Reviewer1.md

Author response to referee comment RC1 by Torsten Albrecht (PIK) 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.

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.

We would like to thank Torsten Albrecht 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).

Specific comments:

> drainage events
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> > *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?

The implementation of proglacial lakes as described in this work only affects the direct ice - lake interaction within PISM. Modeling the impact of redistributions of freshwater onto the climate system would require modeling and coupling Earth's different subsystems. For coupling the PISM-LakeCC model to an ocean model, translation of the water levels into freshwater fluxes would be necessary. This task, however, is not trivial, as the LakeCC model does not conserve water volume. Furthermore, water fluxes would have to be calculated by comparing the water distribution between two time slices. Any changes in water level or drainage route that happened in between those steps would not be resolved.

The sentence you are referring to from the introduction (l. 22) was added to highlight the further potential impact of proglacial lakes on the climate system. However, in many ways this model is a preliminary first step in implementing dynamically evolving lakes, and the first priority was to provide a stable model that can interact directly with the ice sheet.

One way to include data from the PISM-LakeCC model in a coupled Earth System model, could be by using the information of lake basins. Water fluxes from different sources (e.g. precipitation, ice sheet run-off) could then be accumulated within these basins and extracted from the water cycle. By redistributing the collected water volumes the changing topographic setting could be attributed for. Excess water can then again be added as a freshwater flux into the climate system. However, the complexity of accounting for all of the water sources is beyond the scope of this paper.

> lake merging
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> > *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, hmin, while the highest level, hmax, 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.”*

We have added the lake level in the legend of Fig. 1. Probably "current (local)" lake level was confusing, as we just meant the lake level. Therefore we have shortened this description in the new manuscript.

The quote from the Hinck et al. (2020) paper that you mention describes a different context. There we describe the LakeCC algorithm, which is used here to determine the `target_level` , while here we describe the gradual filling algorithm.

> 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.

Yes, this is a good point. We have added the following to the description of the ice sheet model:

"Marine regions of the ice sheet are defined in PISM via the flotation criterion, which describes whether the ice at given sea level and thickness is grounded or floating. The sea level is defined via a 2D map, which allows it to be spatially variable. In general, however, it is set to a global mean value prescribed by a scalar time series."

> > *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?

We have added a short paragraph about it to the Appendix:

"A lake, as seen by an ice sheet model, is very similar to a locally elevated sea level position. It would be possible to implement the lake interface as a sea level modifier in PISM. However, physical differences (e.g. density, temperature distributions) require different treatment of marine and lacustrine environments. To distinguish between both cases, at least two spatial maps are required. This can either be done by providing the lake and sea level combined in one field and additionally providing a mask, or providing lake and sea level elevations as separate maps. For the implementation of the PISM-LakeCC model, we chose the latter case."

For us it seemed more clean to have these things on different fields. Gradual filling should not have been a problem, as these secondary fields are used only internally of the lake model/modifier anyway. It was just a design question and we chose the other path.

> grounding line treatment
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> > *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).

Yes, this is an important point. We have conducted two more experiments targeting the sensitivity of the grounding line treatment (see experiments *TWO* and *nSG* in the appended document). These experiments confirm this high sensitivity.

In *TWO* the "till water ocean" model (PR425) was used instead of the slippery grounding line treatment. The ice sheet evolves in this scenario almost identical to the *lcc* run (which uses the slippery grounding line treatment).

In *nSG* the slippery grounding line treatment was deactivated. E.g. the till water of cells at the grounding line was not modified. This strongly impacts the glacial retreat, as the grounding line flux, and marine and lacustrine discharge are strongly reduced.

In all experiments the sub-grid grounding line treatment as activated by the `-pik` option are used. This also included includes the interpolation of basal melt.

We will address the issue of grounding line treatment (sensitivity of basal strength, but also sub-grid treatment) in the methods section.

