Review comments for the manuscript No. tc-2020-353,
"PISM-LakeCC: Implementing an adaptive proglacial lake boundary into an ice sheet model"
submitted to The Cryosphere by Sebastian Hinck et al.,
reviewed by Torsten Albrecht (PIK)

General comments:

Hinck and colleagues investigate the role of ponded proglacial lakes along the margin of the Laurentide Ice Sheet during glacial retreat. Assuming that, similar to marine-terminating glaciers (e.g. in Antarctica), the adaptive boundary conditions influence the (land-lake-terminating) ice sheet's stress balance in various ways. They can alter the ice flow (ice streams) and hence the overall ice sheet's geometry and stability. The changing ice load, in turn, results in the isostatic adjustment of the underlying lithosphere, which hence affects the formation (and demise) of lakes with up to several hundred meters depth in the vicinity of the ice sheet. By considering this feedback in a coupled model system applied to the North American ice complex the authors find in some lake regions self-amplified deglacial retreat, similar to what is commonly discussed for the Antarctic Ice Sheet in terms of the marine ice sheet instability (MISI). The significance of this "lake"-effect, comparing the four conducted experiments, is in deed surprising. Hence, the scientific insights of this study would be certainly a valuable contribution to the paleo ice sheet modelers community within the scope of "The Cryosphere".

The authors created a method based on a simple and efficient 4-neighbor "connected components (CC)" labelling algorithm that determines ocean and multiple lake basins for a given (or processed) bed topography and estimate the corresponding water levels by iterating over a set of increasing water levels, without including computational-expensive flow routing techniques. The standalone models "LakeCC" and "SL2dCC", adapted from Hinck et al., 2020, were implemented in the open-source Parallel Ice Sheet Model (PISM), which already comes with an solid-Earth deformation module. As the title of this study suggests, the ice model's marine boundary conditions have been generalized for this lake-coupling procedure. This does not mean that water density and calving rates have been simply adjusted, but that a whole PISM sub-module has been created with many functions and special considerations. For numerical stability reasons, the authors consider prescribed (ad hoc) lake filling rates that permit gradually evolving lake water levels for changing bed topography and ice margins.

The focus of this study is the description of the model implementation into PISM with a very detailed technical Appendix (which would also fit well to the Geoscientific Model Development) and to run simple deglacial simulations to test for its relevance as compared to the default case without the LakeCC method. From a modeler's point of view, this study could benefit from a few more sensitivity tests, that could help disentangling the individual contributions of the relevant processes acting at the lake-ice-bed boundary ("modification of the thermal regime at the submerged ice base, formation of ice shelves, increased ice loss due to melting and calving, and enhanced basal sliding near the grounding-line due to decreased effective pressure at the ice base… "). Hence, the reader would not only learn "that lakes matter" but also "why lakes matter". Also, the authors state, that the simulated ice sheet margins do not match well with reconstructions based on geological evidence, which is not the focus of the study. However, I encourage the authors to follow some of my suggestions in the specific comments below (mainly regarding the initialization of the LC bed deformation model and the non-linear precipitation dependence on temperature index), which likely can improve the simulation outcome.

Overall, the study is well structured and the manuscript clearly-arranged. The draft consists of 36 pages including 9 main figures, 12 Appendix pages and 57 references.

Specific comments:

drainage events

l. 22 "Reorganization of the lakes' drainage networks and sudden drainage events due to the opening of lower spillways may have impacted the global climate by perturbing the thermo-haline circulation system of the oceans (Broecker et al., 1989; Teller et al., 2002)." and l. 131: "For simplicity the fill-rates in our model are assumed to be constant." and l. 142: "If a basin disappears because it merged with the ocean, the lake level is gradually changed until sea level is reached, and then removed. "

→ This is super exciting, but is it correct, that due to numerical stability requiring gradual lake filling, such events would be prohibited (or at least smoothed over long times) in this implementation?

lake merging

l. 127: "However, there are special cases, such as adding a lake basin to an existing lake or adding a basin that has previously been connected to the ocean, that need more advanced treatment. For more details on this, see Appendix B."

l. 134: When water level is rising, this common level is chosen to be the lowest water level of that lake, h_{min}, while the highest level, h_{max}, is selected for the falling water level.

l. 139: "Only when h has exceeded the current (local) lake level, does its value get updated."

