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Interactive comment on "Projecting circum-Arctic excess ground ice melt with a sub-grid representation in the Community Land Model" *by* Lei Cai et al.

Anonymous Referee #1

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This is a resubmission of a previous discussion paper that was retracted by the authors following review: https://www.the-cryosphere-discuss.net/tc-2019-230/. For context, my previous review is available here: https://doi.org/10.5194/tc-2019-230-RC1.

The single-point modelling has been changed to simulate 3 geomorphic units in the Lena River delta, rather than Yakutsk and the North Slope of Alaska in the initial submission. The global simulations include comparison of a no ice case, sub-grid representation case, and a grid-average case.

My main criticisms of the first submission were that (a) the results were not validated in any meaningful way, (b) the empirical basis for the parameterization of excess ice

was lacking, and (c) that there was not a clear comprehension of empirical ground ice studies and knowledge of ground ice conditions.

I have read up to the results section and made several observations pertaining to points (b) and (c) above. The points below do little to reassure me of my concerns with (b) and (c) from the previous version. Furthermore, in my previous review I pointed out that references mentioned in text were missing from the reference list. I expected such a simple item would be remedied, but in the first paragraph of the introduction alone, the following references are missing from the list: Walter et al. (2006); Schaefer et al. (2011).

Given these concerns, I have not formally reviewed the results or discussion.

1. It is unclear from the text whether the authors appreciate the difference between "excess ice content", "volumetric ice content", and "visible ice content", as the terms are seemingly used interchangeably or confused.

In different places in the paper, the authors have indicated the CAPS values represent volumetric ice content, excess ice content, and visible ice content. The authors have misinterpreted the legend for the Circum-Arctic Map of Permafrost (CAPS) in their Figure 2. They have altered the legend from the original map by removing the clause stating "visible ice in the upper…", and now only indicate "Ground Ice Content: percent by volume". They report ice contents from the Circum-Arctic Map as volumetric ice content (lines 216, 224) in the text. Then, in the figure 2 caption, they suggest the CAPS values represent the "Spatial distribution of excess ground ice" – very confusing. The CAPS legend, and the Permafrost Map of Canada (Heginbottom et al. 1995) legend on which the CAPS compilation is based, both clearly indicate that the ice content reported is the visible ice content (as the authors correctly indicate on line 177). The legend on the Heginbottom et al. (1995) map indicates this visible ice percentage accounts for "segregated ice, intrusive ice, reticulate ice veins…". The percentages on the maps do not correspond to volumetric ice content (in the strict sense), which also include the pore ice fraction.

Lines 185 to 190, the authors report that Yedoma is "characterized by massive ice wedges leading to typical average volumetric ice contents in the range from 60% to 90%" (line 188). They then state: "We therefore set the volumetric excess ice content to 70%". Nowhere in the text do the authors mention the soil porosity, which is key to estimating excess ice content given only volumetric ice content. For example, if one assumes a soil porosity of 0.5, then volumetric ice contents of 60-90% represent excess ice contents of about 10-40%. Assuming an excess ice content of 70% based on volumetric ice contents of 60-90%, as presented above, is problematic. I refer the authors to Harris et al. (1988) for definitions of volumetric ice content and excess ice content.

Other examples that seemingly use the terms interchangeably: Line 137-138 "volumetric ice contents ranging from 60-80%" and in the next sentence, "higher excess ice contents are found in Pleistocene sediments. . ."; Line 193 "For the low ice landunit, we assume both a significantly lower volumetric ice content and a smaller vertical extent of the excess ice body"; Table 1. The caption reads "excess ice initialization scenario", but the table header indicates "Volumetric Ice content". Presumably, porosity is available, so why not also present the readers with excess ice content?

Finally, the term "ice content" (line 198) is also used on its own, as is "Overall Ground ice content" in Table 2, further complicating interpretation by the reader. What type of ice content? I'm left wondering throughout.

2. The authors suggest that high ice classes mapped on the Circum-Arctic Map of Permafrost and Ground ice Conditions (CAPS), designated in the submitted paper text as chf, chr, and dhf partly coincide with Yedoma areas and are "broadly oriented at the excess ice contents and distribution in intact Yedoma" (line 186-87).

The high ice landunit is considered representative of Yedoma. I'd like to point out the two maps below. Figure 1 shows the areas of chf, chr, dhf highlighted in red. Figure 2

shows the distribution of Yedoma from Schuur et al (2015). The area mapped as chf, chr, and dhf is much more extensive than areas mapped as Yedoma. For example, a large portion of the Canadian Arctic Archipelago (CAA) is mapped as chr: continuous permafrost that has high visible ice content (>10%) and thin overburden cover (5-10m) and exposed bedrock. Most of the CAA was glaciated and includes no Yedoma. It therefore seems inappropriate to me that vast areas such as this include a considerable fraction of the high ice landunit in the modelling that represents Yedoma.

