G., van den Broeke, M., Brunke, M. A., Burns, S. P., Buzan, J., Clark, M., Craig, A., Dahlin, K., Drewniak, B., Fisher, J. B., Flanner, M., Fox, A. M., Gentine, P., Hoffman, F., Keppel-Aleks, G., Knox, R., Kumar, S., Lenaerts, J., Leung, L. R., Lipscomb, W. H., Lu, Y., Pandey, A., Pelletier, J. D., Perket, J., Randerson, J. T., Ricciuto, D. M., Sanderson, B. M., Slater, A., Subin, Z. M., Tang, J., Thomas, R. Q., Val Martin, M., and Zeng, X.: The Community Land Model Version 5: Description of New Features, Benchmarking, and Impact of Forcing Uncertainty, *Journal of Advances in Modeling Earth Systems*, 11, 4245–4287, <https://doi.org/10.1029/2018MS001583>, 2019.
- «Lawrence, D. M., et al. (2018). Technical Description of version 5.0 of the Community Land Model (CLM). NCAR Technical Note NCAR/TN-543+STR, National Center for Atmospheric Research. «
- CESM: «Danabasoglu, G., J.-F. Lamarque, J. Bachmeister, et al. (2020). The Community Earth System Model version 2 (CESM2). *Journal of Advances in Modeling Earth Systems*, 12, e2019MS001916.doi.org»
- NorESM: «Bentsen, M., et al. (2013). "The Norwegian Earth System Model, NorESM1-M – Part 1: Description and basic evaluation of the physical climate."
- «Seland, Ø., et al. (2020). "Overview of the Norwegian Earth System Model (NorESM2) and key climate and carbon-cycle response."

RC3: line 71 CLM already has microtopography and subsidence due to excess ice melt; be more specific of how this adds to the existing scheme

AC3: Thanks for pointing that out. CLM has a *microtopography concept*, which is different from how we use the term. We therefore changed the description to be more specific:

“Here, we present a new implementation of dynamically coupled tiles in the Community Land Model (CLM5). The approach builds on the existing excess ice implementation (Lee et al., 2014) and enables the explicit representation of microtopographic permafrost landscapes and associated thermokarst processes, by coupling duplicated landscape columns.”

RC4: line 77 It is not clear that the sentence beginning "The approach..." is talking about the modified CLM, not the standard CLM that you were discussing in the previous sentences. Perhaps have a different paragraph for the two

AC4: We will start a new paragraph here and rephrase the sentence as follows:

“The new approach, which we present in this paper, captures both gradual permafrost thaw and rapid thaw associated with ground-ice melt and surface subsidence by implementing lateral exchange of heat, water, and snow between landscape elements.”

RC5: Figure 1 is confusing. For a)-d) it is not clear what the correspondence between the reality and conceptual parts is or what the colors represent. In f) I see A1 and A2, but they are not described. Perhaps these should be two figures (a-d) and (e-f) so that you have space to more fully describe them. Similarly, the figure caption should be more descriptive.

AC5: In the revision, we will implement this comment by revising the figure. We will improve the figure and the description by making a separate figure for the geometrical parameters. Additionally, we will point out that a) and c) are conceptual representations of reality, which is presented in panel (b) and d)). The parametrized reality will get an extra letter and will be described in the caption. The colors will be labeled. We will reduce the number of colors label the rim and center in a) and mire and palsa in c). A description of A1 and A2 will be added.

RC6: line 90 what does 'version 2.0' refer to?

AC6: It refers to an unpublished, more advanced version, i.e., the most recent development version at that time. We will just refer to CLM5.0 for clarity.

RC7: line 100 There is no maximum number of soil layers, and 54 soil layers is not the default; perhaps list the default.

AC7: This is correct and will therefore be changed to: *“In the default configuration, CLM5 uses 25 soil layers, of which the upper 20 are also hydrologically active.”*

RC8: line 104 The description of the CLM spatial hierarchy could be improved. Define landunits, columns, and patches. 'Landscape types' are not columns, they are landunits. CLM has a concept of microtopography; explain what it is and how you are using the term here differently.

