Author response to referee comment RC4 to 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.

> This study is a great concept and shows how ice-marginal lakes can potentially change the modelled retreat of large continental ice sheets. This is mostly relevant to the glacial NH paleo-ice sheets (less so Greenland or Antarctica, or small valley glaciers), but nevertheless the study will still be interest to the many people working on N. Atlantic deglacial climates, because of the great significance of meltwater inputs to the ocean from the Laurentide ice sheet. The authors also demonstrate a process driven by ice-marginal lakes that could lead to rapid retreat of what is raditionally considered a land-terminating ice sheet margin.
>
> There are many uncertainties, perhaps obviously – and it’s good that the authors focus mainly on the different behaviours with/without the lakes, and skip a detailed comparison with geological evidence at this stage in development.
>
> I have listed some specific comments/questions below but my two main concerns for implementation of this new LakeCC scheme are as follows.
>
> 1. Due to numerical instabilities, the model apparently cannot cope with rapid lake drainage/filling. This is of concern because the rapid drainage of lakes into the North Atlantic is one of the main applications of this work, yet lake level changes in a ‘workaround’ solution appear to be limited to 1 m/yr (the gamma parameter in Eq. 1 and Table B2). This represents a maximum of only 25 cm per 3-month model time step. The cause of the instability is not discussed but if it cannot be solved without the current ‘work-around’ then I struggle to see how this new scheme can be implemented in a realistic scenario.
> 2. Use of the marine ice shelf parameterisations is really questionable in a lacustrine setting. Fast sub-shelf melting in marine settings is enabled partly by the density contrast between dense saline ocean water and light fresh melt water. This contrast drives rapid overturning in the shelf cavity, and large heat fluxes. In a lacustrine setting, the density contrast between fresh lake water and fresh melt water is much lower, so presumably the sub-shelf melt rate will also be much lower, for a given temperature forcing. In which case, perhaps a lot of the simulated shelves would really just remain as grounded ice. The authors do of course acknowledge this shortcoming, but don’t then address it. Following on from this point, what is the water temperature in the lake? I can’t see how this is estimated but it is crucial for calculating subshelf melting.
>
> Since there is so little work on this subject it would be great to see this study published. In this respect, I believe that the instability issue either needs fixing or needs much more discussion, so we are convinced it isn’t a symptom of some other underlying problem in the model, and so that we know how the problem of unrealistically slow lake draining/filling can be overcome. Second, because this is largely a model development paper, we need some basic indication of importance of a few critical parameters (in this application, perhaps the till friction angle, ice shelf basal melt, grid resolution). Even some short model runs could help answer that? Finally, although not essential to the concept of the study, if you carry out some more simulations I would also revisit the climate forcing and need for the 3500m elevation limit.

We would like to thank the anonymous referee for reviewing our paper. The original comments are indented, while our responses aligned to the left of the page.

For this review round, several sensitivity experiments were run, which are referred to in our responses. Details about these runs and snapshots are combined in a supplementary document. This document is available online (https://doi.org/10.5281/zenodo.4746501).

We will elaborate a bit more on the instability in the revised manuscript and mention the sensitivity test for higher fill rates. These issues are related to the numeric solver (see responses to reviewer #1 on this matter). When doing additional tests, the stability issues we encountered before were not happening. Furthermore, we will clarify that the model output can not be used for direct freshwater forcing of an ocean model.

In the following, we will respond to all comments and questions.

Specific comments/questions:

> *Abstract (L7), Conclusions, & elsewhere:*
> You don't specifically show ice streams along the continental margin, even in Fig 7. There are regions of faster flow in the LCC run but these don't look particularly stream-like. Because you are using a spatially uniform basal parameters, and a coarse grid, this aspect of ice dynamics isn't well captured by your model. Rather than saying you develop ice streams, I would recommend to simply point out that ice velocity is higher at locations X in the LCC run.

Yes, this was also criticized by the other reviewers. We will rephrase the relevant sentences and rather speak of increased ice flow/velocity.

> > *L22: "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 thermohaline circulation system of the oceans (Broecker et al., 1989; Teller et al., 2002)."*
>
> It's important here to note that drainage could have been under the ice, as well as over it. Lack of subglacial routing option means lakes can only overspill, yet subglacial drainage could lead to outburst events long before the supraglacial spillway is reached. Omitting this subglacial option perhaps helps the experimental design but it deserves at least a mention.

