Coverletter.md

Major revision of manuscript tc-2020-353 -- PISM-LakeCC: Implementing an adaptive proglacial lake boundary into an ice sheet model

by Sebastian Hinck, Evan J. Gowan, Xu Zhang, and Gerrit Lohmann

Dear Kerim Nisancioglu,

we are glad to submit our revised manuscript, based on the thoughtful comments and criticisms by the two remaining reviewers.

With this resubmission we provide

- the revised manuscript,
- a document highlighting all changes made on the manuscript, and
- our point-to-point responses to each reviewer (this document),

as required by the journal.

In the following, we will present our responses to all comments of the reviewers and provide, where applicable, the respective changes made in the revised manuscript. We hope our revised manuscript will further be considered for publication in *The Cryosphere*.

Yours sincerely,

Sebastian Hinck on behalf of all co-authors

Reviewer2.md

Author response to Report#2 by Tijn Berends to the revised version of 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.

> Review of Hinkc et al. 2021, revised version
>
> The authors present a set of simulations of the deglacial retreat of the Laurentide Ice Sheet (LIS), where they investigate the effect of proglacial lakes on this retreat. By considering the effect of proglacial lakes on grounding-line dynamics, sub-shelf melt, and calving, they show that the presence of these lakes significantly accelerates the LIS retreat. When no lakes are present in their model at all, a sizeable ice-sheet remains when their model reaches the present day, indicating that it is important to consider proglacial lakes when studying the dynamics of glacial cycles. Determining the processes behind the extremely rapid retreat of the LIS during several phases of the last deglaciation, such as the meltwater pulses, is becoming more and more important as the implications of ice-dynamical instabilities for projections of future sea-level rise are becoming more apparent. I therefore believe that studies such as this one could be very interesting, as they demonstrate that explanations for such rapid retreat can potentially be found without invoking some strong atmospheric forcing.

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

> However, I have a few concerns about both the methodology and the framing of the results, which I believe should be addressed before the manuscript can be published.

Before replying to all comments individually in detail, we want to make a general statement about the intent of our study.

In our study, we are showing the impact of our adaptive lake boundary condition on the ice dynamics, and thus on the glacial retreat. We show with our novel approach that the impacts of a lacustrine boundary condition on the glacial retreat is important and should be taken into account when studying the glacial retreat of land-terminating ice sheets. We are not trying to demonstrate a perfect reconstruction of the North American ice sheets. We are aware that for such an endeavor requires more advanced models for sub-components, and tuning of these would be necessary.

To test our model, we completed a side-by-side comparison of model runs that only differ in the absence or presence of a lake boundary. In this (preliminary) study, the focus with our experimental design was to simulate a glacial retreat scenario, in which lake basins could freely evolve along the retreating ice margin. As long as the model setup stays the same, it is sufficient to use a simplified model setup for other components.

In future studies, when aiming for more realistically reconstructions of the glacial retreat of ice sheets, more attention must be put into identifying and tuning the other feedbacks that are relevant to ice sheet evolution. These include, for example, the feedbacks between lakes and climate, more advanced ablation parameterizations at the ice-lake interface or more advanced GIA models.

In the following, we reply to all of your comments and highlight, where applicable, our changes made on the manuscript.

> Firstly, regarding the nature of the “proglacial lake ice-sheet instability” (PLISI). While the authors compare this to the better-known phenomenon of marine ice-sheet instability (MISI), they ascribe this instability to the elevation-temperature feedback (e.g. the first paragraph of section 5 Conclusions). However, MISI has nothing to do with mass balance processes, but is a purely ice-dynamical process, which is why all the different MISMIP experiments assume a uniform, unchanging, elevation-independent surface mass balance.

