Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-233-AC3, 2019

Geoscientific Model Development Discussions

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Interactive comment on "Lower boundary conditions in Land Surface Models. Effects on the permafrost and the carbon pools" *by* Ignacio Hermoso de Mendoza et al.

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Received and published: 6 May 2019

We thank the reviewer for his comments, which show that we need to better put the article into context and emphasize its main conclusions. We have made many editorial corrections, including the bibliographic mistakes, and added several paragraphs to address the reviewer's questions.

1. **There are several other papers that have examined a similar topic (Alexeev et al, 2007, Nicolsky et al, 2007, and Lawrence et al., 2008). From my reading of this paper, in comparison to what I recall about these other papers, I think that there is some new information here, but I would strongly recommend**

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that the authors strive to make it clear how their study is distinct from these previous studies (e.g., global versus site level assessment).

We have added a paragraph in the introduction, to explain the differences between our study and those mentioned by Dr. Lawrence. To improve the modeling of permafrost, the papers mentioned by Dr. Lawrence pointed out that the subsurface model must be thick enough (at least 30 m) to capture the damping of the annual surface temperature. These papers increased the thickness of the CLM3 from 3.5 m to different depths to capture decadal and centennial variability during the 20th century. Alexeev et al. (2007) used of a slab of variable thickness (30, 100 and 300 m) at the bottom of a several layers representing the soil with high resolution, in order to have sufficient depth to absorb decadal to centennial signals. Nicolsky et al. (2007) did the same by using additional soil layers to increase the thickness of the model to 80 m, which they applied at specific locations with deep permafrost. Lawrence et al. (2008) tried depths up to 125 m by adding extra bedrock layers, and determined how this affected the extent of near-surface permafrost. These studies did not consider crustal heat flux and although they studied the impacts of model depth in near-surface permafrost, they did not analyze the associated effects to the permafrost carbon pool. In our paper, we look into the impacts of the thickness of the subsurface and the crustal heat flux, not only on permafrost but also on the heat content of the subsurface and on the carbon pool, in simulations for the 20th century that we continue until 2300 under two scenarios of anthropogenic emissions.

2. **The paper only assesses the impact of extending the depth of ground beyond the default 42m used in CLM4.5. For more context, it would be very useful to also include a simulation with much shallower ground (e.g., 3.5m or so) as is used in most current generation ESMs. My guess, based on the above cited studies, is that the impact of going from 3.5m to 42m is much larger than going from 42m to 342m. That is an important message**

that needs to be maintained. I wouldn't say that every analysis in the paper needs to be repeated with this shallower version, though for the sake of consistency, it might be worth considering, but for at least the baseline big issues (impact on near-surface permafrost), it should be shown/discussed.

As suggested by Dr. Lawrence, we have included a new simulation with shallow ground (3.8m) by removing the bedrock in the model. We already observed that increasing the thickness of the model provides diminishing returns, therefore, reducing the thickness of the subsurface from 42m to 3.5m has a bigger impact than going from 42m to 342m. The impact of progressively increasing depth depends on the timescale of the simulation, so the increase from 42m to 342 m is more significant for a millennial-scale simulation than it is for our centennial-scale simulations. In this new simulation, we observe that decreasing the subsurface thickness from 42m to 3.8m has a much larger effect in the soil carbon pool than that of increasing it from 42m to 342m. The loss of soil carbon during the 1901-2300 period is increased by 4.4% in the RCP 8.5 scenario, but more importantly by 35% in the RCP 4.5 scenario. The emissions of methane are consistently 1-2% higher for a subsurface of 3.8m than one of 42m, which results in these increased losses of soil carbon. It has already been well established that deepening the bottom boundary below 3.5m improves representation of permafrost significantly, bringing the simulated extent of present permafrost much closer to the observations (Alexeev et al, 2007, Nicolsky et al, 2007, and Lawrence et al., 2008, Koven et al., 2013, Slater Lawrence, 2013). We have not detected a significant decrease in the areal extent of near-surface permafrost, but decreasing model thickness from 42m to 3.8m affects the thickness and depth of permafrost. We have included this point in the discussion, and we have also emphasized the logical conclusion that can be inferred from the diminishing returns to subsurface thickness and the optimal depths. Increasing subsurface thickness produces modest improvements, but reducing it introduces serious miscalculations to subsurface temperature, permafrost and soil carbon.