AC8: We will include a brief description of the spatial hierarchy and provide a reference that explains it in more detail. Additionally, we will explain the microtopography concept of CLM, how we use it, and why “our” microtopography is different.

“CLM5 has a microtopography concept, which refers to grid-cell topographic slope and microtopographic distribution and determines the fractional inundated area. It is therefore a tool to determine surface water and inundated areas. This differs from how we use the term microtopography. We refer to two interacting columns that represent two different parts of a dynamic microtopographic system (in this case, thermokarst).”

RC9: line 110 is there a new spatial structure (tile), or just two columns? Explain your model structure in terms of figure 1 in more detail.

AC9: Thank you for pointing out that we should improve the description of the model structure. We will extend that paragraph and explain the model structures in detail, also referring to Figure1. In short, we do use two columns, but the natural vegetated landscape column, which in the standard version contains only one column, is split into two columns.

“, the natural vegetated soil column is split into two columns, which is not possible in the standard version of CLM. These columns are called tiles further on. These tiles represent a new sub grid hierarchy that allows two distinct natural vegetated soil columns with different soil stratigraphy and state variables to coexist within a single grid cell. Figure 1 e illustrates the tiling concept and the implemented lateral fluxes. As shown, two different soil columns, with natural vegetation exist within one grid cell. One contains the majority of the excess ice (left column in Figure 1e), and the other contains the minority (right column). Due to the greater amount of ground ice, a height difference between the two columns exist. Figure 1e) shows the implemented fluxes: lateral heat exchange between connected soil layers, lateral water flux due to differences in the perched water table, and wind associated snow drift.”

RC10: line 156 why is thermal conductivity called 'ksat'?

AC10: There is no specific reason, we will change it to *kcon* to make it consistent with the model.

RC11:line 160 are there any soil layers that are not thermally active?

AC11: No, we will change that accordingly: “..., over all soil layers”

RC12: line 163 I assume the term in parentheses is a transmissivity? If so, describe that in the text and/or add an equation $T=...$ Why is an absolute value needed (if that is what the vertical lines indicate)

AC12: Yes, the term is the transmissivity; we will add an equation for that. The absolute value is needed because the head gradient can be negative, depending on which tile is the receiving tile.

RC13: line 167 the heat gradient appears to just be a height difference?

AC13: As described in the text, it's the actual difference between the depth of the perched water table between tile 1 and 2. The height difference must be added to get the effective difference in perched water depths.

RC14: line 174 what is 'excess ice standing surface water', and why do you need to prevent it?

AC14: This is a mistake, and is meant to be surface water accumulation. We will change that to: *«To prevent surface water accumulation on the lower tile, an additional land overflow mechanism is implemented.»*

This mechanism is necessary because otherwise surface water, e.g. after snowmelt, can accumulate to 1.00 meter or more, which is not realistic. Therefore, a maximum surface-water height is implemented.

RC15: line 175 conceptually, this pair of tiles is embedded in a landscape of similar tiles, so where would this overflow go?

AC15: In the model, this overflow is added to the surface overflow, which is drained out of the grid cell.

Conceptually, this overflow would be added to larger external water reservoirs such as rivers and lakes, or into drainage flow (polygonal setup).

RC16: line 206 this is not the correct reference for the meteorological data. What is the spatial resolution, and what fields are used to force the model?

AC16: We will change the reference to:

- Dirmeyer, P. A., A. J. Dolman, and N. Sato, 1999: The Global Soil Wetness Project: A pilot project for global land surface modeling and validation. *Bull. Amer. Meteor. Soc.*, **80**, 851-878. and Hyungjun Kim. (2017). *Global Soil Wetness Project Phase 3 Atmospheric Boundary Conditions (Experiment 1)* [Data set]. Data Integration and Analysis System (DIAS). <https://doi.org/10.20783/DIAS.501>

The spatial resolution is 1.9° latitude by 2.5° longitude. We will include that in the manuscript: “*at a temporal resolution of 3 hours and a spatial resolution of 1.9° latitude by 2.5° longitude. Site specific data are downscaled from the nearest grid cell.*”

RC17: line 234 what are the functions of the 'maximum saturated area fraction' and the mean topographic slope in the model?