Subglacial drainage is indeed very likely to have happened, for instance during the final drainage of Lake Agassiz during the 8.2 ka event (Gautier et al., 2020). Developing a model to simulate subglacial drainage (and subglacial water storage) would be very complicated and likely would require a very sophisticated model that is beyond the scope of our current study. We will mention this in the text.

> *L34:* Not an important point but the NH cooling was maybe caused by freshwater fluxes (not definitely, as implied here).

Yes, that is true. We will rephrase it in the revised manuscript.

> > *L41 "Apart from their relevance for understanding processes that lead to the demise of the late Pleistocene and early Holocene ice sheets, interest in contemporary proglacial lakes and their role in glacial retreat is growing (Carrivick and Tweed, 2013). Motivations for these studies range from predicting and managing water resources under a warming climate and recognizing possible risks due to glacial outburst floods (Carrivick et al., 2020)."*
>
> I'm very sceptical of this work being usefully down-scaled to the small present-day proglacial lakes, because the parameterisation of ice-lake interactions would be so specific to a given site. Maybe best to stick to the paleo aspect.

That is correct. We will remove this point from the introduction.

> *L79:* Resolution (20 km), is this really sufficient to capture the processes you are aiming at modelling? I understand the limit on computational effort but wonder if the positive feedbacks between lake development and ice stream development could be underestimated. For example a proglacial lake extending for 50 km along the ice sheet margin would only meet the simulated ice sheet at two or three grid points, is that enough to initiate an ice stream in the model? I am not suggesting lots of runs with different configurations, but it would be good to see some sort of sensitivity. For example, run the model at 20km to a couple of interesting points and use these as initial states for some short simulations with higher (and lower) resolution.

Our results show that lakes that are only a few cells wide impact the ice dynamics (see for example Fig. 4). Resolution is, however, an issue in ice sheet models in general, especially when it comes to determining the grounding line position. In PISM it is determined using a sub-grid interpolation scheme. Using this scheme, even at relatively coarse resolution, grounding line position is reasonably well represented (Feldmann et al., 2014).

One issue though, related to resolution is the "slippery grounding line" parameterization, which reduces the basal friction in the cells next to the grounding line. At a lower resolution the lubricated strip underneath the ice sheet would be more narrow, which would reduce the grounding line flux. In sensitivity experiment *nSG* (see supplementary document) the parameterization was turned off, which revealed the sensitivity of the result to the basal properties at the grounding line. At a higher resolution, we would thus expect reduced grounding line flux. We will discuss the sensitivity to the basal properties at the grounding line and will mention the assumed impact of resolution.

We further want to highlight that, since the few-cell-wide lakes are potentially the result of under-resolved topography, the focus of this model are the major lakes, which are well above the size of the grid resolution.

> *L88:* Basal boundary condition. The ice sheet sits on till, and slides when driving stress is greater than the yield stress of till. Is the substrate taken as being spatially uniform? Is there some sensitivity to this? Actually the design of this experiment is such that specifying a uniform yield stress is maybe of benefit (it is purely the addition of the LakeCC that is being studied), but perhaps the influence of LakeCC is very much dependent on the basal slipperiness, as this will dictate how far inland the "ice streams" can propagate. In which case some simple sensitivity expt as above would be very useful.

Yes, the basal properties are assumed to be spatially uniform in our experiments, which is not realistic but helps isolate the effect of the lake boundary. As also mentioned above, the results of our additional tests show a strong sensitivity on "slipperiness" of the ice base. It seems interesting to develop an improved basal conditions model, which parameterizes a simple form of sediment cover where lakes have been. This could potentially improve the formation of major ice lobes.

> > *L99 "To prevent the ice sheet from expanding into regions and high elevations where a more advanced approach would limit precipitation, the second model sets precipitation to zero above a threshold height or accordingly to a given mask (see Appendix D2)"*
>
> This seems like a dubious approach to me. If the ice sheet can grow sufficiently thick that precip cannot be parameterised, is there not something wrong with the ice sheet model configuration or climate forcing? What evidence do we have the the ice sheet elevation was less than 3500 m? This is a very low upper limit for an ice sheet of that size. On L363 you mention "the climate forcing tends to accumulate too much ice...".
> There are so many unknowns in the ice sheet model, could you not also say "the ice sheet tends to dissipate too little ice"? For example, why not just use a slightly more slippery bed (since this is so poorly constrained anyway) to avoid this problem of too much ice?

