Reviewer2.md

Author response to referee comments RC2 & RC3 by Tijn Berends 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.

> The authors describe a set of simulations of the Laurentide Ice Sheet (LIS) during the last deglaciation, and the effect that proglacial lakes have on the evolution of the ice sheet. They used the well-established PISM ice-sheet model, including an additional module that dynamically tracks the extent and depth of proglacial lakes. By allowing ice shelves to form on these lakes, and by including some additional parameterisations for processes such as basal sliding, calving, and basal melt, they claim to have included all the ways a proglacial lake can affect the dynamics of the adjacent ice sheet. Their results show that the inclusion of these lakes in their model strongly accelerates the retreat of the LIS, in a manner similar to the phenomenon of Marine Ice-Sheet Instability (MISI). While the authors do not mention this in their manuscript, this is an important conclusion, as the asymmetry in the Pleistocene glacial cycles (slow inception vs. fast deglaciation, and particularly the melt-water pulses) is something that’s still not fully understood.
>
> I think a study like this could be very interesting, and could contribute to our understanding of glacial dynamics. However, there are several issues with the methodology which I believe impact the validity of the conclusions. In particular, two important feedback processes (the lake-climate-SMB feedback and, more importantly, the geoid-MISI stabilisation) are not included, both of which would reduce the accelerated retreat the authors observe. I will detail these concerns below, after which I’ll list the smaller technical questions I have.

We would like to thank Tijn Berends for thoughtful comments on our manuscript.

We would like to point out that we do not claim to "include all of the ways a proglacial can affect the dynamics of the adjacent ice sheet", and we explicitly state the shortcomings of our model (e.g. the lack of freshwater-ice interactions, the gradual filling and emptying of the lakes for model stability purposes, and the lack of a lake-specific calving law). We also would like to emphasize that the experiment we present is purely designed to test how lakes interact with the ice sheet during deglaciation. We acknowledge that in many ways it is not a realistic deglacial scenario, and it was never designed to be.

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).

> 1. The lake-climate-SMB feedback. At least two studies I know of, namely Krinner et al. (2004, Nature) and Peyaud et al. (2007, Climate of the Past), have looked at the effect proglacial lakes had on the local climate. Both find a net positive effect on the surface mass balance of the adjacent ice sheet, stabilising it against retreat. This would at least partially negate the acceleration due to grounding line dynamics described by the authors.

This is indeed a very interesting topic! We have neglected this issue so far, because the impact of the lake on the climate, only has an indirect impact on the ice sheet. However, even if this feedback is important, we consider it as beyond the scope of our study, since our experimental setup is not coupled to a climate model.

Adding such feature would require development and testing of a coupled model setup, which does not yet exist. Furthermore, regular coupling of the ice sheet model to a climate model would add significant computational overhead.

In Earth System models, and especially ice sheet models, there are lots of feedbacks that are not included or crudely approximated. Most previous studies have entirely neglected the impact of proglacial lakes (on the ice dynamics and on the climate). The purpose of our paper is to introduce a lake model that changes this, and provides a further improvement to the way we model ice sheets.

We use a simple index forcing method in order to test our lake model, and therefore there is no feedbacks between the lakes and the climate. As we have not included such a climate feedback in our study, we will add some words to the revised version.

> 2. The geoid-MISI stabilisation. The authors refer to Weertman 1974 for proof of the Marine Ice-Sheet Instability (MISI). However, Weertman’s proof that no stable equilibria exist for ice sheets whose margins lie on retrograde slopes did not account for GIA,

> nor for changes in the geoid. While the authors included a simple GIA module in their ice-sheet model, they did not account for changes in the geoid. Several different studies (the work of Natalya Gomez is probably the most important for the geoid, and that of Valentina Barletta for GIA) have shown that the fall in sea level at the ice margin, caused by the loss of ice mass in the interior, strongly reduces the retreat rate, and can even lead to stable equilibria on retrograde slopes. This has been proposed as an explanation for why the rapid West Antarctic retreat predicted by MISI is not really visible in paleo evidence. Since the authors claim (in my view correctly) that the strongly accelerated retreat of the LIS in their model is due to the same instability, I believe it is crucial to take this feedback into account.

Also this issue is very interesting! However, we are bound to the model implementations currently available in PISM. There are efforts to include a more realistic mode of GIA into PISM (e.g. computed by an external solid-Earth model, VILMA), but it is not yet publicly available. Coupling PISM to an external GIA model requires lots of work and testing.