→ What exactly is the current (local) lake level? A simple sketch could help here (maybe added to Fig. 1?). An explanation as in Hinck et al., 2020 may help: *"Until a patch merges with another one that is a sink, i.e. the lake overflows, the current level h is stored for all associated cells."*

2d sea level

l. 157: " to determine the two-dimensional sea level field"

→ I recommend to mention here early in the manuscript that, although the sea-level in PISM is treated as a 2D variable, in this application the value in each cell in every time step equals either a global constant or NaN.

l. 522: "Here, we present the implementation of a sea level modifier, which takes advantage of possibility of a spatially variable sea level..."

→ The manuscript could benefit from some motivation, why it is not sufficient to just update the already available 2D sea-level field with the various target lake levels (as general water level) and simply add a 2D field of corresponding water densities? I guess the numerical instabilities requiring a gradual lake filling (and hence more fields as the lake level) are one important argument for it, what else?

grounding line treatment

l. 92: "The till below the water level next to the ice margin and grounding-line are assumed to be saturated. " and l. 172 "Cells with grounded ice below water level next to a lake are assumed to have saturated till. This reduces the effective pressure at the base of the ice sheet and reduces basal resistance in this location."

→ I fully agree that this is a valid model choice. However, the underlying assumption of having saturated till within one grid cell length upstream of the grounding line (here 20km) has often been criticized within the community. Apparently, it highly increases the ice sheet's sensitivity (e.g., Golledge et al., 2015), in particular in combination with the basal melt interpolation (has this also been used)? For better comparison with other model studies it would be helpful to state the expected consequence of this model choice ("higher sensitivity"). Generally, this study could very much benefit from a few sensitivity runs in order to attribute the relative effects of some of the different processes named in Sect. 2.3 (and Fig. 2). Another (maybe more physical) way would be initializing all water-covered cells as saturated (https://github.com/pism/pism/pull/425), such that an advancing grounding line would not get temporarily stuck on initially unsaturated till (until it reaches saturation), which surprisingly seems to have a similar effect on paleo time scales, also for grounding line retreat (Albrecht et al., 2020a; Fig. A2).

marine vs. lake boundary conditions

l. 165: "Generally, the parameterizations provided by PISM for a marine boundary are applied analogously at the lake interface. This, however, may not be the optimal treatment in every case. The model might benefit from future implementations of advanced or more specialized lake boundary treatment. Such limitations are discussed in Sect. 2.4." and l. 175: " the same parameterization for ice base temperature and sub-shelf mass-flux are used."

→ As this marine assumption may overestimate sub-shelf melt rates in lacustrine environments, it would be helpful to state about what average melt rates we talk here, and what its relative contribution to the ice sheet mass balance is (aggregated rates) and on the enhanced deglacial retreat.

l. 234: "At the shelf base (2), mass flux is parameterized using the model proposed by Beckmann and Goosse (2003)."

→ In this paragraph the authors mention that the melt pump is expected to be weaker in fresh water due to lower buoyancy, while it could be also stronger as the temperature at the grounding line is likely higher than in marine environments. What estimate provides the Beckmann-Goosse model when the default values (35 g/Kg and -1.7°C?) were changed (0 g/Kg and +4.0°C) accordingly? Would the effective melting be higher or lower than for the marine default values and could this still be realistic (even though it was designed for marine environments)?

calving

l. 247: "Implementation of a more physically based calving model capable of accurately parameterization of mass losses in both lacustrine and marine environments, will be needed (Benn et al., 2007)."

→ The authors are also using the Eigencalving parameterization, which applies well for ice shelves in rather confined embayments, as fund around present-day Antarctica. Can you roughly estimate the relative contributions of thickness calving or Eigencalving in this study (maybe by comparing the two experiments with different thickness calving thresholds)?

l. 285: "...large ice shelves like those seen in the LCC experiment are unlikely to have existed."
→ Is there some reference? Does "large" mean covering entire lakes? Or would this imply that really thin (Δh_L = 50m, l. 281) ice shelves are in fact unrealistic.

numerical instability and time stepping

l. 219: "Sudden jumps in water level can trigger numerical instabilities in the ice sheet model"
→ Can you say some more words on the possible reasons for numerical instabilities? Is this due to large areas of grounded ice becoming afloat at once affecting the non-local KSP iterative solution?