AC17: They are fixed parameters specified in the surface file rather than functions.

We will add a short description in the manuscript about how those parameters are used.

“Maximum saturated area fraction and mean topographic slope are model parameters that are used to calculate standing surface water and inundated areas.”

RC18: line 242 what is the 'snow accumulation factor'?

AC18: This is a factor that determines how quickly snow accumulates in a grid cell. We will clarify this in the sentence:

“The snow accumulation factor, which determines how quickly snow accumulates in a grid cell, is set to the maximum value of 0.4.”

RC19: line 248 why was the period 1930-1950 chosen to spin up the model, given that earlier forcing data is available?

AC19: The earliest possible period would have been 1901-1910. As this period was colder than the other periods before and after, 1930 - 1950 was chosen to have a slightly warmer period for the spin up. A 20-year period was chosen to include greater variability.

RC20: line 258 I did not understand the reason why one simulation started with initial subsidence, can you explain more clearly? Similarly, why would the other simulation start with an elevation of 0.38?

AC20: The value of 0.38 represents the initial height difference between the tiles at Samoylov. As the simulations for the sensitivity study start from the spin up of the REF simulation, which does not show any subsidence, all simulations of the elevated tile start with the initial height difference of 0.38.

At Iskoras, simulations for the sensitivity study start from the spin up of the tiling case. While spin up, the palsa and the mire both lose excess ice and therefore show subsidence. As simulations for the sensitivity study start with this case, they show an initial subsidence.

We will clarify that in the manuscript.

RC21: line 274 why does the tile with the deeper snow depth melt-out earlier?

AC21: It doesn't. The snow melts out 10 days earlier at the palsa. We will reformulate the manuscript to make it clearer.

“Snow ablation, defined as the first day in spring when snow depth is zero, occurs between late April and late May. Snow ablation at the palsa is about 10 days earlier than at the mire, because of a thinner snow cover.”

RC22: line 277 does the phrase ' snow remains deeper at the Center than at the Rim' refer to observations or the simulation?

AC22: Mainly the simulations, but it is in agreement with observations from aerial images from the area. We will reformulate and add a reference.

“At Samoylov, simulated snow cover remains deeper at the center than at the rim, while REF falls between both regimes. This is in agreement with aerial images.”

RC23: line 280 is there a reference for this: 'the absence of wind-driven snow removal, an important process in the Lena River delta.'? Why would one disable wind compaction?

AC23: Boike et. al., 2013 and 2016 show, that wind driven snow removal is an important factor for snow depth in Samoylov, while we do model the redistribution from rims to center, snow redistribution on larger scale is not model.

Wind-driven compaction was removed to improve the representation of snow density and, consequently, snow insulation.

RC24: line 293 why do you discuss Figure 3 before Figure 2? Similarly, you discuss figures from the appendix, then return to discussing figure 2 (line 316). I recommend trying to discuss each figure in turn fully before switching to another figure.

AC24: Thank you for that comment; it is true that the order should be improved. The idea was to first discuss everything for one location, then for the other, instead of constantly jumping between locations. But similar plots for both locations are combined in one figure to improve readability and facilitate comparison between locations.

We would like to keep it that way, discussing the locations separately, but also improve the order. Therefore, we will move the discussion from the figure in the appendix to the end of the section and discuss the figures in the following order: figure 2 for Iskoras, figure 3 for Iskoras, figure 2 for Samoylov, and figure 3 for Samoylov, appendix.

RC25: line 301 you note that the temperature differences in the model are due to differences in snow depth, but the observed palsa snow depth is similar or even smaller than the model. Why do the observations not show the same temperature difference as the model?

AC25: Differences in the observations are also due to differences in snow depth, we assume that the strong difference in the model is due to overly cold winter temperatures in the forcing data and to an overestimation of the heat transfer in the dry palsa.

RC26: Figure 2a snow exists through summer at Samoylov? And snow height decreases from October to January?

AC26: No, but snowfall is possible during summer at Samoylov. For one year, snow height does not increase from October to January. However, snow measurements are highly variable at Samoylov, resulting in the unusual snow pattern when averaged. We will improve the description and include a reference to Boike, et al, which shows the annual snow pattern.