We agree, that the results could benefit from a more advanced GIA model, but we are not sure about the relevance of gravitational effects at the lacustrine boundary. Since our current model does not include the gravitational effect of the ice sheet, we would assume the water depth to increase at the lake ice boundary, which would further accelerate the instability. Regardless, the Earth deformation will be by far the largest part of the GIA signal for the deglaciating Laurentide Ice Sheet, and this component is included in the Lingle-Clark model.

We will mention potential impact of the ice sheet's gravitational attraction on the water depth in the revised manuscript.

> The fact that these two processes, particularly the geoid effect, are not included, leads me to believe that this study significantly overestimates the lake-induced acceleration of LIS retreat.

Gravitational attraction would increase the pull of water towards the ice sheet. When the ice volume decreases, it would decrease that attraction, but the magnitude of this would undoubtly be smaller in magnitude than the residual rebound (which is included in the LC model), and geometry changes in the lake due to margin fluctuations. The ice volume changes that contribute to changes in gravitation are relatively slow compared to these factors. Therefore we do not believe that our model will "significantly" overestimate the lake-induced acceleration. Considering all of the other limitations of our current implementation, the lack of gravitational attraction is not likely to be largest source of error in our simulations.

> Aside from this, I also have a number of small, technical questions, which I will list here.

> > *L29: "...ice streams, which impact the mass balance..."*
>
> Don't you mean the ice dynamics? Mass balance is usually meant to include only surface and basal mass gain/loss.

The mass balance (not surface mass balance!) of an ice sheet is the net balance of mass accumulation and losses. Ice streams transport mass from the inner ice sheets towards the ice margins, where ice losses are highest. Therefore, we consider our statement as correct.

> > *L34: "...the 8.2ka event was caused by..."*
>
> Too confident. While there certainly is strong evidence for this, I wouldn't say the matter is entirely settled.

Yes, the formulation was too confident. We will reformulate this sentence and state that it is one hypotheses.

> > *L84: "The stress balance is modeled using a hybrid scheme based on the Shallow Ice (SIA) and Shallow Shelf Approximations 85 (SSA) of the full Stokes equations (Bueler and Brown, 2009)"*
>
> It is well known that these hybrid models perform poorly at simulating grounding line migration. Many models now include a semi-analytical solution for the grounding-line flux as a boundary condition, but as far as I know this has not yet been implemented in PISM, and it is not discussed anywhere in the manuscript. While I can't say if this would lead to an over- or an underestimation of ice-sheet retreat in this particular study, I think it is important to discuss this, since grounding line dynamics are the root cause of all your results.

In PISM the position of the grounding line is determined by applying the flotation criterion. The exact position is further refined using an sub-grid interpolation scheme. Using these schemes, even at relatively coarse resolution grounding line position is reasonably well represented (Feldmann et al., 2014). PISM does not include a model to prescribe the grounding line flux, and its implementation is beyond of the scope of this study.

Another related issue is the basal friction at the grounding line. In our standard setup we set the till in cells next to the grounding line as saturated, which decreases the overburden pressure and thus reduces the basal friction. In sensitivity tests without this parameterization (see experiment *nSG* in the supplementary document) we could confirm the sensitivity of the grounding line position as it was also reported by Golledge et al. (2015).

We will add a paragraph about the grounding line treatment in the revised manuscript.

> > *L88: "The basal resistance is determined using a model that assumes that the base of the ice sheet is underlain by deformable till."*
>
> Since you explicitly state that the effect of lakes on basal sliding is important, this seems an oversimplification. The distribution of regolith in North America is far from uniform, and the interplay between (erosion and transport of) regolith, basal sliding, and glacial dynamics has been studied for over two decades (e.g. Clark and Pollard, 1998). If you really want to present the effect of lakes on basal sliding as an important factor, then I believe a more elaborate approach is needed.

We are in the process of implementing a more elaborate basal conditions model in PISM that takes into account changes in sediment distribution and grain size (e.g. Gowan et al., 2019). While we agree that the differences in basal conditions could change the outcome of the simulations, we do not believe it would fundamentally alter our conclusion that proglacial lakes affect ice sheet dynamics. Once this new basal conditions model is complete, we will attempt this test.