Ice sheet reconstructions of the Laurentide Ice Sheet (e.g. NAICE and ICE-6G) do not have much area that exceeds 3500 m, so we regard this as a crude way to ensure the ice sheet is not growing to an unrealisticly large size. We agreee that this is likely a problem related to how the climate forcing is parameterized, but this approach was necessary to ensure that the deformation of the Earth did not become so large that the area covered by the ice sheet doesn't become a large sea during deglaciation (and preventing us from testing the lake model). Using a different slippery bed condition might also allow us to avoid this issue, but that is not the approach we took in our idealized experiments.

Sect 2.2 LakeCC

> *L116:* Rapid filling causes the model to crash. As a modeller this is very worrying given that the maximum time step for the ice sheet model is only 0.25 yr (3 months). What is the reason for the numerical instability – e.g., is it in the ice sheet model or LakeCC? The implemented lake filling algorithm effectively dampens lake level changes, what is the time scale for this? Is the max filling/draining rate (gamma) really just 1 m/yr?
> This limit on drainage rate could place quite some restriction on the usefulness of the model in a real deglacial lake drainage setting. For example, if a lake drainage is initiated by overspilling the ice sheet (or by growing a subglacial channel), then in practice the very strong positive feedback could lead to rapid drainage by incision of a supraglacial channel or growth of a subglacial conduit. But how can the model cope with this? If the lake level is reduced artificially slowly, does that mean a lot more water drains out of the lake than was actually in it? And would a drainage channel/conduit become vastly over enlarged because the lake level (and thus the hydrostatic head driving the channel/conduit development) is held artificially high?

As elaborated in the response to reviewer #1, the stability issue is related to how PISM numerically solves the ice sheet flow. In our original tests, if the lake filled or drained too quickly, it would cause the model to crash. We have further run some sensitivity tests with higher rates (*FR5*, *FR10*, *FR50*, with 5, 10 and 50m/year, respectively). For details on these, see the supplementary document. Surprisingly, all runs finished without any problem and the results do not show any big difference to the *lcc* run. Experiment *FR50* already comes pretty close to an immediate response of the lake level to a change in the target level. We have to state that the appearance of the instability is strongly dependent on the configuration of the ice sheet when the model is evaluated. That is to say, for these new experiments, we might simply have been lucky that no critical situation was triggered and that slight changes in the ice sheet configuration might crash the numerical solver even at a lower fill rate.

If still a higher fill rate is desired but the model becomes instable, reduction of the time step would likely help.

Furthermore, we want to note that advanced processes, such as incision of drainage channels, are not included in the model. The model only adds an dynamical "wet" boundary to the margin of the ice sheet.

> > *L142 "If a basin disappears because it merged with the ocean, the lake level is gradually changed until sea level is reached, and then removed."*

> I don’t understand here how a lake can merge with the ocean unless its lake level already matches the sea level.

This kind of situation could happen if, for instance, the lake is formed purely from ice damming, but is otherwise the basin is below sea level. Once the ice margin retreats, the lake becomes part of the ocean. This happened with the final drainage of Lake Agassiz (Gautier et al., 2020), for instance. It transitioned into the Tyrrell Sea (the proto-Hudson Bay) after the ice saddle collapsed.

> *L165:* The general PISM marine boundary parameterisations are used. Were these not developed for saline water? I think modifying these for freshwater would be a fairly fundamental step in developing this new LakeCC component. In particular with ref to Line 237 (“Due to the difference in density between fresh and ocean water, the layer of melt water underneath the ice shelf experiences a ~200 times higher buoyancy in sea water...”) this does seem like it should be addressed here rather than a future study. What if there is a 200 times less sub-shelf melt beneath a lacustrine ice shelf? Will the development of floating ice shelves then be much less likely? What is the ambient water temperature in the lake – in a marine setting this would be crucial information.

We revisited the sub-shelf melting parameterization used in our experiments and tried to estimate values more suitable for the lacustrine setting. For details see sensitivity experiment *MR* in the supplementary document.

Concerning the lake temperatures:
The main problem is, that it is not possible to find values valid for all proglacial lakes. Also seasonal variability might change the properties of a lake greatly.

Even though we estimated a ~40x lower melt rate in a freshwater lake (water temperature of 2°C at a depth of 300m) the results did not significantly change compared to the *lcc* experiment.

> > *L176 “Ice temperature at the base is set to the pressure-melting point, which is a function of pressure, hence ice thickness.”*
>
> Presumably the PMP is also a function of salinity in your model?