l. 83: "In this study we set an upper bound of 0.25 yr for the time step." and l.222: "Future implementations could possibly adapt a volumetric rate instead of fixing the rate of change of water level. By limiting the time step, sudden changes in water level could be performed quicker.
→ Please better motivate this particular time step. Does this choice help with keeping numerical stability with regard to the constant lake filling rate (l. 131)? Is this a best-practice value or is there some relationship between temporal and spatial resolution, as for instance the famous CFL criterion? Is the adaptive time step of PISM's sub-modules for the used resolution usually larger?

solid-Earth feedback

l. 93: "Bed deformation due to ice load is modeled in PISM using the Lingle-Clark model ..." and
l. 210: " If higher resolved input fields are available, e.g. from an external GIA model, the LakeCC model could be modified to do the calculations on that field instead and interpolate the output back onto the ice sheet model grid."
→ PISM has been recently coupled to a global solid-Earth model (VILMA, not published yet). Therefore, PISM (https://github.com/pism/pism/pull/463) can read the history of bed topography change on the ice model grid relative to a (high resolution) reference topography. The real benefit of this external GIA over the internal LC model, however, is that it self-consistently solves for the sea-level equation, i.e. it accounts for self-gravitational effects, which can be very relevant in (deglacial) grounding line migration in marine (or lake) environments. My guess is that lakes could be quite easily included, but this may require a closed water budget between ice sheet, lakes and ocean (l. 213). In any case, this would be rather an option for a follow-on study.

l. 259: "The temporal evolution of the topography provided by NAICE can be used to calculate the uplift rates that are used to initialize the Lingle-Clark bed deformation model of PISM."
→ The NAICE model makes use of a GIA model (which also solves the sea level equation) constrained with many different paleo data types, but it makes use of very simple assumptions on the steady ice state and boundary conditions. Please, provide some more information on how and when the NAICE uplift rates were used. It reads as you would take the LGM state and uplift rates from NAICE and run the Lingle-Clark model from there up to the present day? If this is true, I would expect for a different GIA (bed deformation) model with different mantle viscosities used, that this would imply a (almost

equilibrated) bed topography at present, which may differ from what we observe today, even if the ice thickness would be perfectly reproduced. Can you quantify the misfit in bed topography in your study?

l. 365 *"After deglaciation, the Hudson Bay region is over-relaxed and above PD sea level (see Fig. 8f)." and l. 342: " Our deglacial scenario fails at simulating realistic ice margin positions for the western LIS."*
A probably better way to avoid such a large misfit would be to initialize the Lingle-Clark model from present-day geometry and uplift rates and run it into the LGM state, from which you then start the experiments. Or if you want to make use of the constrained NAICE results at LGM you could make use of the simulated misfit at present an rerun each experiment with the initial bed topography adjusted according to the misfit, such that you end up with a better match at present in the second iteration.

l. 361: "Maximum water depths close to the grounding-line are up to 1000 m. The main reason for this, we assume, is the GIA response modeled by PISM's bed deformation model."
→What if the surface mass balance is simply overestimated (see my comments about index method), which "tends to accumulate too much ice" (l. 363), such that the Lingle-Clark model simply responds (in the correct way) to the higher load?

l. 362: "This simple, two layered Earth model is not capable of handling the extreme deglaciation scenario of an entire continent."
→ I think this statement is a bit harsh, as for the assumptions made, the Lingle-Clark model in fact can handle glacial cycles over an entire continent comparably well. I would agree that it is simple as it uses only one spatially constant mantle viscosity and does not account for self-gravitational effects, which may play a large role here. But my guess would be that the proper initialization of the Lingle-Clark model may bring much improvements here (see comment above).

l. 364: "the rebound was not quick enough."
The PISM default value for the upper mantle viscosity (likely used in this study) is 10^{21} Pas. NAICE, for instance, used a lower value of 4×10^{20} Pas for the upper mantle, which implies a faster rebound. According to the relevance of GIA in this study, one or two sensitivity tests with varied mantle viscosity could bring some more interesting insights.

climate forcing

l. 269: "To prevent ice sheet growths ... above 3500 m elevation, ice accumulation is prevented by setting precipitation to zero in these regions." And also l. 338: "Only by limiting precipitation above 3500 m elevation ... does further expansion of the ice sheets stop. "
What is the motivation for this constraint? Is this related to the findings (of maximum surface elevation) in the NAICE model? Or is it just gained experience with the model setup?

l.96: "The surface mass balance (SMB) is estimated from monthly means of precipitation and surface air temperature fields... Precipitation and temperature fields are interpolated between two distinct climatic states, which are weighted according to a glacial index (see Appendix D1). "
→ Does this mean that the interpolation is between each month of the two climate states?