> marine vs. lake boundary conditions
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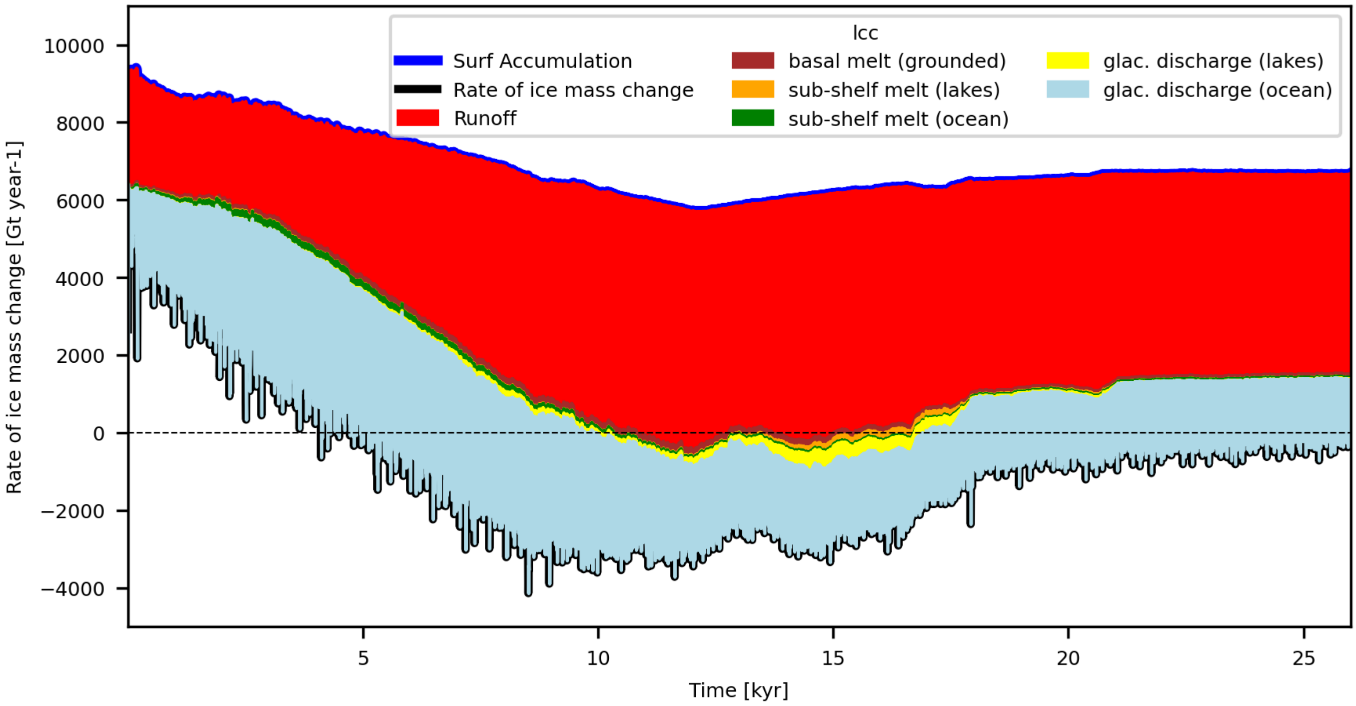
> > *l. 165: “Generally, the parameterizations provided by PISM for a marine boundary are applied analogously at the lake interface. This, however, may not be the optimal treatment in every case. The model might benefit from future implementations of advanced or more specialized lake boundary treatment. 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.

We have investigated the different contributions of mass loss for our experiments (see the figure for the LCC standard lake experiment).



Different contributions to mass loss for the lcc experiment. The top blue line shows the surface accumulation, while the lower black line shows the overall rate of change of the ice mass (The net surface mass balance is SMB = Accumulation - Runoff). The plot shows that the main process governing mass loss is surface runoff (= melt - refreeze) followed by glacial (marine) discharge (i.e. calving). Sub-shelf melting in lakes contributes only up to 6% to the total (non-surface) mass losses when large lacustrine ice shelves are present (15 kyr).

In the LCC experiment typical (depth dependent) sub-shelf melt rates in lakes are between 100 and 200 kg m^{-2} $year^{-1}$.

For the impact on the deglacial scenario please see the next point!

> > *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)?