Yes, we agree. The original formulation was a bit misleading. We have rephrased the respective paragraphs in the Introduction, Discussion and Conclusion:

l.74 ff:

"In regions where there is ice-inward sloping topography, the grounding line retreats in a self-amplified manner, which corresponds to a rapid expansion of the lake. The PLISI, in combination with the increased runoff at the lowered ice surface in the warming climate, results in an accelerated retreat of the ice sheet. In our study, this mechanism leads to the demise of the LIS and finally to the drainage of the lake through Hudson Strait. In the control experiments, Hudson Bay is still glaciated when the present day conditions are achieved."

l.392 ff:

"The most drastic impact on the glacial retreat can be observed at a later stage, when the grounding line enters the deeply depressed basin of Hudson Bay (Fig. 8c - e). Since there is a reverse sloping bed, no stable grounding line position can be achieved (see Fig. 5), leading to an increase of ice flux into the expanding lake basin. This process is similar to the marine ice sheet instability (MISI, Weertman, 1974; Thomas and Bentley, 1978; Schoof, 2007). Contrary to the MISI, where the ice loss is generally driven by calving and sub-shelf melt processes at the ice shelf, we observe a dominance in ice loss via surface runoff over the latter processes. The strong increase of surface runoff is not surprising at the strongly lowered ice surface in the warming climate. However, we want to stress here that changes in local climate due to the presence of the lake might weaken this process (Krinner et al., 2004; Peyaud et al., 2007). This proglacial lake ice sheet instability (PLISI) described here, was also reported by Quiquet et al. (2021). Finally, the PLISI results in the disintegration of the ice saddle blocking drainage through Hudson Strait (Fig. 8d - e)."

l.425 ff:

"Once the lacustrine grounding line enters a deep basin on a retrograde bed, the ice sheet exhibits an instability similar to the MISI, which is called accordingly PLISI. As the ice thickness over the new grounding line position is higher, this increases the grounding line flux, which forces the grounding line to retreat even further. This positive feedback loop is ongoing until a stable grounding line position is reached. Due to increased melting at the lowered ice surface, the feedback cycle is further accelerated."

> In their rebuttal to my previous review, the authors show a timeseries of the different components of the total mass balance, showing that the retreat of the LIS is dominated by runoff and oceanic (not lacustrine) calving. This seems to be at odds with the findings of the different studies investigating MISI, and it also raises the question of how the presence of the lake can lead to such a strong lowering of the land-based ice dome, if the lake itself hardly removes any mass.

I am not sure here if I can follow you. As you said before, the MISI is a purely ice-dynamical process, which is caused by the unstable grounding line position. This increases the flow of ice over the grounding line. The MISI itself does not extract ice from the ice sheet. Ice from the shelf is then removed by different processes such as surface melt, calving or sub-shelf melt. I don't understand why it is so surprising that surface runoff is the dominating factor for ice loss in a transient experiment. Compared to other experiments that study MISI at Antarctica, air temperatures in our mid-latitude experiment are much higher and calving rates and sub-shelf melting are set to lower values in the lacustrine setting.

Furthermore, the figure that you refer to shows higher oceanic calving, because the LIS has longer marine than lacustrine ice margins, which have higher calving rates.

> Indeed, the authors report (in their rebuttal and in the supplementary material) that neither changing the sub-shelf melt rate nor the calving threshold thickness over the lakes significantly affects the results. If neither of these processes is significant, then what causes the difference in retreat rate between the lakes and the non-lakes simulations? The ice-dynamical processes governing MISI affect mass transport from the sheet to the shelf, but the shelf mass still has to go somewhere. If it is not removed by either sub-shelf melt or calving, then the shelf will grow thicker over time, the grounding-line will advance, and the basin will fill with grounded ice. The modelled grounding-line retreat must be caused by mass loss either on the shelf or on the sheet; the former does not happen, so the authors claim, but the latter should not be so different between the lake and no-lake experiment. This issue should be investigated further.

As described above, the shelf mass is mainly lost due to surface runoff.