*l. 295: "Starting from the LGM initial state after the spin-up, the ice sheets laterally expand and gain mass for about 6 kyr. Within this time, the LIS almost doubles its volume, before it retreats during the rest of the simulation." and a*lso *l. 332: "...dynamical equilibrium, as the initial ice sheet was reconstructed using geological evidence of ice margin history and GIA observations, and no ice dynamics were included (Gowan et al., 2016)."*
→ A model drift after initialization is typical. A dynamic equilibrium simulation (with constant dry LGM climate conditions) prior to actual forcing experiments could help here identifying relevant parameter settings that counteract the lateral expansion.

l.335: "The reason for the ice accumulation is assumed to be due to the climate forcing. By linearly interpolating between the warm, humid PD and the cold, dry LGM climate states, unrealistically high accumulation is produced, especially in cold regions and on top of the ice sheet."
→ This seems to be quite some considerable imbalance for applied linearly changing climate forcing. I agree that the interpolation is the most likely candidate here, as precipitation is rather non-linearly related to temperature change (Frieler et al., 2015). This means that precipitation after 6kyr would be much overestimated by the arithmetic mean (index 0.5) between LGM and PD state.

SLE unit

l. 295: " sea level equivalent (SLE) ice volume" and Fig. 4 caption: *"...and also includes ice shelves"*
→ I assume that the authors use SLE as a converted unit of grounded ice volume. If so, please specify the used conversion factor and also mention, that you do not make use of a 'volume above flotation' definition (for good reasons), to avoid misunderstandings.

ice lobes

l. 300: "...ice margin between about 1 and 3.5 kyr is due to formation of small ice lobes that advance into small lakes (compare also with Fig. 6). "
→ Is the ice margin at those ice lobes grounded or floating? Maybe provide some definition here? Is there a difference to an "ice shelf tongue"? There is a reference quite later in the text (l. 375).

ice streams

l. 360: "The formation of ice streams .."
→ Does a speed-up of ice flow imply that there wasn't an ice stream before, i.e. in terms of confined ice flow with speeds above (let's say) 100 m/yr?

channel filter

l. 504: "Finally, to get rid of narrow lakes, which are often caused by the under-resolved topography, the target level is filtered by applying the FilterLakesCC method using a filter size of N_{filter}."
→ Why would this be an issue? Does it mean, that a drainage event can only occur for a channel opening of width larger than N_{filter}?

Technical corrections:

l. 11 and l. 70 "…at the end of the simulation"
→ "… at present-day"

l. 28: "These processes can lead to the formation of ice streams…"
→ or: "…speed-up of ice streams…"

l. 30 "Due to various differences between the freshwater and ocean water, the interactions might be different than at a marine boundary (Benn et al., 2007)."
→ "Due to various differences between the freshwater and ocean water **regime**, the interactions **at the lake-ice boundary** might be different than at a marine boundary (Benn et al., 2007)."

l. 32 "In times when there was the production of large amounts of meltwater that caused the formation of proglacial lakes…"
→ "In **periods with** large amounts of meltwater that caused the formation of proglacial lakes…"

l. 34: " caused by rapid demise"
→ " caused by **a/the** rapid demise"

l. 36: "governed by the negative surface mass balance due to a warming climate"
→ As there are counteracting effects on the surface mass balance (also more precipitation for higher temperatures), it would be good to be more precise here, i.e. "due to **enhanced melting in** a warming climate"

l. 40: "Lake reconstructions of this time suggest water depths up to several hundred meters (Teller et al., 2002; Leverington et al., 2002)."
→ This has been already stated in l. 20, but that's ok.

l. 46: global **mean** sea level

l. 56: "The latter reference discusses concerns when implementing such a lake-ice boundary condition for ice sheet models."
→ "The latter referenced article discusses **challenges for the implementation of** such a lake-ice boundary condition for ice sheet models."