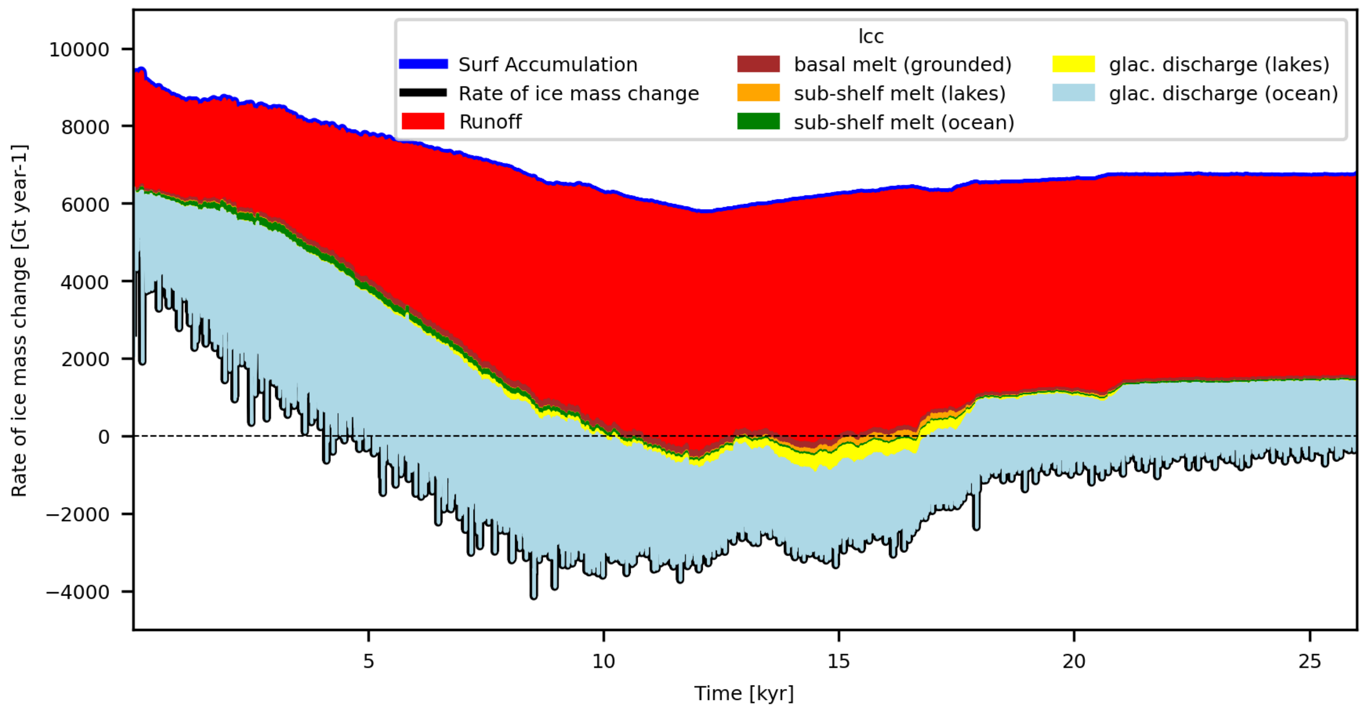
> > *L102: "The marine boundary treatment is described in Winkelmann et al. (2011) and Martin et al. (2011). It includes a sub-shelf-melting parametrization..."*
>
> If I'm not mistaken, this basal melt parameterisation was developed specifically for the Filcher-Ronne and Ross shelves in Antarctica. There, basal melt is mostly related to the intrusion of relatively warm deep water into the cavity between the ice shelves and the continental shelves, which leads to the depth-dependence in this parameterisation. I don't believe this translates well to the situation in Lake Agassiz. Since the authors show that calving plays in important role in their glacial dynamics, I suspect sub-shelf melt (which ultimately affects grounding line dynamics just as much as calving does) is equally important, and deserves a more accurate treatment than this. However, whether this oversimplification leads to an over- or underestimation of ice-sheet retreat, I cannot say.

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 (Beckmann and Goosse, 2003). 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. Compared to the *lcc* experiment, the impact of sub-shelf melting on the ice sheet evolution is minor when applying this simple correction for lake melt rates.

Since you mention the importance of calving on the ice sheet evolution, we shortly discuss the results of sensitivity run *redcalv*, with reduced lacustrine calving with Δh=20m. For this scenario we expected the glacial retreat to be slower that in the *lcc* run, but found that this changed thickness calving threshold hardly impacts the retreat pattern. Only in the extreme scenario (*lns* - Δh=500m) does lacustrine calving become the dominant process.

To get an overview about the different contributions to the ice sheet's mass balance we have prepared a plot (here for the *lcc* 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).

The experiments with reduced calving rate *redcalv* and adapted sub-shelf melt rate *MR* show that the driving process of the ice sheet instability is neither calving nor sub-shelf melting. The driving process is rather surface melting due to the surface elevation feedback. The same was recently also found by Quiquet et al. (2021). We will discuss this issue in the revised manuscript.