The high ice landunit cryostratigraphy (70% excess ice in the upper ∼8 m), may reasonably represent ice-rich Pleistocene deposits where permafrost has aggraded syngenetically, or local areas where large bodies of buried glacial ice occur just below the permafrost table. However, I can't think of situations where 70% excess ice content in the upper 8-10m would be reasonable for other deposits in which permafrost has formed epigenetically, given the typical decline in ice content with depth in epigenetic permafrost (e.g., French and Shur, 2010; Fig.2; Gilbert et al, 2019). I realize the authors acknowledge that the cryostratigraphies prescribed in the simulations are a coarse first-order approximation. However, the assumption that areas mapped with high ice content on the CAPS include significant areas where ground ice content is similar to thick Yedoma deposits, including those defined on the CAPS map as chr, seems particularly unrealistic and poorly justified.

It would have been simple to overlay CAPS and Yedoma areas in a GIS and examine the overlap within chf, chr, and dhf to better inform and substantiate landunit parameterizations/area weights.

3. The authors provide a rationale for the excess ice content in the high ice landunit (for global simulations), which is commented on above, but provide little rationale for the medium and low ice content landunits (lines 193-200). One reference to an empirical study is provided (Line 197). The authors indicate that the excess ice content and distribution for the low ice landunit "account for a wide range of different excess ice conditions found throughout the permafrost domain" (line 197-198). It would have ben-

efitted the reader if some of these excess ice conditions were elucidated, with pertinent references.

4. The authors state that subsidence of “more than 10 meters” (line 203) could occur if all ice melted from the high ice landunit in the global simulations. Earlier, the authors indicate that “we put excess ice in all the soil layers between 0.2 meters below the active layer and the bottom of hydrologically-active soil layer (8.5 meters)”. As it is written, >10 m of subsidence is implied from thaw of <8.5m of ground.

5. The authors indicate that abundant field data in the Lena River delta provide a good basis for initializing ice conditions in refocused single-point simulations. I fully agree that simulations in areas with good available data is crucial. However, the authors in fact report no measurements of excess ice content anywhere in section 2.2 (only some volumetric ice contents are provided). It would benefit the reader to have some of these examples if there is abundant field data.

I am also confused by the authors' interpretation of the data that is presented in this section. For example, in Line 136 the authors indicate that ice wedges extend to 9 m depth in the Holocene terrain unit, and that there are volumetric ice contents of 60-80%, citing Schwarmborn et al. (2002) and Langer et al. (2013). Schwarmborn et al. (2002) indicate much smaller ice wedges in the Holocene sediments: “and subaerial or buried ice wedges of 2–3m in height and width are common.” (p. 123), and I cannot find wedge dimensions in Langer et al. (2013). I can only find mention of ice wedges that extend deeper (5-10 m) in the Ice Complex (Yedoma) unit in Schwarmborn et al. (2002).

The volumetric ice contents of 60-80% reported for the Holocene unit are seemingly from Langer et al. (2013, p.13) who indicate: “The elevated rims are usually covered with a dry moss layer underlain by wet sandy peat soils featuring massive ice wedges. The volumetric water/ice content of the peat soils typically ranges from 60 to 80%.”. This value appears to refer to the volumetric ice content of the mineral soil

between ice wedges, rather than to an average representative value for a terrain unit or cross-section that includes both the icy soil matrix and ice wedges. At the scale of the modelling, this is what is pertinent, otherwise the contribution to ice content in the upper permafrost from ice wedges is not accounted for.

6. Line 106: “The added ice is evenly distributed within each soil layer”. In Figure 3, ice it not depicted as evenly distributed in the cross-sectional diagrams. Tile 4 shows large ice wedges, tile 3 a discontinuous (across the landunit) body of ice. The model does not represent ice in this way. These diagrams should reflect that ice is evenly distributed and consistent with the depictions showing “Present” and “Future” conditions.

References French, H. and Shur, Y., 2010. The principles of cryostratigraphy. Earth-Science Reviews, 101(3-4), pp.190-206.

Gilbert, G.L., O'Neill, H.B., Nemec, W., Thiel, C., Christiansen, H.H. and Buylaert, J.P., 2018. Late Quaternary sedimentation and permafrost development in a Svalbard fjordâĂŘvalley, Norwegian high Arctic. Sedimentology, 65(7), pp.2531-2558.

Harris, S.A., French, H.M., Heginbottom, J.A., Johnston, G.H., Ladanyi, B., Sego, D.C. and Van Everdingen, R.O., 1988. Glossary of permafrost and related ground-ice terms.

Heginbottom, J.A., Dubreuil, M.A. and Harker, P.A., 1995. Canada, Permafrost. National Atlas of Canada. Natural Resources Canada, 5th Edition, MCR, 4177.

Langer, M., Westermann, S., Heikenfeld, M., Dorn, W. and Boike, J., 2013. Satellite-based modeling of permafrost temperatures in a tundra lowland landscape. Remote Sensing of Environment, 135, pp.12-24.