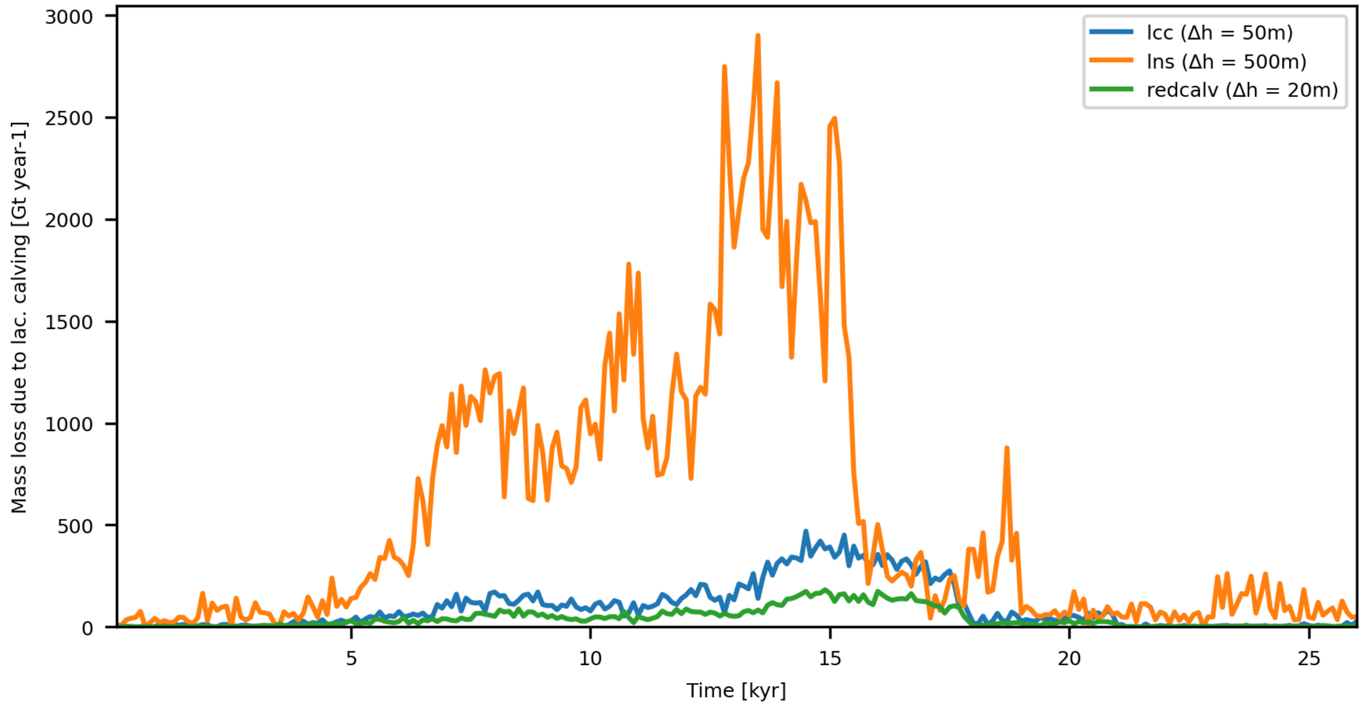
To address this point we conducted an experiment (*MR*) where we tried to estimate the relative difference between melt rates in marine and lacustrine settings using the melt pump parameterization. Details can be found in the appended document describing the additional experiments. Melt rates are strongly dependent on the assumed mean temperature of the lake. Furthermore, melting depends on the pressure and thus depth dependent freezing point. For fixed lake temperature and depth we estimated the effectiveness of melting in marine relative to lacustrine environments. Using T=2°C and d=300m marine melting is estimated to be about 40 times stronger. This factor is used to scale the melt rate tuning parameter accordingly.

The impact of sub-shelf melting on the ice sheet evolution becomes even smaller when applying this simple lake correction. Calving is still the dominant lacustrine term so that the results are very similar to the *lcc* scenario. The largest contribution on ice mass loss is from surface runoff, which is further increased due to the ice surface lowering upstream the lake boundary. This effect is also described in a recently published study by Quiquet et al. (2021). We will discuss this issue in the revised manuscript.

> 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)?

