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Discussions

Interactive comment on “PISM-LakeCC: Implementing an adaptive proglacial lake boundary into an ice sheet model” *by* Sebastian Hinck et al.

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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.

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.

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.

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.

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.

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.

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.

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.

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.

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 ($\sim$60h computation time, including the SELEN sea-level 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?

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 set-up, prescribed externally with the glacial index method. What you’re describing here is a routine that determines the ocean mask.

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.

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?

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?

L249: “Another important issue that is ignored in our model is the effect of ice meÌĄlange. . .” 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?

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.

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?

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.

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.

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?

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.

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).

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?

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.

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?

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.

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?

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

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 wouldn’t expect the LGM ice sheet to be in equilibrium with the LGM climate in any case).

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

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.

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.

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.

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.

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.

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.

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.

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.

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

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