RC27: Figure 2b why is there a large temperature difference for the model between rim and center in the autumn (starting around October), but it is gone by April?

AC27: In the model, dry soils, shallow snow cover, and low temperatures lead to a sudden drop in soil temperature at the rim. Temperature decrease in the center is slower, but continuous, leading to smaller differences in later winter. Also, the temperature at the Rim rises earlier, due to less snow cover and drier soils, which leads to a similar soil temperature in winter. We will include the above clarification.

RC28: Figure 2d obs show no difference in ALT, but model shows center being colder. Does this give you information about the site soil properties, i.e. perhaps the model soil properties are more different than observed?

AC28: Soil properties could be one reason for this difference, but we think it is more strongly related to soil moisture and temperature. In the model, the rim does not retain much water and therefore dries out substantially, resulting in a higher seasonal temperature amplitude. The water retention curves and thermal conductivities could be improved in future model development, although these properties are also closely related to the underlying soil properties.

RC29: line 333 you say REF lacks ice melting, why is that? It also has excess ice (?).

AC29: Yes, but it does not have lateral fluxes, so REF only shows ice melting if ALT deepens, which is not the case for the REF simulations, whereas dynamic tiles can experience ice melting in only one tile. We will reformulate to:

“The uniform warming throughout the soil column at REF reflects the lack of lateral heat exchange and ice melting, as no excess-ice melting occurs in the REF simulation, hence warming only occurs due to an increase in air temperatures from the top.”

RC30: line 341 I did not understand i) and ii). For i) you are not comparing maximum values, you are comparing to the model at the same time. For ii), I only see a rim observation, not an ice-wedge observation.

RC30: We are comparing the maximum values of ALT in the model with the measurements, but measurements are only taken in early September, while in the model ALT continues to deepen during September. Therefore, the maximum September ALT is deeper than the measurements taken at the beginning of September show. Only Rim observations are shown, but are compared to a mixture of rim and ice-wedge conditions. We will clarify that:

“(i) ALT measurements are mainly taken in early September, before maximum depth is reached; while simulations show the mean for the whole of September (ii) observations distinguish between rim and ice-wedge (not shown), whereas the model represents a combined rim/ice-wedge tile, which is compared to the rim observations;”

RC31: line 362 you say the Rim becomes wetter but the water table is deeper. Usually a deeper water table indicates drier conditions; can you clarify?

AC31: The deepening of the perched water table is mostly due to a slight deepening of ALT caused by warmer soils. However, slightly more water is available due to subsidence. We will clarify this in the manuscript.

RC32: line 376 you say 'REF fails to capture the observed soil moisture regime', but how could it capture both? It is only one column. At best it might capture one or the other, but saying it failed to capture two moisture states at the same time is not really correct.

AC32: That is correct, we will rephrase it to:

“, while REF only captures a mixture of palsa and mire conditions, which is not comparable with reality.”

RC33: line 380 snow melt appears to occur in may and June. Why does surface water peak in July or august? I would think evaporation would reduce surface water after snow melt is over. Is the increase due to summer precipitation?

AC33: This is because the soil can still be frozen after snowmelt; therefore, the penetration of water into the soil is limited. Surface water also becomes shallower as ALT deepens and the perched water table lowers. Evapotranspiration is lower than summer precipitation.

RC34:Figure 5 the model Center shows surface water above a dry layer near the surface. How is that possible? Wouldn't the surface water infiltrate into the soil? REF shows similar behavior.

AC34: This occurs for the following reasons. The water holding capacity, set by the soil structure, is highest at the surface and lower further down, so wetter layers (shown in mm/mm, not as a percentage of saturation) can exist over drier layers. And the water does not cover the whole “grid cell” but just a certain percentage of it, as specified in the text, which means there can be drier parts of the grid cell and wetter ones. Additionally, infiltration can be slower than surface water accumulation.

RC35: line 407 Can you further explain why there is greater energy input during snow melt for the Mire?

