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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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This paper evaluates the influence of modeling decisions regarding the depth of the soil. It finds that with shallower soils, the influence of the bottom no heat flux boundary can be detected on century timescales.

The study is pretty straightforward and the conclusions are essentially as expected. 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 that the authors strive to make it

clear how their study is distinct from these previous studies (e.g., global versus site level assessment).

I don't have many technical concerns with the paper. It is fairly straightforward. Run the model at varying soil depths and with and without geothermal heat flux and assess the impact on simulations. The authors covered issues that I would be worried about regarding spinup and computational costs. My main recommendations, in addition to that mentioned above, are:

1. 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.

2. 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

3. 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 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 quantiies. To this end, I would like to see something more in the form of recommendations. An example recommendation could be that if the main interest is in projections of intermediate-depth permafrost thaw, then a deep ground column is required, but if the main interest is in near-surface permafrost, a depth of roughly 50m may be sufficient (and necessary).

Minor points:

1. The reference for CLM4.5 is not Bonan (et al. 2013), it should be Oleson et al. (2013).

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.

3. The key reference for the soil biogeochemistry in CLM4.5 is Koven et al. (2013)

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.

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 could be). Yedoma is located deeper in the soil column 5-20m (?) and therefore may be susceptible to the specified soil thickness.

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.

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)

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