The pressure at the ice base is only a function of the ice thickness (given constant ice density). Therefore, the pressure does not depend on the salinity of the ambient water. Salinity would only change the submerged height of the ice shelf.

The temperature at the ice base is different to the temperature used for calculating the melt rate. In this model the temperature difference for calculating the energy flux, is calculated between the mean ambient water temperature and the freezing point of water (which depends on pressure and salinity). For details see the model description in Beckmann & Goosse (2003).

> > *L266 “When using a glacial index derived from the NGRIP δ18O measurements (North Greenland Ice Core Project Members, 2007), the modeled deglaciation is too rapid to identify contributions from the lake model. Instead, the deglacial climate signal was crudely approximated by a linear model (see Fig. A1a).”*
>
> I agree that a linear transition is a better way of understanding how the new LakeCC model works, than using the noisy NGRIP d18O. Indeed the NGRIP record may well contain signals of the very lake drainages your model is trying to capture. But I worry from this statement (& comments above) that the model can’t cope with rapid changes in climate forcing, and this would be a significant limitation given its intended application to deglacial climates.

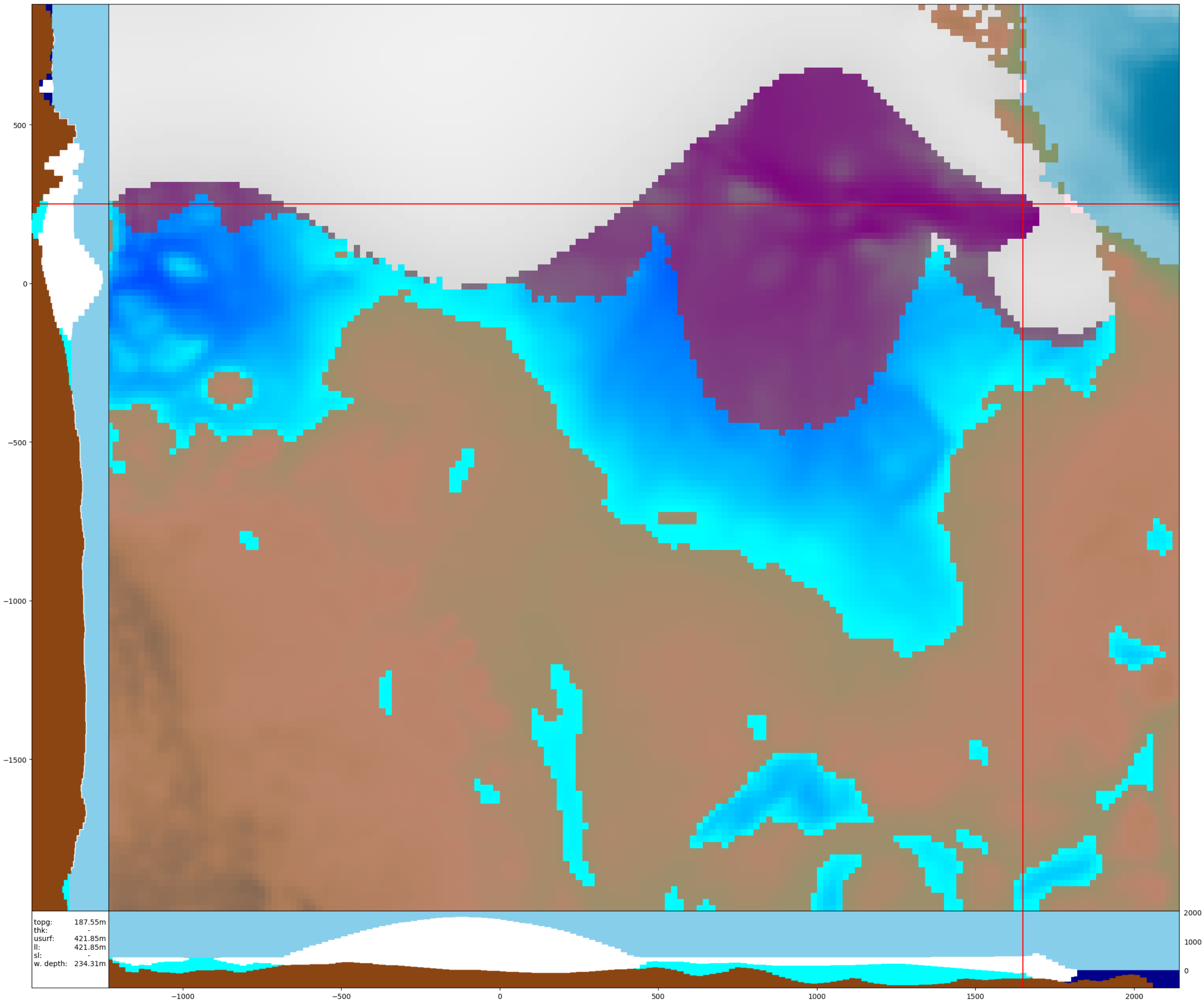
The sensitivity experiments discussed above (*FR..*) suggest that even higher fill rates are possible without causing problems. If this value should be much higher, this would have to come at the cost of smaller time steps, but it appears to be more stable than we initially thought.