> > *L111: "Depending on the complexity of the lake model, computational overhead can drastically increase."*
>
> I wonder if you considered using the flood-fill algorithm which I specifically developed for this kind of application (Berends and van de Wal, 2016). I've been using this for a while now, included in a 40km resolution ice-sheet model (solving for lakes at a 1 km resolution), and running full glacial cycle simulations is no problem at all (~60h computation time, including the SELEN sea-level model).

Yes, we actually took your algorithm into consideration when developing our lake module. However, if I understood correctly, each lake requires a seed element, which we wanted to omit in our algorithm, since through a full glacial cycle, the locations that might need a seed could change. Our lake filling algorithm takes only a trivial amount of computation time, and provides a reasonable lake configuration even at lower resolutions (Hinck et al 2020). We found the main increase in computation was due to lakes causing the ice velocity to increase, necessitating the increase use of the SSA model.

> > *L117: "However, rapid changes in the boundary conditions resulting from this approach often cause numerical instabilities which cause the model to crash."*
>
> What kind of numerical instabilities are these? I've never encountered this problem myself. It sounds like something that should be addressed in the numerical solver of the ice-sheet model itself, rather than by compromising on the lake-filling code. Regarding this compromise: exactly how fast do you move the lake level to the "target level", and how does this impact your results?

I have to admit, that I am not an expert on the numerical solver of PISM (it is based on PetSc). The model crashed with a message that the stress balance solver failed (KSPSolve). This problem is, however, not unique to using the LakeCC model, but also appear under other circumstances (see https://pism-docs.org/wiki/doku.php?id=kspdiverged). In our case, we could narrow the cause down to either abruptly forming or draining lake or ocean basins. This causes a rapid change in ice geometry and stress boundary condition. It is not surprising that a numerical model, which is based on the assumption of steadily evolving physical system, can not cope with such sudden jumps at the boundary. In my opinion this issue can not be solved within the numerical solver , but could be tackled by accordingly reducing the numerical time step to maximize the speed in which the water level is gradually adapted. The default value, which was used in the experiments is 1m year^-1 (this parameter choice was rather ad-hoc). We conducted some further test runs (*FR5*, *FR10* and *FR50*, see supplementary document), where this value was increased to 5, 10 and 50 m year^-1, respectively. Surprisingly, all runs finished without any problem and the results do not show any major differences 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 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.

We do not see this gradual adaption of the water level as a big problem, since uncertainties in water level, ice geometry and missing closed hydrological cycle are at least of the same order. Consequently, lake reconstruction from this model can not be used for direct use in a coupled Earth model. This, however, has never been the intention of our modeling approach. We only focus attention to direct ice-lake interactions, which were missing in most ice sheet modeling studies.

We will elaborate a bit more on the cause of the instability in the revised manuscript.

> > *L155: "Therefore, the use of a more advanced sea level model is necessary. This sea level model implemented here."*
>
> This is not a sea level model. Sea level is, in your setup, prescribed externally with the glacial index method. What you're describing here is a routine that determines the ocean mask.

This is correct. We were rather referring to the naming of this sub-system within PISM. We will clarify what exactly we mean by sea level model in the manuscript.

> > *L202: "For hydrological applications, such as lake basin reconstructions, the resolution an ice sheet model usually operates on is too coarse to resolve spillways through the terrain. Even more important than data resolution is the ice margin position and bed deformation due to GIA (Hinck et al., 2020). Considering the uncertainties of these fields retrieved from an ice sheet model, the resolution issue is regarded as a secondary issue."*
>
> I'm not sure I agree here. Determining lake extent in a low-resolution DEM leads to a systematic overestimation of lake water volume (since you'll always underestimate the depth of drainage channels), which Berends and van de Wal (2016) showed to be around 10% for a resolution of 20km (this is why I developed my own algorithm!). This might not be much, and I don't it's something that should be fixed right away, but since it's an overestimation that's there throughout the simulations, it's something to keep in mind when you start looking at sea-level jumps and the likes.

This is indeed a valid point, we will add a sentence acknowledging the systematic overestimation of lake volume to the revised manuscript. However, given the large uncertainties in the modeled glacial topography, we don not see this as the main problem. Focus of this study is not to perfectly model the water volumes, but to determine potential positions where proglacial lakes might have existed and impact the ice dynamics.

> > *L219: "Sudden jumps in water level can trigger numerical instabilities in the ice sheet model. To avoid such jumps, the water level 220 is gradually adjusted with a constant rate."*
>
> Again, what do you mean by this? And why are you so sure that it is the sudden jumps in water level (which, if you look at sea-level records of the 8.2 kyr event, are definitely hinted at) that are unrealistic, rather than the behaviour of your numerical solver?