3. **There are way too many figures, perhaps even an excess of a factor of 2. Many figures are included that essentially show no change. That doesn't need to be shown in a figure and can easily be characterized in text or a table. The authors should carefully consider each figure and ask whether or not this figure is needed to tell the story. If it isn't required, then remove it, keeping in mind that if the story is that the impact is small (which is part of the story), then that can be stated in words.**

   We agree that the number of figures is too large, and we have reduced it significantly. Following the recommendation of the reviewer, we have removed from the main paper many figures that show very small changes and that can be sufficiently explained in the text or with the support of the tables. These figures have been moved to supplementary materials, which we will submit along with the revised version of the paper. We have moved Figures 9, 10, 11, 12, 14, 15, 17, 23, 24, 28 and 29, cutting the number of figures in the main body of the paper from 29 to 18. We have kept Figure 16 although it shows only a small difference, to have at least one figure showing the evolution of near-surface permafrost, and Figures 19 and 20, because they show the significant differences produced by the crustal heat flux to the evolution of the soil carbon pool. We have also changed Figure 18 significantly, to show the differences to the original model in the same way as Figures 21, 25 and 27 do. We have also eliminated the 2000 CE time frame in Figures 18, 21, 25 and 27, which allows us to enlarge these maps.

4. **Finally, I think the authors need to carefully consider what their main messages are and, in parallel, put these messages into into context. Currently, they dutifully report about the % change (down to tenths of a percent in many cases) that arises from a deeper column. From my perspective, in the grand scheme of things in Earth System Modeling today, errors of order 1-2% out to 2100 or 2300 are not first order problems. Uncertainties in climate projections and many other simulated land processes are likely**

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having a much bigger impact on permafrost simulations than the depth of the ground column (once you get beyond a depth of 30m or so). If the authors want to argue otherwise, that's fine, or they can acknowledge that these deep depths may only be relevant on very long timescales or for very specific quantities. To this end, I would like to see something more in the form of recommendations.

We agree that the order of these errors are small compared to other sources of error, and we will not argue otherwise. We however defend that these small errors are very easily avoidable, because the implementation of a crustal heat flux and the extension of subsurface thickness is justified, easy to implement and computationally cheap. We have added a new paragraph at the end of the discussion, where we acknowledge the small scale of the corrected errors, but at the same time arguing our point. We also acknowledge that it is more important to not drop subsurface thickness below 40m than to extend it to 200m, but that the importance of a thick subsurface increases with the time scale of the simulation. We provide a explicit recommendation to have a subsurface thickness of at least 40-50m for a correct reproduction of near-surface permafrost, and increase it to 200 m to avoid errors in the order of 1-4%, even more if we were to include deep carbon deposits in the model.

The reviewer also made several minor points, which we address point by point:

1. **The reference for CLM4.5 is not Bonan (et al. 2013), it should be Oleson et al. (2013).** We have corrected this reference.
2. **P.4, line 18: Kirtman et al. is not the correct reference. Kirtman lead the near-term decadal prediction chapter, not the long term projections chapter of AR5.** We have corrected this reference with Collins et al., 2013 (Climate Change 2013: The physical Science Basis. Long-term Climate Change: Projections, Commitments and irreversibility).

3. **The key reference for the soil biogeochemistry in CLM4.5 is Koven et al. (2013).** We have corrected this reference.

4. **P.9, line 25: This sentence is not quite correct. Glaciers are represented in CLM4.5 as columns of ice (42m thick, as with the soil). In CESM2, there is the option to run with an ice sheet model beneath CLM, but even in that situation, CLM is still representing the surface mass balance over glaciers and then passing that information to the ice sheet model.** We have corrected this sentence. It now states that CLM4.5 represents the interior of Greenland with the upper 42 m of ice and passes this information to the land-ice model, but it does not represent the soil.

5. **One thing that might be worth considering with respect to impact is what the impact might be from having a deep column on the vulnerability of yedoma (not treated in CLM, but with variable soil depths introduced into CLM5, could potentially be). Yedoma is located deeper in the soil column 5-20m (?) and therefore may be susceptible to the specified soil thickness.** We have added Yedoma and frozen thermokarst deposits as an example of deep carbon deposits in the discussion. These hold an estimated 211 +/- 160 PgC of carbon in depths up to 50 m (Strauss et al., 2013). Our study shows that the thawing of intermediate-depth permafrost is largely overestimated by the 42 m subsurface, therefore an appropriate subsurface thickness of 200m would be necessary if these deep carbon deposits were included in the model.

6. **Figure 18: You have to study this figure very hard to see the differences. Maybe it should be removed or difference maps should be shown instead of mean states.** We have changed this figure to show the active layer thickness of the original CLM4.5 and the differences between the modified versions of the model and the original model. We have also changed the color scale to be colorblind-friendly.

7. **P.29, line 12-14. The correct references for variable soil thickness in CLM5 are Brunke et al., 2016 and Swenson and Lawrence (2015).** We have corrected these references.

Finally, note that by requirement of the executive editor of GMD, we will change the title of the manuscript to “Lower boundary conditions in Land Surface Models. Effects on the permafrost and the carbon pools: a case study with CLM4.5”.

References:

Strauss, J., Schirrmeister, L., Grosse, G., Wetterich, S., Ulrich, M., Herzschuh, U., Hubberten, H. W. (2013). The deep permafrost carbon pool of the Yedoma region in Siberia and Alaska. *Geophysical Research Letters, 40*(23), 6165-6170.

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