For the newly conducted experiments we checked the contribution of the Eigencalving to glacial discharge and realized that for our experiments it is always zero. Ice discharge at the ice margin is thus solely due to the thickness calving parameterization. To better compare the impact of the thickness calving threshold on the glacial retreat another experiment (*redcalv*) with reduced threshold (Δh=20m) was conducted.

The ice loss rises roughly linearly with the thickness calving threshold Δh:



Mass losses due to lacustrine calving for different experiments.

Comparing the results of the *lcc* ans *redcalv* experiments, the glacial retreat is almost unchanged. The main difference is that in the *redcalv* experiment the lacustrine ice shelf extent is a few cells (up to 100km) wider, which exhibits a larger ice surface area to surface melting. This approximately balances the difference in ice loss due to calving.

We will mention this issue in the revised manuscript.

> > *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 (ΔhL = 50m, l. 281) ice shelves are in fact unrealistic.

By large we mean shelves that extent several grid cells from the grounding line (e.g. 100's of km). 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).

Our recent sensitivity experiments suggest that the formation of ice shelves is sensitive to grounding line treatment. Whether or not the currently implemented conditions are realistic for proglacial lakes should be a target for future studies. We will mention this in the revised manuscript.

> 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?

Yes, we will say a few more words about this in the revised manuscript. However, unfortunately we are not sure about the exact reasons for this instability. Error messages indicating that the iterative KSP solution does not converge. We think that jumps in the boundary conditions (as they are obviously introduced when the water level is immediately changed) are just incompatible when numerically solving systems of differential equations. If the relatively slow gradual filling of lake basins is considered to be a major drawback of this model, higher fill rates could be realized in future implementations by requesting smaller time steps from PISM's adaptive time stepping mechanism.

> > *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?

Adaptive time stepping strongly depends on the model's state in PISM. In times when many ice shelves exist, ice flow is usually higher and therefore the adaptive time step is reduced accordingly (e.g. to ensure the CFL criterion). If the ice flows relatively slow, time step lengths can even be higher than one year. The main reasons for reducing the time step length is better comparability between different experiments:

- as we are comparing the results from different experiments we want the time step for all experiments to be as similar as possible. There is a known dependence of PISM model results dependent on when the model is evaluated.
- to estimate the efficiency of the LakeCC model (Fig. C1), we compare the efficiency of the different PISM runs. Time step length in PISM, and thus runtime of the model to finish, strongly depends on the model state (see previous point). To get the number of model evaluations for Lake and no-Lake experiments in a similar range, we manually reduced the time step.
- smaller time steps reduce the potential risk of the numerical instability of occurring (although by choosing an appropriate fill rate this might not be necessary)