> Regarding grounding-line retreat: in my previous review, I referred to the work of Natalya Gomez, who showed that gravitational effects can significantly reduce grounding-line retreat, and can even lead to stable configurations on (mildly) retrograde slopes even in the absence of buttressing. This was followed by a response from one of the authors, who claimed that the Lingle&Clark GIA model used by PISM is “self-gravitating”. After consulting with a colleague who specialises in GIA, I found that this is only partially true; the Green’s functions in the Lingle&Clark model include a self-gravitating term that is appropriate for a solid Earth that is in equilibrium with the surface load. The added ice mass on the surface is then balanced by the displaced mantle mass, so that the resulting gravitational perturbation is very small (deviating from zero because of the tensile strength of the Earth’s crust, so that the locally displaced mantle mass does not necessarily equal the local ice load). However, this assumption is not appropriate for a retreating ice sheet; as phrased in the original article by Lingle and Clark: “Additional changes in depth caused by perturbation of the gravitational potential field are not included.” The very existence of the vast proglacial lakes studied here is owed to the delayed rebound of the Earth’s surface. At such moments, the gravitational signal can be significant, and the effect on water depth at the grounding line, and therefore on grounding-line retreat, should not be neglected. Based on the different studies by Natalya Gomez, I expect that this could significantly reduce the accelerated retreat reported by the authors. While I acknowledge that it might be too much work to include an appropriate GIA model in PISM for this study, the drawbacks of not doing so should be discussed in the manuscript.

Studying the self-gravitational effect of ice sheets on proglacial lakes (and thus on the glacial retreat) is undoubtedly a topic that will need to be investigated in the future. However, we assume that its impact on the lake geometry is minor compared to other factors as ice margin location, GIA signal and topography. See i.e. James et al. (2000): "*Changes to a level surface are affected by gravitational potential changes, although the effect is much smaller than crustal displacement changes.*".

Gomez et. al (2015), for example study the impact of the Laurentide-Cordilleran ice saddle collapse on the sea level, and they provide an estimate of ~160m local SL change for the combined effect of elastic GIA and gravitational changes. Although the Lingle-Clark model does not perfectly capture the details of the GIA signal, it produces Earth deformation that is in the same range as this value, and therefore provides a suitable way to test our lake model. However, we can not estimate the magnitude of the gravitational effect on the tilting of proglacial lakes.

Adding gravitational effects of the ice sheets would, compared to our current lake reconstructions, lead to a tilt of the lake surface normal towards the ice sheet. This would lead to an increase in water depth at the ice margin, and not, as you propose, to a drop in water depth. The magnitude would depend on the ice mass and distribution, but also on the lake spillway location. The difference here to the studies of Gomez et al., where they investigate the impact of loss of parts of Antarctica on the SL, is that their initial state (e.g. present day) has the gravitational anomaly of the ice mass already included. Therefore, when studying the impact of ice loss on the SL, this fact leads to a relative drop in SL. For this reason, we would argue that adding this effect would accelerate the glacial retreat rather than dampen it.

Nevertheless, we think that the gravitational effect would be smaller than the uncertainties in our lake model, or the modeled ice sheet itself. The right place to add such feature would be the GIA model, that provides the topography to the LakeCC model. We therefore added one sentence to the description of the ice sheet model:

l.100 f:

"Self-gravitational effects of the ice sheet onto the sea level are not taken into account by this model. Compared to crustal deformation, this effect is only secondary (James et al., 2000)."

We further want to point to your last issue here. With regard to this point we have reframed our results. Our intention with this study is not to study the glacial retreat of the LIS, but study the impact of the LakeCC model on the ice sheet retreat, by comparing it to the no-lake experiments. Therefore, discussing every limitation of each of the ice sheet model's submodules, is out of scope of this study. We do, however, acknowledge that this issue could indeed be relevant for simulating more realistic glacial retreat.

> Then, regarding the experimental set-up. The authors explain that their model is initialised with ice thickness and bed topography from the NAICE model, and thermodynamics are spun up to achieve a stable englacial temperature. However, when the simulation starts, ice volume rapidly increases to ~50% more than the initial value in all experiments (both with and without lakes), which suggests that the surface mass balance parameterisation is not properly tuned. The resulting over-sized ice-sheet (exactly how over-sized is difficult to quantify, as the authors rather confusingly chose to exclude ice in the Cordillera and the Canadian Arctic from the volume calculation) causes an unrealistically deep GIA depression, which leads to modelled lakes that are probably significantly larger than they would have been in reality. This likely means that the accelerated retreat reported by the authors is overestimated.

