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Interactive comment on "Ground subsidence effects on simulating dynamic high latitude surface inundation under permafrost thaw using CLM5" *by* Altug Ekici et al.

Anonymous Referee #2

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Review:Ground subsidence effects on simulating dynamic high latitude surface inundation under permafrost thaw using CLM5

1 General comments

The fate of carbon stored in soils in the high northern latitudes is determined by landscape features whose horizontal extent is often below the kilometre-scale. Thus, finding an adequate representation of (near-) surface subgrid-scale processes in permafrost affected regions is an issue that is of great importance for modellers working at the coarser scales. Here, Ekici et al. propose a simple parametrization that they use to link the Comunity Land Model's subsidence submodule to the calculation of the inundated fraction by changing the microtopography wherever subsidence occurs.

In the study, the authors show how the inundated fraction reacts to extreme changes in the microtopography, i.e. by halving and doubling the paramter used to describe the microtopography distribution. Furthermore, for selected grid-boxes they demonstrate how the simulated subsidence affects the microtography and the inundated fraction and they show how the surface water fraction in the high northern latitudes differs in simulations with and without their new scheme.

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To the best of my knowledge, there is no process-based model representing the lake/wetland dynamics resulting from the melting of ground ice that is suitable for the use in large scale models. Therefore, the proposed parametrizations could very well help to capture the resulting effects in scenario simulations using ESMs. However, even though the article is generally well structured and well written, there are a some key issues that should be addressed prior to publication.

One of my main concerns with the parametrization pertains to the use of the accumulated subsidence for estimating the changes in microtopography. I see this as problematic as subsidence in the model can only increase over time (as the Lee scheme does not account for the formation of soil ice) and the authors introduce a fixed threshold above which further subsidence increases the microtopographic parameter rather than decreasing it. Hence, for the scheme to produce meaningful changes in inundated fraction, it does not only need to be initialized with the correct microtopography and soil ice content but also with reliable information of how much subsidence has happened in the past in any given grid box; in other words one would have to know, how close the subsidence is to passing the threshold when it will lead to an increase in sigma; and I am not aware of any dataset that could provide this information. In their study the authors avoid this initialization problem by starting the simulation with zero-subsidence and then having a 100-year spin-up period. But in this case, the results will be highly dependant on the selection of the spin up period, e.g. if the spin up of the model would have been done for 1000 instead of 100 years the results could look very different as in many grid-boxes the subsidence may have already passed the 0.5m-threshold meaning that the inundated fraction would actually decrease during the simulation.

Additionally, even though the authors make it clear that this is merely a first step, I am not fully convinced by the arguments that are being made in favour of the chosen parametrisations/assumptions. On pages 5 (l. 31) - 6 (l. 2) the authors claim that the simulated changes in inundated fraction stay within the range that results from halving or doubling the reference value of sigma; but I fail to see how that validates the coupling assumption? It merely shows that the parametrisation has a certain sensitivity, but how sensitive should it actually be? Also, while I can see a certain spatial correlation between the simulated subsidence and the changes in microtopography, i.e. Fig 3 and Fig 2a, I have a very hard time seeing any meaningful correlation between the changes in microtopgraphy (Fig. 2a) and changes in inundated fraction (Fig. 2b). But most importantly, I am not convinced by the comparison to the GIEMS dataset (page 9, l.6 - page 10, l.6). In Figure 6. there is almost no difference in the inundated fractions simulated with the two model versions. And if there was any difference I do not understand how that could demonstrate that it is beneficial to use the new scheme. The control simulation uses the present day sigma and should therefore also result in the best simulated present day inundated fractions. If the simulations with the new scheme give inundated fractions that are closer to the observations (which is not visible in the plots) it merely means that the function CLM uses to compute the inundated fraction could be improved, but not that the reference microtopography is wrong. So at best this comparison shows that the new scheme doesn't change the microtopography so much that it substantially affects the simulated present day inundated fraction. As the scheme is used to capture the dynamics related to subsidence, it would be key to show a comparison with observed trends/changes in the inundated fraction, in order to demonstrate that the scheme performs well.

Consequently, until the authors demonstrate the scheme's ability to improve the models surface water dynamics and provide a strategy for the initialization and spinup of the model, I can not agree with the their conclusion that "the parametrization is implemented successfully and can be used for further climate scenarios".

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2 Specific comments

- p.2, l.24-l.27: As the subsidence simulated by the scheme is a key input to your model it would be very helpful if you could provide some more details on the scheme by Lee et al..
- P.3, l.32: Why preliminary?
- P.3, l.35: Here, it would be very helpful if you could clarify whether s is indeed the accumulated subsidence since the beginning of the simulation.
- P.4, l.12ff: Is there a specific reason why you do the spinup using the forcing from 1901-1930 while you start your simulation in the year 1860? Wouldn't it make more sense to use the climate forcing from the beginning?.
- P.4, l.18ff: Could you also indicate how the microtopography was initialized in the Exice experiments. I just assumed you use the same index that is used for the control simulation (Fig. S1).
- Fig. 1: I find it quite difficult to judge the differences in fh2osfc between the simulations. Maybe you could show the differences between sigma-0.5 and sigma-2 as a sub-figure? Or maybe you could also provide a graph with sigma and d on the x and y axes and fh2osfc as a colour to give a more systematic overview?
- P.6, l.1f: I fail to see how this supports your coupling assumption. It merely says something about the sensitivity of your parametrization. Without knowing which sensitivity should be expected it is very hard to use this in support for the assumption.
- Fig. 2: I was quite surprised to see so little spatial correlation between the change in microtopography and the change in inundated fraction (could you maybe calculate a correlation coefficient). While sigma is almost exclusively lower in Exise,

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there is actually quite a number places where the inundated fraction is also smaller. Additionally, most of the areas in which you find the strongest changes in microtopography show now substantial increase in the inundated fraction. Thus I would not say that the patterns are similar. Here I think more information, especially on the changes in the surface water level, is required for the reader to better understand the plots.

- P.6, l.8-l.13f: I find this formulation problematic. The connection between melting ground ice and surface hydrology is not suggested by the correlations between Figs2 and 3, but because the connections where directly implemented with Lee et al.'s and your scheme. But, while I do see a correlation between Figs 2a and 3, I do not see the same patterns in Fig 2b.

- P.7, l.7ff: If you initialize your simulation with the present day sigma and the present day ice content, and then run it for 240 years (spinup + 1860 - 2000) during which time the ice content can only decrease, wouldn't you necessary end up with a worse microtopography for present day?. I presume that the initialisation/ spinup procedure was carried out because there is no data to consistently initialize the model either at 1860 or at present day? But what would be the strategy to initialize/ spin up the model for future simulations?

- Fig. 4 and Fig 5.: Why is the difference in fh2osfc so variable even if there are no pronounced changes in sigma and the two experiments use the same forcing?.

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