Anonymous Referee #1 Received and published: 1 June 2020 This is a resubmission of a previous discussion paper that was retracted by the authors following review: https://www.the-cryosphere-discuss.net/tc-2019-230/.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.

Authors' reply: We appreciate your valuable comments which have contributed much to this new revision of our manuscript. Here we respond to your two (remaining) main concerns. The individual points have been addressed below.

First of all, we have tried to clarify the scope of the study in the new manuscript, which is to provide a proof-of-concept for how heterogeneous excess ground ice can be represented in a global Land Surface Model (LSM) used in Earth System Models (ESMs). While much work remains before excess ice is represented in a fully satisfactory way in ESMs, we believe this study represents an important step forward compared to the current generation models, which for the most part fully ignores excess ground ice (only representing pore ice). Much development of CLM (and other LSMs) in recent years have aimed at mechanistic representation of key features, even when improvements to the model performance cannot be demonstrated. As an example, the latest version of CLM showed an apparent degradation in representation of snow water equivalent at global scale, despite mechanistic improvements in snow physics (Lawrence et al. 2019). We believe our model enhancement is in line with this aim, as it accounts for the effect of heterogeneous excess ice on hydrology and thermal properties in a physically sound way, even though there are great limitations in the current study, especially related to the initialization of excess ice.

Secondly, we have now clarified the terminology. As you correctly pointed out, the previous version of the manuscript was ambiguous here, which understandably gave concern about the use of observational studies. We want to highlight here again that we fully recognize the limitations in excess ice initiation in our study. The observational studies listed in the manuscript are not intended to be replicated here but are used to motivate the use of three broad excess ice classes, which should be revisited in future studies.

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.

Authors' reply: We agree with the referee's comments that the terminology about ice content is somewhat unclear throughout the manuscript that could lead to misunderstanding of the main purpose of this study. But we do not believe we misrepresented the physical properties of ground ice overall when incorporating them into the structure of the large scale land surface model. The physical properties of ground ice used in our model development is only for the excess ice bodies that exceed the pore space of soil. In our model development, we do not address pore ice physics because it is already represented in the original CLM model, with the output variable named "soilice". The melting of "soilice" in the CLM5 does not cause surface subsidence as this ice only exists as part of pore space. Therefore, we emphasize that volumetric ice content in this study refers only to excess ice bodies. We agree that directly applying the groun ice content in the CAPS data is not necessarily an accurate way, while we have to make sufficiently simple classes of ice content levels to avoid over-parameterization. We think that using the volumetric ice content provided by the CAPS data is generally valid for the purpose of this research since the CAPS data is based mostly on "visible" ice bodies (Heginbottom et al. 1995). We have clarified the definition of "volumetric excess ice content" following Harris et al. (1989) in the methodology section. We have also discussed the limitation of applying the ice content values in the CAPS data in our model development in the discussion section.

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 C3 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.

Authors' reply: We agree that overlaying the Yedoma coverage information and the CAPS data can give a better interpretation over the Yedoma region. However, the excess ice content and located depth of ice wedges out of the Yedoma region is still unclear and lacks observational support. Although we fully acknowledge the importance of accurately representing different Yedoma cover in the model, for the sake of model representation of permafrost thaw processes, having an accurate projection over the Yedoma region does not improve the projections of the excess ice melt over the whole circum-arctic in general. Since the main purpose of our study is to represent permafrost thaw processes on a global scale, we make a decision to initialize different kinds of ice wedged ice as "Yedome type ice". As we understand this may not be fully representing reality, we added discussion on how these initialization scenarios brings uncertainties to surface subsidence projections in the discussion section. The high excess ice content and the relatively cold climate where the high ice landunit is located make the wedged ice almost impossible to melt out completely by the end of the 21st century. The remaining part of the excess ice at the bottom has little effect on the surface subsidence. In this way, surface subsidence projections by 2100, initializing Yedoma type ice at the Yedoma region does not substantially affect the final result in our model simulations.

As we write in the discussion section, the purpose of simulation on top of this first-order scenario is to show how our model development can represent permafrost thaw processes on a global scale. Our modeling result shows that the current version of the CLM5 can represent permafrost degradation process with a wide range all the way from continuous to discontinuous permafrost and even no permafrost with the developed sub-grid representation of excess ice. The surface subsidence in the sub-grid representation produces greater heterogeneity to the land surface. Talik forming can also be retrieved during the degradation process. All of the above are novel progresses that no other state-of-the-art global land models can represent.

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

Authors' reply: The scenario we designed for the low ice landunit is based on previous studies that the segregated ice is widely distributed throughout the permafrost area. We have added more reference that segregated ice has been widely distributed throughout the permafrost area, both continuous and discontinuous permafrost (Line 239-246). We also provide an additional empirical excess ice volumetric content (25%) and located depth (ALT+0.2 ~ALT+1.2) to the low ice landunit. For the mid ice landunit, the volumetric content of excess ice and located depths are set in between the low and high ice landunits, which are also based on empirical data. As we mentioned, there is a lack of dataset on ground excess ice with enough information helpful for our sub-grid excess ice representation. For this reason, this is our best effort to make a possible scenario of excess ice distribution based on the best dataset (the CAPS data) at this time, even though it only provides generalized information and has been released for more than 20 years. Due to the lack of adequate information in excess ice distributions, the purpose of this study is not to make an accurate estimate of excess ice melt and surface subsidence in the 21st century, but rather to develop a functionable process within a land surface model on a global scale. Once there is a new generation of excess ice dataset, the CLM with sub-grid excess ice representation is able to be operational and give more accurate projections of excess ice melt and surface subsidences.

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.

Authors' reply: We have mentioned in the methodology section (Line 115) that the soil layer depth increases accordingly after adding excess ice. In this way, the soil thickness with excess ice added is thicker than 8.5 meters. For example, adding high ice landunit (70% volumetric excess ice content) in the soil layers with the original depths between 1 and 8.5 meters can make the thickness of hydrologically-active soil 18.5 meters in total. > 10 m of subsidence is therefore possible in the simulation. We have added the content above in the main text to make the model design clearer.

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 C5 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.

Authors’ reply: For this single-point case for model evaluation, our goal is not to retrieve realistic excess ice melt, but rather to compare the model results from this study and from Westermann et al. (2016). Initializing realistic excess ice condition does not help the model evaluation in this case because the Lena River Delta has observed hardly any surface subsidence yet, making model-observation comparisons inapplicable. Alternatively, we make model-to-model intercomparisons to evaluate our developed physics and sub-grid representation. So we initialize excess ice strictly following that in Westermann et al. (2016). As a result, our sub-grid representation simulates comparable surface subsidences for each sub-grid landunit compared to Westermann et al. (2016), proving the reasonability of our developed sub-grid representation of excess ice. We have added the above clarification in the main text (Line 157-160).

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.

Authors’ reply: Although in the schematic figure and in reality, the ice is not distributed evenly, the framework of CLM and our developed sub-grid representation is able to convert this uneven distribution of excess ice into evenly-distributed excess ice landunits in the CLM. The relative locations of excess ice bodies does not matter because CLM does not include horizontal heat and water fluxes (we have mentioned it in the discussion section). The set-up of excess ice in the CLM can be treated as “squeezing” all excess ice (of the same type) into a part of grid point with evenly-distributed excess ice and the other part of the grid point without excess ice.

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