This is correct, we also state this in the manuscript. However we added the following paragraph to the Discussion:

l.413 ff:

"We further want to note that the glacial retreat seen in our results might be strongly accelerated due to our simplified experiment setup. The large ice sheet growth, caused by the simple climate forcing, leads to deeply depressed topography and thus deeper lake basins."

> The authors mention a “problem” with the initialisation of the Lingle&Clark GIA model, which causes the Hudson Bay to become subaerial when the simulation reaches the present day. They ascribe this to the difficulty of differentiating in their code between the initial state and the equilibrium reference state. They also state that circumventing this problem by starting the simulation during the previous interglacial was not feasible, as this “suffered from the fact that the bed deformation along the southern ice margin was so deep, that the basin was connected to the Atlantic Ocean, which consequently inhibited the formation of lakes”. I find this unsatisfying; as with the “numerical instabilities” they report elsewhere (which they circumvent by creating a rather convoluted scheme of different lake water levels, masks, and filling rates), these kinds of coding problems should really be solved before using a model for research applications.

We want to clarify, that this problem is not a "coding" problem, that can fixed. It is rather a problem of the simple experimental setup. Ideally, a GIA model is initialized from an ice-free state, so that when the ice retreats again, the topography tends towards this initial topography. This, however did not result in a "LGM" state, from which we could start our experiments to test the lake model (Due to the excessively large ice sheet, the topography was unrealistically depressed). Therefore we chose an initial state from a prescribed LGM condition, which does allow for lakes to evolve, but that results in an unrealistic PD state. We agree that more effort need to be spend on this when aiming for a more realistic deglacial scenario. Since the goal of our study was more generally to look at the impacts of having a lacustrine boundary condition, this is out of the scope of the present study.

> Regarding the surface mass balance: in my previous review I referred to a few studies that showed how the presence of large proglacial lakes could positively affect the surface mass balance over the adjacent ice sheet, thereby potentially reducing retreat rates. The authors responded to this very briefly in their rebuttal, stating that including such SMB effects was beyond the scope of their study. However, they also claim that the accelerated retreat observed in their simulations is caused by surface mass balance processes (via the elevation-temperature feedback), which are triggered by the presence of the lake. I’d like to see some more discussion about why they think the latter process is so much stronger than the former.

We are not claiming, that the lakes' impact on local climate is generally negligible. In the Limitations section, we do acknowledge the lack of this feedback, stating that adding this feedback could potentially counteract the observed processes. In the revised manuscript, we have added a sentence about the potential stabilizing effect of the local climate effect of lakes on the ice sheet:

l.398 f:

"However, we want to stress here that changes in local climate due to the presence of the lake might weaken this process (Krinner et al., 2004; Peyaud et al., 2007)."

When aiming for a more realistic deglacial scenario, adding or further discussing this feature is needed. This will require a dedicated modelling setup, which is currently not implemented in PISM.

> Lastly, regarding the framing of the results: as I mentioned at the start, studies such as this one are important not only from a purely palaeoclimatological / palaeoglaciological perspective, but also for the way we think about near-future retreat of the Greenland and Antarctic ice sheets. The idea that ice-dynamical processes such as MISI, and more recently the ice-cliff instability caused by brittle fracture, can be as or even more important than atmospheric processes has only relatively recently become commonly accepted; the uncertainty in sea-level projections beyond 2100 is dominated by ice-dynamical terms, and a lot of effort is being dedicated to improving our understanding of these processes and reducing those uncertainties. Understanding the interplay between atmospheric and ice-dynamical processes in the geological past is an important part of this effort. I feel that the authors here could improve the readability of their manuscript by more clearly framing their study in this context; they could choose to present it as (A) a schematic study that investigates a particular process (e.g. PLISI), (B) a reconstruction of ice-sheet / lake / GIA evolution during the last deglaciation, or (C) a system-based study that looks at the role of lakes in the Earth system. Right now, I feel the manuscript does not really fall in any of these three categories, which makes it difficult to decide which drawbacks are acceptable and which are not. If the only aim is to quantify the ice-dynamical processes, then the lack of atmospheric processes is not problematic. If the authors want to go for a realistic reconstruction, then the choice of climate forcing is probably the largest source of errors. If they want to take a comprehensive approach to the Earth system, then the forcing, timing, and geometry are probably of lesser concern than the lack of atmospheric / geoid / GIA feedbacks. I suggest that the authors make a conscious choice about which direction they want to move in with this study, and frame the drawbacks and uncertainties of their findings accordingly.

