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.

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.

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. 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. 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.

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.

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.

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.

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.

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.