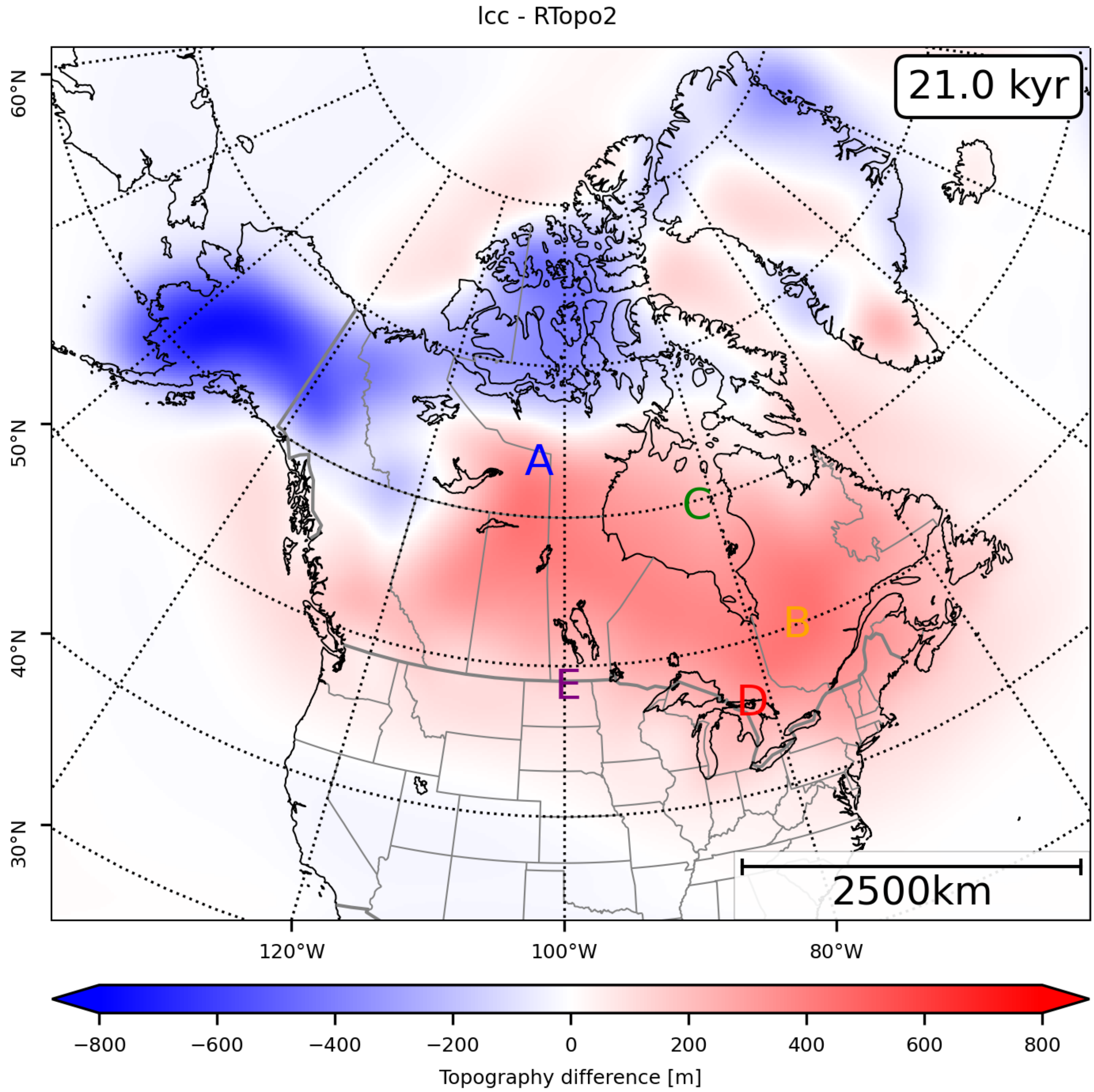
We will add a sentence about it to the revised manuscript.

> 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.

Yes, indeed, this would be an interesting future study. Although, implementing a closed water budget between ice sheet and lakes would require substantial changes of the Lake model.

