Response to reviewer comment 3:

Special comments

p.3 l.1: Cound you explain the effect of the modified parameters (e.g. microtpography distribution and surface inundated fraction) on the entire model? Those descriotions would be helpful to understand the proposed parameterization is crucial to assess the biogeochemical feedbacks.

- We thank the reviewer for this suggestion and the following text will be added to the revised manuscript:

"Surface water is defined by spatial scale elevation variations that is the microtopography. The microtopography is normally distributed around the grid cell mean elevation. The fractional area of the grid cell that is inundated (fh2osfc) can be calculated with the standard deviation of this microtopographic distribution. The surface inundated fraction, in turn, affects the soil heat/water/carbon fluxes with the atmosphere."

p.3 l.24-33: The delineation of the actual relathinship between ground subsidence and microtopography is necessary to understand the relevance of modeling instead of a required parameterization by governing equations in CLM.

- We are not sure if the reviewer is requesting us to show the relationship between ground subsidence and microtopography in reality, which is hard to assess due to the lack of observational data. As a result, we used existing parameterization in the CLM surface hydrology based on TOP model. We acknowledge that this is only the first step in this kind of parameterization and hope that our study can bring attention to observational community for such observational data.

p.3 l.35: Related to the previous comment, if you could calculate more realistic value of microsigma with finer-resolution topographic data and subsidence information, does it improve the model applicability? It would be helpful if you explain the limitation of "modeling (conceptulization)" and "parameterization" respectively.

- The parameterization in models such as CLM should focus more on functionality and that this is a very conceptual step in the parameterization. Next step will be subgrid-scale representation of this process but this is not within the scope of our study. We refer to Aas et al. (2019) for the subgrid scale process representation in the revised manuscript.

p.10 fig.6: As the authors pointed out, it is difficult to directly compare inundated area between GIEMS dataset and simulated results due to the gap of definitions of water surface. However, I think some other variables relating water budget (e.g. river discharge) are modified by the proposed parameterization and can be compared with observation data. I apologize if I misunderstand the numerical implementation in CLM.

- We thank the reviewer for the question. It is correct that other water budget variables are affected from our parameterization, however for river discharge, the direct effects from the surface subsidence are minor compared to permafrost thaw related spring river discharge increases, hence not useful to validate the new model.

References:

Aas, Kjetil S., et al. "Thaw processes in ice-rich permafrost landscapes represented with laterally coupled tiles in a land surface model." *The Cryosphere* 13.2 (2019): 591-609.