The basic LakeCC model (as it was described in Hinck et al.(2020)) does calculate potential lake basins for a given ice and topography setting. This means, changes in these fields cause the sudden appearance or disappearance of lake basins. If these changes happen at ice covered grid cells, the ice sheet at these spots could suddenly become afloat or ground. This marks sudden and extreme changes for the stress boundary condition. The sea level record you are referring to does show rapid jumps, but these are never 10's of meters in one time step. We are not saying that sudden jumps are unrealistic, but even in reality they are steady and not immediate. Even the final drainage of Lake Agassiz at the 8.2 ka event likely happened over the course of centuries (Gautier et al 2020), so gradual filling and emptying might not be so unrealistic in reality. There might be better ways to handle this, but all would certainly include gradually adapting the water level. For more details see our previous replies.

> > *L234: "The model relates the mass flux to..."*
>
> Could say anything about water temperatures in the lake? Krinner et al. (2004) find bottom water temperatures < 4∘C in a proglacial lake that's frozen over 7 – 11 months per year. How does this compare to your parameterisation?

We will refer here to our sensitivity experiment *MR* (see supplementary document), where we tried to find parameters for this model more suitable for lacustrine settings. We will also mention the the lack of our model to include seasonal ice cover. This could for example be added by using the ice mélange parameterization of PISM. This would, however, require parameterization of the lake temperatures.

> > *L249: "Another important issue that is ignored in our model is the effect of ice mélange..."*
>
> It was my understanding that ice mélange buttressing is only relevant in fjords and such, where the convex coastlines can provide a backpressure to the mélange, which in turns pushes against the shelf front. Do you think this plays a significant role on the open water of Lake Agassiz?

We agree that buttressing might not be relevant for large open lakes as Lake Agassiz. But it might be a missing effect to reduce mass flux into smaller lakes. As also mentioned in the previous reply, this parameterization could also be used to mimic the presence of a seasonal ice cover.

> > *Fig. 3: "The shaded area shows the regions attributed to the (continental) Laurentide Ice Sheet (LIS) in this study."*
>
> I don't understand what you mean by this.

For discussing the temporal evolution of a spatially bound ice sheet, we have to define a region within which ice masses are attributed to it. By "continental" we emphasize that we have excluded the parts of the LIS in the Canadian Arctic Archipelago from our discussion. We will check how this can be stated more clearly.

> > *L269: "To prevent ice sheet growths in eastern Siberia and above 3500 m elevation, ice accumulation is prevented by setting precipitation to zero in these regions."*
>
> This seems rather ad-hoc. Given that (at least in the ICE5G and ICE5G reconstructions) large parts of the LIS interior are above 3500m, how do you think this affects your results?

We want to state that we are not talking about ice thickness, but rather surface elevation relative to the present day geoid, here. Ice sheet reconstructions Ice6G and NAICE both exhibit LGM surface elevations above 3500m only in few high mountainous grid cells.

> > *L272: "Transient sea level forcing is applied accordingly to the glacial index."*
>
> This seems like an oversimplification, given that your main conclusion is rooted in grounding line dynamics. Even without using a geoid model, you could at least let eustatic sea level be calculated dynamically.

Our simulation is only meant as an idealized experiment. We have not included the other ice sheets, so calculating sea level change in this way is not possible. Regardless, the configuration of the lakes in continental North America is not dependent on sea level (since the lakes form above sea level), and therefore prescribing sea level in this way does not affect our results. As mentioned by Reviewer #1, coupling PISM with a proper GIA model is underway, so such an experimental setup will be available in the future. At that point, we can test this.

> > *L274: "Before running the experiments, the model needs to be spun-up."*

> How do you initialise englacial temperature? And why do you want the ice sheet to be in equilibrium with the prescribed climate before you start your simulations? Just as the real climate is never in a "steady state", so the real ice sheet would never have been in equilibrium with the climate, but always lagging behind it. It seems more logical to avoid these questions by starting your simulation in the Eemian interglacial.

Yes, we agree that the ice sheet can not be expected to have been in equilibrium with the climate at LGM. Our spin-up does only aim to thermo-dynamically equilibrate the ice sheet. Therefore, PISM is run in a fixed geometry state until the internal enthalpy field are in better agreement with the energy fluxes prescribed by the boundary conditions.

Initially we planned to start from an interglacial state and run the full cycle (similarly as it was done in Niu et al. (2019)), but the resulting LGM ice sheet and bed deformation were too large. The post-glacial topography was depressed so deeply that large parts of Canada were below sea level and connected to the Atlantic Ocean, so that lakes could not form. Since studying the impact of proglacial lakes on the ice sheet dynamics in the main focus of this study, this setup was inappropriate, and we decided to initialize the experiments from a more geologically constrained LGM state.