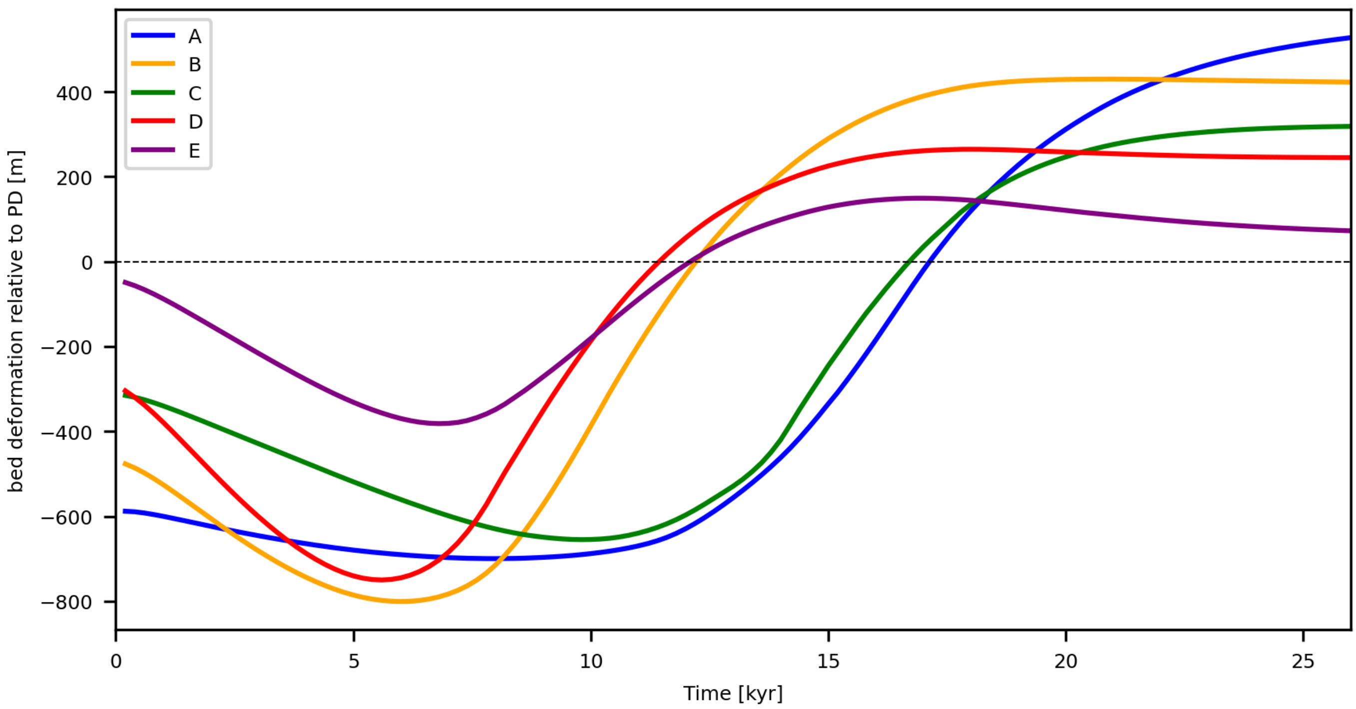
> > *l. 259: "The temporal evolution of the topography provided by NAICE can be used to calculate the uplift rates that are used to initialize the Lingle-Clark bed deformation model of PISM."*
>
> → The NAICE model makes use of a GIA model (which also solves the sea level equation) constrained with many 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?

We actually do use the LGM state and uplift rates from NAICE and used this for initialization of the LC model. The misfit between the topography state at 21kyr from the *lcc* experiment and the PD topography from RTopo2 is shown in the following plot.



Topography anomaly between 21kyr from the lcc experiment and present day RTopo2 data.

In the north-western part, where there is still ice in the *lcc* experiment, topography is still deeply depressed, while south-eastern Canada and the northern US are strongly over-relaxed. The letters mark the points for which the temporal evolution is plotted in the figure below.



Temporal evolution of the bed deformation relative to PD for different locations. The locations are marked by the colored letters in the above map.

> *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.

Our initial plan was also to start from a LGM state calculated from PISM. I.e. simulating the glacial inception from a PD-like geometry (as was done in Niu et al., 2019). However, it turned out that this modeled ice sheet was unrealistically too large and caused bed depression that was much greater than reality. During the rapid retreat the modeled bed response was too slow, so that along the southern ice margin the topography was below sea level and connected to the Atlantic ocean. The presence of this ocean basin inhibited the formation of lakes along the ice margin in this region, which is the main focus of this study. Therefore we decided to start our simulations from a more geologically constrained LGM state.

We added a short paragraph to the discussion section:
"The initialization of the Lingle-Clark model from a glaciated state is problematic here. For the model to calculate dynamically changing topography, to which bed deformation is applied, it should ideally be initialized from an interglacial state and then run through a full glacial cycles. Our test runs, comparable to the experiments by Niu et al. (2019), however, suffered from the fact that the bed deformation along the southern ice margin was large in magnitude. This created a basin was connected to the Atlantic Ocean, which consequently inhibited the formation of lakes. We therefore chose to initiate the experiments from NAICE LGM reconstructions. The mismatch in calculated relief topography, results in ice free regions to drastically over-relax. Hudson Bay, for example, is elevated above sea level at PD (see Fig. 8f)."

We further followed the suggestion and did an experiment (*dtopg*) in which we subtracted the PD misfit from the initial LGM topography. The results are shown in the supplementary document. In this experiment the final PD topography fits relatively well with the RTopo2 data, but we have similar problems as described above. During glacial retreat the large basin at the southern ice margin is connected to the ocean and thus inhibits the formation of lake basins. We therefore did not follow this approach any further.