l. 68: "In places where there is ice-inward sloping topography, the lake can rapidly expand, as the water is deep enough that the ice begins to float."
→ "In places where the water is deep enough, the ice begins to float. In regions of ice-inward sloping topography, the grounding line can retreat in a self-amplified manner, which corresponds to a rapid expansion of the lake."

l. 109: "…quite dynamic" → "…particularly dynamic"

l. 110: "Therefore, when dynamically coupling ice sheets and proglacial lakes, steady updating of the geometry is necessary. "

→ "**For the dynamical** coupling **of** ice sheets and proglacial lakes **the continuous update** of **their geometries** is necessary. "

l. 112: "continental**-**sized "

l. 113: " trade-offs have to be **made**"

Fig. 1 caption: "before it overflows into the ocean **or a neighboring lake basin**."

l. 183: "The pressure difference against the submerged ice margin is done analogously to the marine boundary"
→ but evaluated for the fresh water density

l. 207: "a after applying a secondary field."
→ As far as I understand this means "applying a correction" or "adjustment" of the low resolution data. Is this just an "anomaly field added"?

l. 259 "that are used ~~to used~~ to initialize…"

l. 286: "… and Ctrl, without lakes. The PISM default run Def produces…"
→ Would it be possible to use some different font for the experiment names?

l. 293: "topographic setting" → maybe "configuration" would also fit here

l. 299: "Fig. 5 " → Figure 5

l. 300: "southward**-**shifted"

l. 311: "Where ice streams diverge, the surface velocity anomaly partially shows a slowdown of ice flow."
→ Please be more precise, where is this and why?

l. 320: "… in **the** ice volume evolution of the LIS"

l. 325: " CIS" probably means Cordilleran Ice Sheet, please define.

l. 350: "**The r**econstruction"

l. 368: " large response" → "high sensitivity"

l. 369: " upstream **of** the lake boundary"

l. 371: " due to the changes in ice dynamics, the different experiments constantly diverge and features visible in these plots might therefore only indirectly be triggered by the lake boundary, but rather be a result of differences in ice sheet geometry."
→ Maybe split into two sentences. What is diverging?

l. 389: “ice-covered”

l. 392: “ retreat**s**”

l. 406: “ continental ice streams”
→ What does this mean? Is this related to the size?

Table A1 and A2: It would be nice for other modelers, if you could emphasize the options, which were added compared to the PISM base code (A1: index,precip_cutoff, lake_level (lakecc), (,sl2dcc), A2: -lakecc_dz, -lakecc_zmin , -precip_cutoff_height , -use_precip_cutoff_height).

l. 427: “Connected Components Algorithm”
→ capital or not (e.g., l., 457)? But in any case be consistent through the manuscript.

l. 442: “about **the** presence”

l. 442: I think, a common name in numerics for this approach at the boundary is “ghost point method”

l. 453: “Furthermore, two additional 2D diagnostic variables, which can be added to PISM’s output, were added (see Table B1)”
→ Please be more precise. Do you mean the two variables, which are not “general” in Table B1? Maybe use the word “optional”?

l. 465-473: A simple schematic map including such cases (just some grid cells) could be helpful to understand the conditions in this paragraph.

l. 468: “ice-covered”

l. 522: “of **the** possibility ”

l. 528: “model **to** identify”

l. 530: “Only patches that are connected to the margin of the computational domain are considered to be part of the ocean.” and also l. 467: “but are not connected to either of the domain margins”
→ As the southern margin is land, I guess the condition would be “margin below sea level” or so?

l. 551: “usees ”

References, which are not already cited in the study:

Albrecht, T., Winkelmann, R., & Levermann, A. (2020). Glacial-cycle simulations of the Antarctic Ice Sheet with the Parallel Ice Sheet Model (PISM)–Part 1: Boundary conditions and climatic forcing. *Cryosphere*, *14*(2).

Frieler, K., Clark, P.U., He, F., Buizert, C., Reese, R., Ligtenberg, S.R., Van Den Broeke, M.R., Winkelmann, R. and Levermann, A., 2015. Consistent evidence of increasing Antarctic accumulation with warming. *Nature Climate Change*, *5*(4), pp.348-352.

Golledge, N.R., Kowalewski, D.E., Naish, T.R., Levy, R.H., Fogwill, C.J. and Gasson, E.G., 2015. The multi-millennial Antarctic commitment to future sea-level rise. *Nature*, *526*(7573), pp.421-425.