> > *L284 “In the LNS experiment ΔhL was greatly increased to remove almost any floating ice, as large ice shelves like those seen in the LCC experiment are unlikely to have existed”*
>
> I get the point of this sensitivity run but is there actually paleo evidence to convincingly disprove the existence of extensive ice floating ice shelves?

We are not aware of any reference investigating the potential size of ice shelves on large proglacial lakes. Furthermore, we are not aware of geological evidence that would support the existence of such vast ice shelves. In general, geomorphologically constrained reconstructions of glacial lakes do not depict ice shelves that extend deeply into the ice sheet, as is simulated in our experiments (e.g. Veilette, 1994; Teller and Leverington, 2004; Lemmen et al., 1994).

> *L315, L358* Growth of the very large proglacial lakes (current Great Lakes region & then Agassiz). What are the lake levels and would they have been likely to drain subglacially, given their proximity to the ice margin and also considering that sea level was lower at that time? This subglacial drainage route would be one way of avoiding unreasonably large lakes – although I understand that adding that process isn't the point of the present study. Nevertheless, is surely worth a mention.

The lake levels of the major lakes shown in Fig. 8 are: a) 160m, b) 205m, c)435m, d) 421.85m (falling towards target level: 410m), e) 410m. Note the water level is relative to the PD geoid. We have not checked every drainage route for every lake, but the lake in Fig. 8d) clearly drains though Hudson Strait. Only a shallow plug of ice preventing the lake from draining into the Atlantic. The following figure is a plot of this time slice (17.5kyr). The panels left and right show transects along the horizontal and vertical red lines.



Map depicting the glacial configuration of the lcc experiment at 17.5kyr over Hudson Bay. The panels left and right show cross-sections along the red lines.

The sea level elevation does only have minor impact on the drainage route.

We will mention in the revised manuscript the spill points of the lakes in Fig. 8, and mark them on the maps.

> > *L345 "Without adding more advanced climatic feedbacks a realistic deglacial reconstruction is not expected. This, however, has not been the focus of this study, which is to test the PISM-LakeCC model and studying its impact on the ice dynamics and the glacial retreat. For these purposes the experimental setup is sufficient. Analyzing the interplay between ice sheets and proglacial lakes in more realistic setups, e.g. fully coupled to a climate model, and comparing against various geological proxy data is an interesting topic for future research"*
>
> To me this is one of the real positives of this paper – it focuses just on how the addition of lakes modifies the modelled retreat. The simple linear forcing greatly helps interpretation of the results, and similarly there is no long discussion of why the model inevitably doesn't fit geological reconstructions. This is also why I think the paper would really benefit from a little more testing of sensitivity. At the moment the paper is a model development study and in this case the sensitivity aspect is very important.

Thank you. As mentioned earlier, we have performed a number of additional sensitivity experiments.

> *Fig 3:* Last sentence of the caption – what is the continental LIS & should it really extend that far into the Atlantic?

To split the contributions from the different ice sheets, we defined regions via a mask, in which ice is attributed to the respective ice sheets. With the name "continental" LIS, we want to emphasize, that parts of the LIS (namely the parts on the Canadian Arctic Archipelago) were not taken into account. The shaded area in Fig. 3 depicts the region chosen focused on in the study. The selected region extends so far into the Atlantic, to also capture ice shelves that (potentially) extent far into the ocean. This does, however, not happen. In the Atlantic, it is usually ice free and thus does not contribute to the LIS.

> *Fig A1:* would be great to have this in the main text.

We will reconsider to add this plot into the main text.

> *Spelling/grammar:* Needs a proof read to iron out several minor errors.

We will do more proof reading to fix grammar and spelling mistakes.

References

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