> *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?

The reasons for the high lake depth at the grounding line are manifold. One reason is the strong bed depression, which is the reaction to the increase of ice mass due to the simple climate parameterization. Furthermore, comparison with sensitivity experiment *nSG* shows that the parameterization of basal friction at the grounding line impacts the grounding line flux. Less friction leads to dynamical thinning, which in return allows the glacial lake to further penetrate underneath the ice sheet, causing high water depths.

We have rewritten the paragraph in the revised manuscript:
"Another issue, that is also partly related to drainage, is the immense size and depth of some lakes that appear along the southern ice margin. From around 6 to 9 kyr one large lake occupies the entire Great Lakes region. Later, it transitions into the basin of Lake Agassiz/Ojibway (Teller and Leverington, 2004) before expanding into Hudson Bay (~ 13 – 18 kyr) (Fig. 8a-d). Maximum water depths close to the grounding-line are up to 1000 m. As a result of the increased accumulation of ice due to the simplified climate forcing (see above), the topography is further depressed. The deeply depressed topography in combination with lowered ice thickness, due to dynamic thinning (this is discussed in the next section), allows the lake to further expand underneath the ice sheet."

> > *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).

Yes, the formulation might sound too harsh. However, the simple two-layered Earth model neglects the large influence of the much higher viscosity lower mantle, which play an important role for determining bed response the central parts of the Laurentide Ice Sheet. The lack of a lower mantle contrast means that the depression will be overestimated, and the response to loading will be too fast. We have rephrased this paragraph and added the reference for our claim:
"Furthermore, for such deglacial scenario, use of a more advanced GIA model might be needed. PISM’s default model is based on a simple two-layered Earth model. Viscosity variations in the upper and lower Earth mantle are not included in this model, even though these variations are shown to be a dominant factor in the central Laurentide Ice Sheet (Wu, 2006)."

> > *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 4x10^20 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.

Using comparable Earth model parameters as used by NAICE (see experiment *GIA*) does not significantly change the results. Only the timing of the glacial retreat changes slightly compared to *lcc*.

> 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?

At high elevations very little precipitation is expected. In the simple glacial index climate model the decrease of precipitation with temperature, which decays with elevation based on a simple lapse rate approach, is calculated. However, with our input data this still resulted in accumulation of mass even at high elevations. This simple cut-off model is an easy workaround to reduce the ice sheet growth. The choice of the cutoff height (3500m) was rather ad-hoc. However, several LGM ice sheet reconstructions (e.g. NAICE, ICE-6G) indicate that most of the LIS was below this threshold. Only in few high mountainous areas the ice surface is slightly higher. Note, that PISM and we refer to surface elevations relative to PD sea level/geoid.

To clarify, the following paragraph was added to the revised manuscript:
"To prevent ice sheet growths in eastern Siberia and above 3500 m elevation (relative to PD sea level), ice accumulation is prevented by setting precipitation to zero in these regions. The maximum elevation threshold was chosen in a rather ad-hoc way. However, current LGM reconstructions (e.g. NAICE (Gowan et al., 2016), and ICE-6G (Peltier et al., 2015)) exhibit higher surface elevations only in mountainous regions."

> *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?

In principle yes. If the input fields are time dependent, they are assumed to be periodic over one year and each field is treated as piecewise linear in time. When accessing the temperature and precipitation fields for time t from the glacial index model, the respective LGM and PD fields are evaluated at t (one year periodicity) and used for interpolation. Therefore, the calculated climate fields do have a seasonal cycle.

> *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 also
> *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.

The growth of the ice sheet could also be related to a discrepancy between the modeled climate from equilibrium simulation using NAICE, and how a dynamic ice sheet reacts to the modeled climate, so it is not surprising that the ice sheet could grow after starting the simulation (e.g. the climate forcing might be too cold). However, as the goal of our experiments is simply to test how proglacial lakes affect the ice sheet, and we successfully simulate deglaciation, further investigation of the cause of this growth is not relevant to this study.

> *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.