> > *L282: "...in order to reflect the fact that calving rates for freshwater terminating glaciers are reported to be an order of magnitude lower than rates observed for tidewater glaciers..."*
>
> I thought part of the reason why tidewater glaciers experience more calving is because of the tides after which they're named, which cause increased crevassing, as well as more wave action and other goings-on that weaken the ice. Do you think Lake Agassiz is more similar to the ocean, or to a small mountain lake, in that regard?

I guess a large lake as Lake Agassiz can neither directly be compared to the ocean nor to a small mountain lake. I would expect higher waves than in a small lake, but smaller waves compared to the ocean. Tides will not play a major role in an enclosed water body (e.g. the tides in the modern Great Lakes are less than 5 cm https://oceanservice.noaa.gov/facts/gltides.html).

> > *L285: "large ice shelves like those seen in the LCC experiment are unlikely to have existed"*
>
> Why not? Do you cite any studies that support the existence of an open lake? Has IRD not been found in sediment cores, or has it never been looked for? Ice shelves are hard to track in proxy evidence, so I wouldn't be too quick to dismiss them.

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 and calving condition. 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.

> > *L296: "Within this time, the LIS almost doubles its volume..."*
>
> This sounds rather problematic. Why are your initial ice sheet and prescribed climate so far out of equilibrium? Aren't there any tuning parameters in your PDD scheme to correct for this? Also, looking at Fig. 4, your initial state has a volume of about 40 m SLE, which seems rather small for the Laurentide – I believe 60 – 80 m is a more commonly accepted number. (e.g. ICE5G, ICE6G).

Our initial ice sheet is taken from the NAICE reconstruction. It is not in equilibrium state calculated by PISM, we therefore expect a model drift after initialization. Directly after initialization, the climate is shifted from the cold and dry LGM state towards a warm and humid PD state. On top of the ice sheet temperatures are still cold due to the temperature-elevation drop, but precipitation increases as the PD climate becomes more dominant. This leads to a strong increase of ice mass.

The NAICE LGM reconstruction features less ice volume in the LIS, compared to the IceXG models. The advantage of using NAICE is that it includes some (minimal) ice sheet physics in its construction, and therefore works as a more realistic starting point for an ice sheet model than the ICE-xG models. Furthermore, we did not take into account the entire LIS, as it was stated in the text. By LIS we denote only the "continental" LIS (which does to take the Canadian Arctic Archipelago into account). See also the reply above.

> > *Fig. 4:*

> Where is the spin-up phase? When does the forced warming in the glacial index method start? Are lakes already included during the spin-up? And how does your "simulation time" correspond to real world time?

As already stated in a previous reply and in the manuscript, the spin-up phase only equilibrates the thermodynamic fields of the ice sheet model. The model is run in a fixed geometry state, with boundary conditions fixed at LGM values.

I am not sure if I understand your question about the simulation time. Simulation time starts at year 0, which corresponds to the LGM state (~21ka BP). Simulation year 21000 would correspond to a present day state (i.e. the climate conditions according to the glacial index matches the pre-industrial state). However, if you compare the ice sheet states to a geologically constrained glacial history, the timing does not match. The idealized experiments we present are meant to simulate a deglaciation, and it is not meant to represent the specific last deglaciation.

> > *L314: "At around 9 kyr the water level of this lake rapidly dropped, as a lower outlet to the Atlantic became ice-free."*
>
> Which outlet? I'd like to see a map showing the locations of the possible spillover (Mississipi, St. Lawrence River, MacKenzie River) and drainage (Hudson Strait, Lancaster Sound, North-West Passage) routes in relation to your ice-sheet geometry.

We have not checked the locations of spillover yet. For the major lakes shown in Fig. 8 we will determine these points and mark them in the revised manuscript.

> > *L321: "At around 17.9 kyr, the ice saddle over Hudson Strait breaks apart and allows the lake to drain into the Labrador Sea"*
>
> The deepest part of Hudson Strait is quite narrow, so a 20km DEM might significantly underestimate the water depth, and therefore the retreat rate. How do you think this affects your results?

The retreat of the Hudson Strait does not happen until near the end of the simulation in our idealized experiment. In reality, the Hudson Strait deglaciation happens much earlier, and the final ice dam is located in the southern part of Hudson Bay (Gautier et al., 2020). It might be possible that the depth is underestimated and preventing a more realistic retreat of the Hudson Strait. An earlier retreat of the Hudson Strait would cause an earlier drainage of the lakes south of the ice sheet. This might allow for the preservation of some of the ice that is located in the Labrador sector of the ice sheet compared to our simulation, but Hudson Bay would likely still become completely ice free due to ocean interaction.

