Response to reviewer comment 2:

General Comments

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.

- We acknowledge the reviewer's concern. We agree that this is one of the largest sources of uncertainty in our work. As the reviewer pointed out, our parameterization depends much on the initialization of excess ice and there is currently no global scale dataset to parameterize and evaluate the model. One feasible proxy for evaluating the surface inundation is to use the terrestrial CO_2 and CH_4 fluxes once we use our parameterization coupled to the CLM biogeochemistry module. This is the aim for the next step in our work and we hope that our work can motivate the observation community to collect such dataset.

- The spin up procedure was sufficiently long enough to bring the physical state into equilibrium, since we did not use the biogeochemistry, we did not need a longer spin up period than 100 years. Also the excess ice melt comes to an equilibrium with the spin up climate state so a longer spin up would not change the initial excess ice melt conditions.

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?

- We have chosen to double and halve the reference microsigma value in the sensitivity analysis to show the upper and lower boundary of the sensitivity in fh2osfc with changing microsigma. The behavior of fh2osfc in these sensitivity simulations support that the dynamic parameterization in this study does not lead to unrealistic fh2osfc values in the simulations under present day climate. This test is merely to constrain any extreme sensitivity cases that might have originated from our conceptual scheme. Finding the best sensitivity of surface inundation to soil subsidence is beyond the scope of this study and currently very challenging to estimate with global observational datasets.

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).

- The subsidence directly dictates the microsigma changes in the code, therefore, it is more straightforward to diagnose the relation between subsidence and microsigma than subsidence and fh2osfc. We agree with the reviewer that it is difficult to tease out direct relationship between microsigma changes and surface inundation. This is due to the fact that surface inundation is not only affected by the subsidence but also by combination of factors such as precipitation, air temperature, and soil moisture. Hence, the fh2osfc changes in Fig2b is difficult to interpret only from the changes in microtopography under excess ice melting. Yet, we would like to draw the reviewer's attention to the extreme subsidence areas (red points in Fig2a) and the corresponding changes (even though very small) in the surface inundation map (small blue areas inFig 2b), which suggests that our parameterization is creating surface inundation at the areas where it should. Figures 4 and 5 are added for similar reasons to compare the changes in fh2osfc in global and point scale dynamics. The future simulations under climate warming will show pronounced subsidence (Lee et al., 2014) and the consequent effects on surface inundation will be more visible.

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.

- We thank the reviewer for opening this point to discussion. Fig. 6 indeed does not show a large difference between the Control and Exice simulations. However, as we pointed out in the discussion, we intended to show that our new parameterization does not create unrealistic values compared to the Control simulation and this work is merely to increase our confidence to use the new dynamic parameterization for future climate change scenarios, where the differences due to major subsidence will be more pronounced. So, we do not claim the current CLM microsigma parameter is faulty, our new parameterization introduces a temporal variability to the microsigma parameter and it shouldn't diverge too much with the present day conditions. Hence, the similarity between Control and Exice simulations in Fig 6 supports our aim. On the other hand, we use the GIEMS dataset to additionally show that the regions where extensive high surface inundation occurs in observational dataset and to confirm that the model results correspond well with the observations in the spatial patterns of surface inundation. Since the GIEMS dataset was not a very long time series, we couldn't use this dataset for direct comparison over time. However, Fig 5 demonstrates model's behaviour in time for different climatic conditions and the deviations from the control run are quite distinguishable.

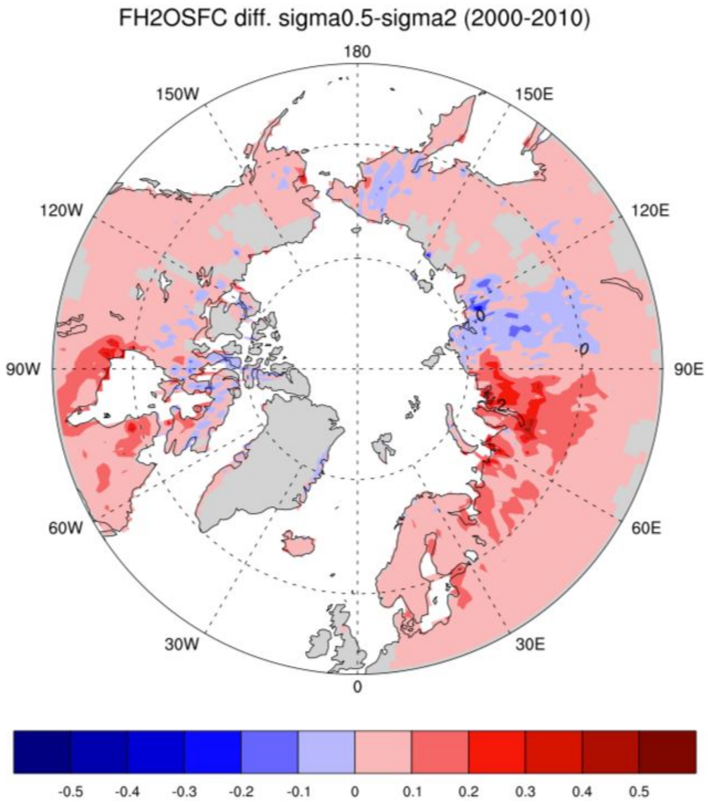
Consequently, until the authors demonstrate the scheme's ability to improve the models surface water dynamics and provide a strategy for the initialization and spinup of the model, I can not agree with the their conclusion that "the parametrization is implemented successfully and can be used for further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

- We believe we have answered some of the reviewer's concerns and we are not sure if the reviewer has some other suggestions at this point. We want to clarify that one of the points of this manuscript is to show a new parameterization that works globally for a land surface scheme. We suggest to revise our conclusion points to tone down the implications of this study that it is the first step in this kind of parameterization. But more importantly, this study really brings out the importance of observational data and we encourage observations to take this into account.

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..
- we are adding some details of Lee et al. scheme in the methods section in the revised manuscript.
- P.3, l.32: Why preliminary?
- wrong choice of word, changed 'preliminary' to 'conceptual'
- 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.
- yes we added clarification in the text
- 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?.
- it was just a standard procedure for CLM to use the 1901-1930 block for the spinup and we wanted to stay consistent.
- 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).

- yes it was using the same reference microsigma. This information is now added in the text
- 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?
- we are adding the difference map sigma-0.5 - sigma-2 in the supplements



- 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.
- discussed this above in the main points
- 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, 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.
- this point is also discussed above in the main points
- 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.

- this point is also discussed above in the main points
- 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?
- yes it is true that the microtopography is expected to be different in accordance to the subsidence levels occurred during the spin up and transient simulation, but the idea here is to constrain the dynamic parameterization and to avoid any major extreme sensitivity from the conceptual method. since there is no way to properly initialize the soil subsidence, we will use other biogeochemical variables (co2/ch4 fluxes) to constrain the surface inundation in our future work, but it is out of scope of this merely model development manuscript.
- 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?.
- In the CLM, fh2osfc is also affected by soil and atmospheric changes, however, Fig 5 shows that the changes in microsigma influence fh2osfc on a point scale. This change is difficult to point out in larger spatial scale as in Fig 4, where the spatial averages are used.