As we have also written above, the intention of this study is to describe and test our lake model and demonstrate that proglacial lakes likely play a significant role in ice sheet retreat. It is neither *(B) a reconstruction of ice-sheet / lake / GIA evolution during the last deglaciation*, nor *(C) a system-based study that looks at the role of lakes in the Earth system*. But we think that it is also not *(A) a schematic study that investigates a particular process (e.g. PLISI)*. The PLISI is only one process being triggered by the presence of the lakes.

To be more clear, we highlight in several places that the glacial retreat setup is simplified and meant only as a testcase for the LakeCC model. E.g. in the Abstract:

l.7 ff:

"As a test scenario, a simplified glacial retreat setup of the Laurentide Ice Sheet (LIS) is used. By comparing the lake experiments with no-lake control runs, we show that the presence of proglacial lakes [...]"

As we have written before, we are testing the impact of our model on the ice dynamics, by side-by-side comparing the lake vs. no-lake experiments. For this purpose, the lack of more advanced feedbacks is not problematic.

In the previous manuscript we discussed problems of the glacial retreat that were due to the simple model configuration. Since these were not related to glacio-lacustrine interactions, we chose to change this: The "Experimental setup" section was renamed "Experiments" and all details about initialization and parameterization of the different submodels were moved into the Appendix. In the Experiments section, we refer to the respective section in the Appendix and briefly mention that realistic glacial retreat patterns can not be expected from our simplified model setup:

l.295 ff:

"Further details on the used parameterizations are given in Sect. A.
The experiments for this study are all based on the same simplified model setup. To properly simulate a realistic glacial retreat, more advanced models and setups would be needed. For example, the simple climate forcing using a glacial index leads to increased mass accumulation in cold regions and on top of the ice sheet. The experiments therefore suffer from excessive ice sheet growth after model initialization. Also, PISM's default GIA model is based on a simple two-layered Earth model and therefore lacks viscosity variations in the upper and lower Earth mantle. These variations significantly contribute to the GIA signal in central Canada (Wu, 2006). However, these shortcomings are the same for all of our experiments."

Furthermore, we removed the sub-section "Transient experiments" from the Discussion, because the differences to a realistic glacial retreat and their causes, are not relevant in this context.

Literature

- James, T. S., Clague, J. J., Wang, K. & Hutchinson, I. Postglacial rebound at the northern Cascadia subduction zone. Quaternary Science Reviews 19, 1527–1541 (2000).
- Gomez, N., Gregoire, L. J., Mitrovica, J. X. & Payne, A. J. Laurentide-Cordilleran Ice Sheet saddle collapse as a contribution to meltwater pulse 1A. Geophysical Research Letters 42, 3954–3962 (2015).

Reviewer3.md

Author response to Report#1 by Referee#3 to the revised version of 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.

> First I would like to thank the authors for their efforts in providing detailed and thoughtful responses to the reviews. From my point of view I still really like this study, and the questions I raised in the first review have been addressed - but with just one exception which I still believe is important.

We would like to thank the anonymous referee #3 for reviewing our paper. The original comments are indented, while our responses aligned to the left of the page.