> > *L322: "Due to GIA processes, the ice-free Hudson bay basin eventually rises above sea level"*
>
> Is this realistic? I've never seen this happen in my own model runs (which include an actual geoid model), for me sea-level rise always outpaces isostatic rebound, but I don't know what the field data indicates.

No, this is not realistic. We believe that it is related to the initialization of the Lingle Clark GIA model of PISM. As already discussed before, we were not able to initialize the experiment from an interglacial state and run it into the glacial. Starting from a glaciarized continent, the model overestimates the relief topography because the initial LGM state was calculated using a different GIA model.

We added a short paragraph to the discussion section:
"Also the initialization of the Lingle-Clark model from a glaciated state is problematic here. For the model to calculate a relief topography, to which bed deformation is applied, it should ideally be initialized from an interglacial state and then run into a glacial. Test runs, comparable to Niu et al. (2019), however, 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. 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)."

> > *L326: "At around 21 kyr, the saddle collapses, which drains most of the lake."*
>
> Again, how does 21 kyr simulation time correspond to real-world time? Does this match the 8.2 kyr event?

The timing would match with PD (21kyr after LGM). However, as we also write, the timing of the glacial retreat does not match with geologically inferred retreat pattern. Our idealized model setup is too simple to create a realistic retreat. Geologically constrained ice margin reconstructions (e.g. NAICE - Gowan et al., 2016) indicate separation of the Cordilarian and Laurentide ice sheets by ~14ka BP (which would be around 7kyr in model time).

> *Fig. 5:*
>
> why does the ice margin in the LCC simulation make a∼500 km “jump” at∼ 18 kyr?

The jump that can be seen in Fig. 5 happens between 17.4 and 17.5 kyr and is because the ice shelf section shown in Fig. 5 is close to the western margin of the ice shelf. For this time the displayed cross-section (northwards, along 90°W) is disadvantageous, because the predominant retreat direction of the ice margin here is eastwards. This can be seen in Fig. 8d.

Even though this "jump" does not directly display the rapid disintegration of the large ice shelf, the breakdown happens within ~500 yrs (compare with Fig. 8d-e). At 17.5 kyr the drainage through Hudson Strait is only blocked by last rest of grounded ice. As the ice becomes thinner, the high water level lifts this ice barrier and drains into the Atlantic - the lake level drops, until the ice barrier is grounded again. The rapid drop in water level caused the grounding line advances again a bit southwards (see Fig. 5) and the mass flux into the shelf drastically drops, which stops balancing the mass loss.

We will add a paragraph to the revised manuscript.

> *L336: “By linearly interpolating between the warm, humid PD and cold, dry LGM climate states, unrealistically high accumulation is produced”*
>
> There is no climate feedback in your model, your entire climate is prescribed through the glacial index. With a glacial index of 1 at t=0, the prescribed climate should be exactly that of the GCM that produced it. What you mean is that your initial ice sheet is simply not in equilibrium with this steady-state climate. This is why paleo-ice-sheet models generally need some form of tuning in their SMB parameterisations, and also why it’s usually better to start a simulation in an interglacial (e.g. the Eemian) and run forwards from there (since you would not expect the LGM ice sheet to be in equilibrium with the LGM climate in any case).

I would call the lapse rate corrections of the temperature and precipitation fields a feedback. The other criticisms were already addressed above.

> *Fig. 9:*
>
> What does the thick blue line in panel d) signify?

We have added the following description to the figure caption:

"The blue line in panel (d) marks the lake level elevation in the *LCC* experiment, while the gray line marks the sea level elevation of the *Def* experiment."

> *L354: “For this reason, the lake reconstructions are not expected to match well with observations”*
>
> This is a bit of a chicken-and-the-egg question; are your lakes wrong because your ice margins are wrong, or are your ice margins wrong because your lakes are wrong? Since the entire point of your paper is to show that the presence of the lakes affects the ice sheet (and the therefore the ice margin), you cannot simply ignore the feedback here.

To discuss the impact of the lake model we compare the different experimental setups ("lakes vs. no lakes"). From the no lake scenarios, we do not see a better match with geological reconstructions. We do, however, see that adding the simple lake boundary condition does have a massive impact on the resulting ice sheet configuration (i.e. Hudson Bay becomes ice free when lakes are included).

This is an idealized experiment, and obviously the margins would not be able to be simulated exactly the same as the geological observations. For that, we would need to implement the lakes in a coupled climate model (as an example), which is beyond the scope of this study. However, our idealized experiments do demonstrate that the presence of lakes will affect ice sheet dynamics and played a role in the deglaciation of the Laurentide Ice Sheet.