In future studies a more appropriate climate forcing should be applied. For our model test case, however, this simple forcing is acceptable.

> 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.

We have added a paragraph to the beginning of the results section:
"The ice sheet volume is quantified in the following as sea level equivalent (SLE). This is rise of the water level, if the ice volume V_{IS} would melt and the equivalent fresh water volume V_{fw} is added into a basin of the mean ocean area $A_O = 3.625 \cdot 10^8$ km^2 :
$\Delta_{SLE} = V_{fw} / A_O = V_{IS} * (\rho_i/\rho_{fw}) / A_O$,
where $\rho_{i,fw}$ are the respective densities of ice and fresh water. Note, this formulation neglects any changes in the ocean surface area and the geoid due to mass redistribution. Furthermore, the ice sheet volume accounts for both, grounded and floating parts."

The caption of Figure 4 was adapted accordingly:
"[...] Ice volume is given in sea level equivalent (SLE) units; see main text for definition. [...]"

> 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).

The ice margin here is grounded. Thin floating ice tongues would immediately be calved off by the thickness calving parameterization.

Ice lobes (i.e. thin, broad glaciers that extend from the main core region of the ice sheet) are a common occurrence along the terrestrially terminating margin of the Laurentide Ice Sheet (Margold et al 2015). These lobes are grounded and are often associated with ice marginal lakes. So these features are not analogous to marine terminating glaciers.

In the revised manuscript we state that the ice margin is grounded here. This becomes also apparent from Fig. 5, which shows the grounding lines and ice margin. The discussion, why the ice margin advances into the lakes and why no floating ice is seen, fits best into the discussion section.

> 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?

Yes, this was also mentioned by the other reviewers. In this context it might be more correct to call it an increase of ice flow. We will change this in the revised manuscript.

> 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 Nfilter.”*
>
> → Why would this be an issue? Does it mean, that a drainage event can only occur for a channel opening of width larger than Nfilter?

This filtering scheme helps removing few-cell-wide lake basins, which often appear due to resolution issues in rough topography (i.e. river valleys that are under-resolved and incorrectly are filled as a lake). It is not a requirement to apply this method. As we are mainly interested in the interactions between ice sheet and the major lakes, we decided to remove them. Wet boundary grid cells potentially trigger faster ice flow, which require smaller time steps. Therefore, limiting the number of lake cells can be beneficial in terms of time step length.

The filtering method does not modify the underlying topography and thus does not impact the drainage routes of other lakes. It only labels and removes narrow basins in an extra step, after the lake mask was determined.

Technical corrections:

Thank you for all the suggestions and corrections. We will consider these in the revised manuscript.

> > *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** ."*

The target level is the maximum water level before a lake basin overflows into the ocean. Yes, this often also means that drainage is routed via other lake basins into the ocean, but this is not what is meant here. Overflowing into another lake basin can also mean that two lakes merge, but in that case the filling algorithm is not complete. We think that it is less confusing to just say that it is overflowing. Where the water is routed to is irrelevant here. We will change this in the revised manuscript.

> > *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”?

Yes, basically we determine the anomaly between the PD smoothed and "minimum - filtered" topography, which is then added as a correction. We will rephrase this.

> *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?

Yes, all experiment names will be in italics in the revised manuscript.

> > *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?

Since both ice geometries diverge (i.e. it is a ridge or dome on the peak of the ice sheet), and so do the flow patterns. We wanted to highlight that a local slowdown in surface velocity, as shown in the anomaly plot, can also be due to the changes in flow pattern. However, we feel that this statement is somehow redundant, and will be removed from the revised manuscript.

> *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?

We mean, non-marine-terminating ice streams. This will be rephrased in the revised manuscript.

> *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"

Ok, thanks for the hint. We might add it to the revised manuscript.

> > *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"?

By general, we mean that the lake or sea level interface provides these diagnostic variables, independent of the chosen modifier. In this sentence we wanted to refer to `lake_depth` and `lake_level_real` . We will reformulate this in the revised manuscript.

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

We might add such a figure to the revised manuscript.

> *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?

Yes, correct. However, if the margin is above sea level, it would not be part of the patch.

> *l. 551: "usees "*

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