AC35: The ground heat flux includes heat flux into the soil and snow, including energy associated with snowmelt. As there is more snow at the mire and REF than at the palsa, energy input during snowmelt is greater and is the main reason for the greater ground heat flux at the mire. We will include this in the manuscript as follows:

“Ground heat flux is largest at the mire, partly due to greater energy input during snowmelt because of the thicker snow cover.”

RC36: line 417 is this statement also true for REF (not just the tiled simulations)?

AC36: Thank you for pointing that out. There seems to be an error in the manuscript, as the energy division belongs to Iskoras. We will therefore move the sentence up to line 408 and include a new one for Samoylov. We will reformulate to:

Line 408: “In the tiled simulations, the summer energy budget is dominated by LH, accounting for 50 % of the total energy (340 W m²), followed by SH (approximately 30 %) and ground heat flux (20 %). REF is also dominated by LH, accounting for two-thirds of the energy input.

New line 417: “In the tiled simulations, the summer energy budget is dominated by ground heat flux, accounting for 40 % of the total energy (174 W m²), followed by LH (approximately 33 %) and SH (26 %). In REF ground heat flux and LH each account for 40%, while SH is of minor importance.

RC37: line 424 where does surface runoff go? In the model, runoff can simply be removed from the system, but is surface runoff from the sites actually observed?

AC37: In the model, runoff is removed from the site.

At Samoylov, runoff is observed through drainage channels, and at Iskoras there is also runoff from the mire which flows further down the mire and eventually into a small creek.

RC38: line 428 in satellite phenology mode (no active biogeochemistry), plants of the same type should have identical leaf area index. If vegetation ET is different, it may be due to differences in snow cover; please confirm.

AC38: This is true. Leaf area index is identical, but ET is still different, because of differences in snow cover, column size, and standing water. We will change the manuscript accordingly:

“While standard variables like leaf area index are identical in satellite phenology mode, differences in vegetation and evapotranspiration between tiles can occur because of differences in snow cover.

RC39: line 435 is there outflow from the Center in reality? If not, can the model be parameterized to reproduce such behavior?

AC39: At Samoylov, there is outflow from the center. It either tops over the rim (e.g. after snowmelt if there's lots of snow, but generally it does not), or it flows subsurface through the rim to a trough system between the rims, from where it flows out to the next creek as well. In reality the situation is a lot more complex, but we model a simplified representation of the outflow.

RC40: line 496 perhaps this is a fine point, but is the soil column split, or are you simply adding a second soil column? The latter implies no change to the model spatial hierarchy, but the former seems to add a new layer ('tile'). I would recommend using the model terminology ('column') rather than 'tile' to be clear if the number of columns is simply increased.

(Similarly for line 510.) If a new spatial unit is added to the model, explain how the tile fits within the previous structure (perhaps in section 2.1.1), and why adding a tile was done rather than adding another column.

AC40: It is a mixture; we are adding a second column, specifically a second natural vegetated soil column, which in the standard CLM version is not possible. These two columns do interact with each other. We will change it to column instead of tile and improve the description. We will improve the description of the tiling approach, as discussed in Comment 9, which should clarify how we use tiles in the manuscript. We will put an in-paper reference here and also use column instead of tile for clarification.

“To represent permafrost microtopography, the natural vegetated soil column is split into two interacting columns (called tiles, to distinguish them from standard CLM columns, see Section 2.1) that simulate distinct components of patterned permafrost landscapes, such as the palsa and the mire of a peat plateau.”

RC41: Perhaps the authors could add some discussion of how this site-level study relates to global ESM simulations. Can this implementation be used more broadly, or is it primarily useful for site-level research? For example, how could the snow redistribution scheme be used in a larger domain?

AC41: The idea is to use this approach for global simulations. The approach is useful for patterned landscapes, where similar microscale structures cover large areas, as is typical for ice-wedge polygons and palsas. For these landscapes, the implemented lateral fluxes and snow-redistribution schemes can be upscaled to larger areas by simply adding multiple structures like this within a grid cell.

By identifying areas with patterned landscapes, the approach could be upscaled to regional or global applications. One limitation is the geometrical parameters, which can currently only be measured at the site level and would therefore have to be estimated for global applications.

We will extend the discussion and include a section on upscaling and GHG calculations.