> *L355: “Drainage towards the Arctic, for example, is blocked until the ice saddle connecting the LIS and the CIS collapses”*
>
> This raises the question of, where would the real paleo-lakes have routed their spillover, and is that pathway indeed blocked by your modelled ice-sheet? If not, then this might be a resolution issue as I mentioned earlier.

Geological records show that the Cordilarean and Larentide ice sheets separated quite early, as noted already earlier. Since in our experiments the ice barrier collapses quite late in the simulation, drainage, which in reality was towards the Arctic, is thus not possible in our experiment. It is possible that this is related to the resolution, but more likely this an issue with the climate forcing, which has an obvious cold bias in the Arctic (as the ice sheet never retreats there). In our LakeCC standalone paper (Hinck et al., 2020) we discuss different lake stages and compare these to geologically inferred data.

> > *L362: "This simple, two layered Earth model is not capable of handling the extreme deglaciation scenario of an entire continent"*
>
> This is unsatisfactory. In the introduction section, you (correctly) state that the GIA depressions are the reason those lakes exist in the first place. If your GIA model isn't performing well, then this should be fixed.

For further studies aiming to reproduce the deglaciation this might be necessary. Here we studied the impact of the presence of lakes on glacial retreat. A perfect model setup is not required to demonstrate this.

> > *L373: "We assume that this is due to the enhanced sliding at the lake boundary"*
>
> This assumption can, and should, be easily verified, by turning off the "till saturation at next-to-lake ice pixels" parameterisation you described earlier.

We have run a sensitivity experiment (*nSG* - see supplementary document) where we turned off the "slippery grounding line" parameterization. In this experiment no such ice lobes are observed. We rephrased the sentence in the revised manuscript:

"Due to the enhanced sliding at the lake boundary an advance is locally promoted."

> > *L375: "When water depth of the proglacial lake becomes too deep, the thin advancing ice front is presumably lost due to calving."*
>
> Presumably? Again, this seems like something that could (and should) be easily checked.

All lakes which border to the ice sheet have a calving rate greater zero.

Using the thickness threshold calving model, the thin ice front of an advancing ice shelf is calved off when it is below the threshold thickness. Only if the ice thickness is is either above this value or above the flotation thickness at a given water depth (i.e. it is grounded), is the ice not removed. The lakes shown in Fig. 6 all have water depths below 40m, and ~20m at the grounding line. The advancing ice therefore needs to be greater than ~22m to be grounded. If the lake is deeper, the ice would need to be even thicker, which we do not observe in our experiments.

> > *L390: "Lakes, however, do impact the early retreat by inducing the formation of ice streams, which drain the ice sheet interior"*
>
> This conclusion is not supported by your results. You should check what happens when this lake-enhanced sliding is turned off.

In the sensitivity experiment *nSG* (see supplementary document), which was also mentioned earlier, the "slippery grounding line" parameterization was turned off. This does indeed drastically change the results, as the mass flux into the lakes decreases. The sensitivity to the grounding line treatment will be discussed in the revised manuscript.

> > *L395: "Reconstruction of lakes could benefit from more realistic accounting of water fluxes"*
>
> The assumption that the lakes are always filled to overflowing is probably one of the most justifiable ones you made, so I doubt that including a water transport model would significantly alter your results.

This might not be true though. For instance, there is a hypothesis that the Moorhead low stage of Lake Agassiz was caused when there was insufficient meltwater and water from other sources in the catchment to exceed evaporation (Lowell et al., 2013). Therefore the lake were not necessarily filled to the top.

> > *L397: "a more physically motivated calving model valid for grounded and floating ice termini or a lacustrine sub-shelf melting model, would improve the ice-dynamical response to lakes"*
>
> Since you already show that the choice of calving law has a strong impact on your results, this one seems a lot more important.

We agree.

> > *L406: "The lake model promotes the formation of continental ice streams"*
>
> Your figures only show ice lobes, no ice streams.

Our results show the increase of ice flow, which is triggered by interactions with proglacial lakes. The increase of ice flow, which we called ice streams, might not fulfill the characteristics of locally confined ice streams. We will therefore reformulate these text passages and speak of increased ice velocity.

From RC3

> Lastly, a minor point: the phrase "dynamical topography" is typically used to describe tectonic movement, changes in elevation due to mantle convection, and other processes that act on the Myr timescale.

You are referring to line 22, where we write "[...] and topography was dynamic", don't you? We have rephrased this sentence in the revised manuscript:

"[...] and topography was not static."

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