Schuur, E.A., McGuire, A.D., Schädel, C., Grosse, G., Harden, J.W., Hayes, D.J., Hugelius, G., Koven, C.D., Kuhry, P., Lawrence, D.M. and Natali, S.M., 2015. Climate change and the permafrost carbon feedback. Nature, 520(7546), pp.171-179.

Schwamborn, G., Rachold, V. and Grigoriev, M.N., 2002. Late Quaternary sedimentation history of the Lena Delta. Quaternary international, 89(1), pp.119-134.

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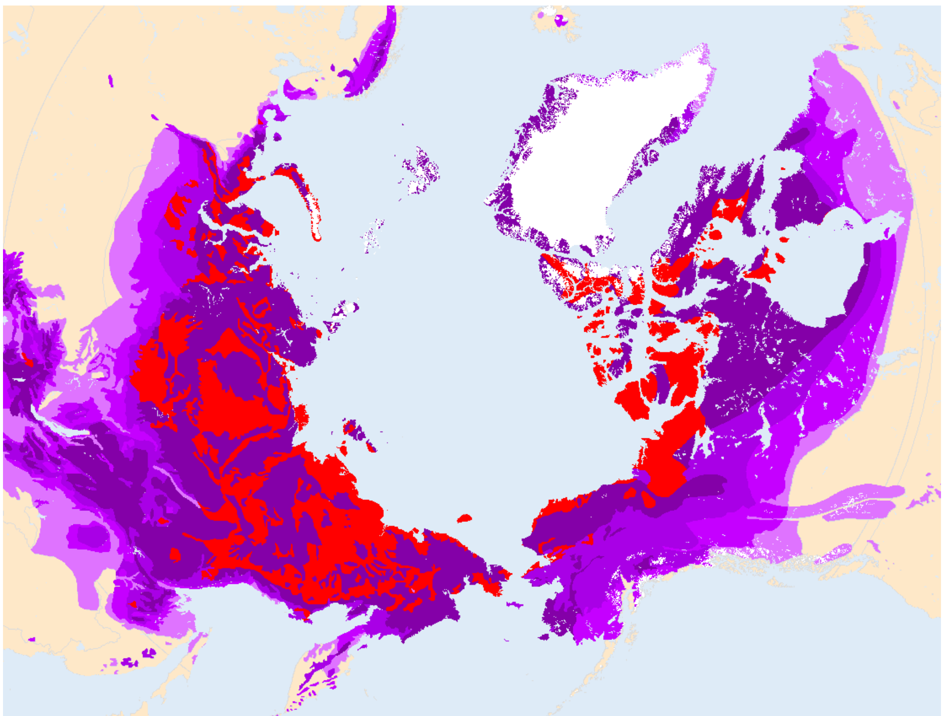

Fig. 1. Mapped by reviewer from the CAPS GIS files.

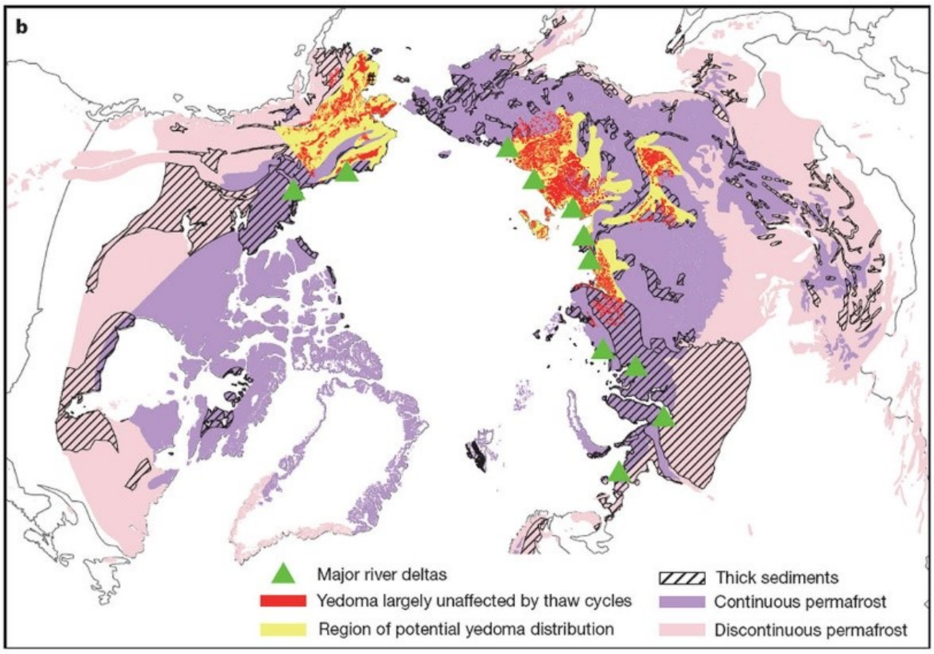

Fig. 2. From Schuur, E.A., McGuire, A.D., Schädel, C., Grosse, G., Harden, J.W., Hayes, D.J., Hugelius, G., Koven, C.D., Kuhry, P., Lawrence, D.M. and Natali, S.M., 2015. Climate change and the permafrost car