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

> It is clear that the lake levels are determined by the elevation of spillways, either over land or over ice - as in the snapshots in Fig 8 where the changes in spillways are very helpfully highlighted. Although I agree that modelling subglacial drainage of proglacial lakes is far beyond the scope of this paper, neglecting this process does represent a potentially important limitation of LakeCC. I really urge the authors to clearly acknowledge this limitation, in both the discussion and abstract, particularly as the paper is in TC not GMD.
>
> Perhaps I could suggest two possibilities in support of my opinion.
>
> Firstly, subglacial drainage events (if they do occur) will prevent the lake from reaching the level/extent predicted by LakeCC. This is particularly likely once the ice saddle becomes narrow (see the 17.5 kyr snapshot). The lake level must of course be at least as high as the lowest bedrock saddle, or sea level, whichever is higher. However, the potential for triggering either PLISI or MISI could be considerably moderated if subglacial drainage prevents lakes from filling completely.
>
> Secondly, overspilling over an ice saddle with LakeCC is associated with rapid disintegration of the ice saddle due to the MISI (as reported in Section 4.3). Ice from the saddle is presumably lost completely by this process. Even if the MISI is initiated, the ice saddle can remain intact once rapid subglacial drainage triggered by dynamic thinning empties the lake before the MISI has completely removed the ice saddle. Once the lake has drained, remaining ice will close the subglacial drainage channels and allow the lake to refill. Amongst several obvious differences in simulated deglaciation under this scenario is the potential for cyclic drainage/filling events, or for a smaller but more persistent lake than that predicted by overspill alone.
>
> Adding some discussion of this limitation (and ideally a sentence in the abstract) would not detract from what is a useful and well-presented paper.

We agree that subglacial drainage is an important process that can regulate the evolution of proglacial lakes. However, mentioning it directly in the abstract is not appropriate in this context, we think. We rather highlight in the abstract the simplified assumptions made for water level calculation, which the lack of sub-glacial drainage is somewhat related to:

l.6 f:

"For simplicity, the PISM-LakeCC model assumes lake basins to always tend to be filled to the brim."

However, subglacial drainage could also be integrated into such model keeping the simplifications and assumptions unchanged, as it can be regarded simply as a lower spillway, that limits the maximum water level of a basin. Implementation of such process is not straightforward, as the dynamics is complex. We mention the lack of such a parameterization in the Limitations section:

l.226 ff:

"Another process which is missing in this model is sub-glacial drainage through channels. This process would potentially limit the maximum water level of basins dammed by a narrow ice saddle. However, parameterization of channel formation is not trivial."

> Two minor points... Line numbers refer to the version with track changes.
>
> > L205 "The model does not allow for refreezing at the ice shelf base".
>
> Why is this not the case? Also it's inconsistent with Fig 2, Process #2 "Sub-shelf melting/refreezing".

The way this melting/refreezing scheme was implemented into PISM, the model uses prescribed salinity and ambient water temperatures (35PSU & -1.7°C), as also mentioned in the manuscript. Using these fixed parameters the calculated (salinity and depth- dependent) freezing point of water is always below ~ -1.9°C. This is colder than the prescribed ambient water temperature and consequently refreezing is not permitted. We have rephrased this sentence more precisely:

l.185 ff:

"The model's choice of parameters is such that the temperature of the ambient water is always above the calculated freezing temperature and consequently the model does not allow for refreezing at the shelf base."

> > L302 "the numerical representation of a physical system requires the underlying equations to be smooth functions".
>
> If 'smooth' means differentiable then I'm not sure that restriction applies to all numerical representations of all physical systems. The difficulty with finding the reason for the numerical instability is clear and I suggest simply stating that the solver issues are associated with the appearance/disappearance of lake and ocean basins.

You were right, the original formulation was written too generally. We kept the sentence, but weakened its statement.

l.278 ff:

"From a numerical point of view, however, this is not surprising, as commonly the numerical representation of a physical system requires the underlying equations to be sufficiently smooth."

> Please check again for typos/grammar. For example:
> L18. Eurasian and North American continent. Change "continent" to "continents".
> L62. Recent work of Sutherland (2020) present... Change "present" to "presents".
> L113. The till below the water level next to the grounding line are... Change "are" to "is".
> L138. Details on how this lake boundary condition affects the ice dynamics is... Change "is" to "are".
> L266. ...implementation to model lubrication... Change "to" to "of".
> L333 "by setting" has been mistakenly deleted in "by setting precipitation to zero".

Thank you for the suggested edits. We have corrected the respective passages in the revised manuscript and also checked for further typos!