Response to the editor comments by Lev Tarasov,

We thank the editor for constructive and helpful commentary and would hereby like to address some of the issues they raised.

As you draft your response to reviewers, I have identified 4 other major issues that need to be addressed.

1) **Experimental design.**

The current design leaves too many uncertainties unaddressed in part stemming from model limitations somewhat buried or not even described in your much too brief model description (as already raised by one of your reviewers).

The model description will be expanded. There are three processes that will be explained in more detail:

1) Treatment of proglacial lakes. We use the difference between bedrock and sea level to determine the location of lakes. We also acknowledge that as a limitation.
2) Basal hydrology was not mentioned in the method, and this should have been there from the start. An appendix will be added that explains the way basal hydrology, basal roughness and sliding law are implemented in our model and presents the governing equations.
3) The treatment of grounding line migration. Here we used a sub-grid friction scaling scheme. This process is important to all our experiments, so we will explain it in mfore detail.

Additionally, each simulation has slightly different ice volumes at glacial maxima, which has an effect on the deglaciation. This effect is small, but difficult to completely disentangle, and will be acknowledged.

We have also conducted High and Low Friction cases for Fast GIA and Rough Water to show that our main conclusions are consistent regardless of basal friction.

We will furthermore mention this limitation in more detail, as well as indicate that there are more sophisticated methods to deal with basal friction (e.g., sediment mask / models) and basal hydrology.

1a)

As far as I can tell, you do not use a soft/hard bed mask for Eurasia, even though it has long been known that this can have a large impact on ice sheet response and geometry (eg cf Tarasov and Peltier, 2004). So a significant

fraction of your sensitivity to basal drag changes from pro-glacial lakes could be due to this easy to address design limitation as regions that are soft-bedded will generally have less basal drag (when warm-based).

For basal friction we use a parameterization from Martin et al. (2011). However, we do not use a sediment map to calculate basal friction.

Therefore, we will add two additional experiment to investigate the importance of basal friction on the deglaciation.

We perform two new experiments by multiplying the till friction angle with 1.5 (High Friction) and 0.5 (Low Friction). A new paragraph will be written to show the results of these two additional experiments. These include showing a time-series as well as 2D ice thickness / bedrock topography maps of the last deglaciation.

The same friction experiments will also be applied for Rough Water and Fast GIA, adding an additional four experiments. Though the results of these four experiments will be added to the supplementary information instead.

We found that the basal friction has a substantial effect on ice volume as well as the deglaciation and we will mention that this shows the importance of basal friction / hydrology on modelling glacial cycles.

To briefly summarise the results of the friction experiments:

With increasing friction, ice volume increases (at LGM a 14% increase for high friction and 16% decrease for low friction). The differences in extent at glacial maxima is similar regardless of the basal friction use here (e.g., at LGM we found <5% difference). An increase in friction will cause a slower deglaciation.

However, basal friction remains a substantial limitation to these experiments, and will be addressed in the discussion section.

1b)

I suspect you have no basal hydrology (again model description is way to brief on relevant aspects of the model configuration) and

I also suspect you are simply taking the adjacent water depth to compute effective pressure for the Budd sliding law. On a 40km grid, that choice is hard to defend. At the very least, try an easy to implement leaky bucket approach (eg as in PISM or https://tc.copernicus.org/preprints/tc-2022-226/) for comparison.

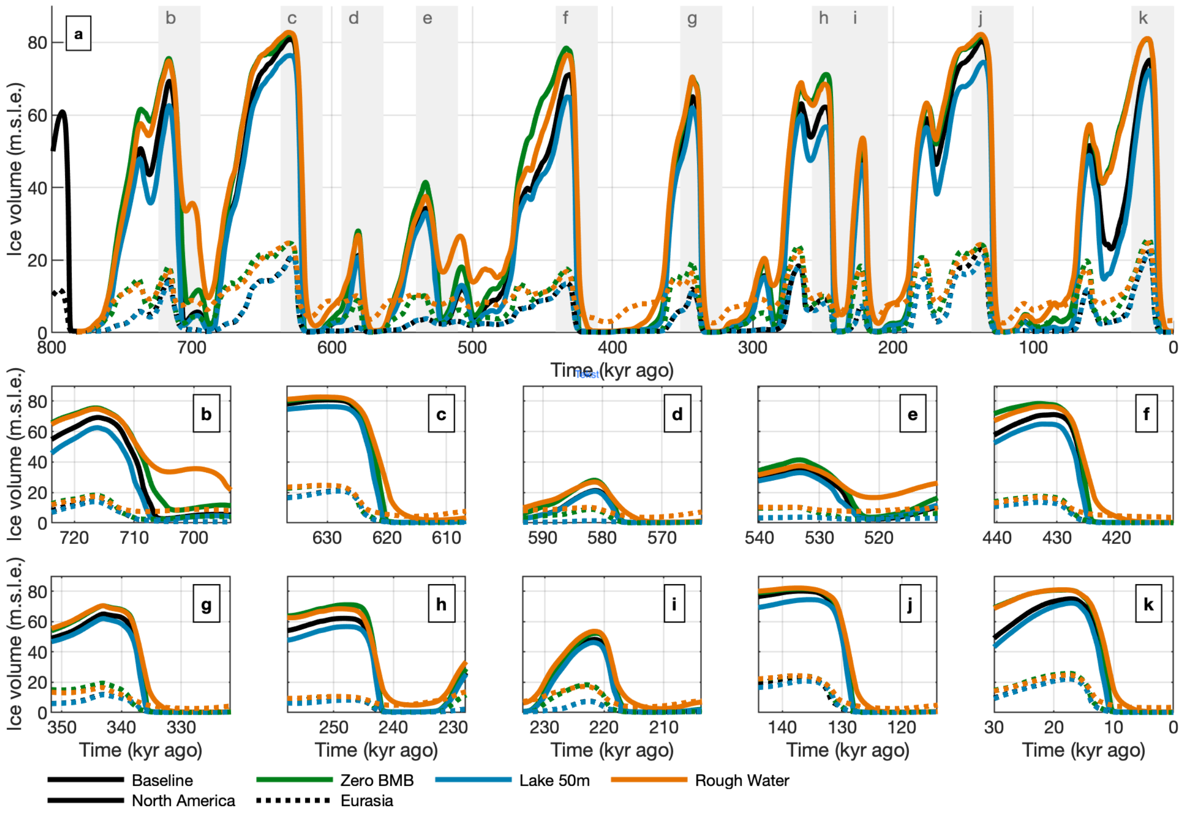
Our basal hydrology parameterisation is based on Martin et al., 2011 (The Cryosphere), and has been used in PISM. Here, the pore water pressure is calculated based on the ice thickness, bedrock topography and sea level.

Currently, the basal hydrology has not yet been explained in the model description, and will be added when revising the manuscript. A new appendix section will be added to describe the basal hydrology, till friction angle, resulting basal friction and sliding law.

1c) The assumption that lake depth is given by bed depth below contemporaneous sealevel is another major limitation, eg long known in the geological community (or cf Tarasov and Peltier 2006 from a modelling perspective) from the critical dependence on controlling sill elevation for eg Laurentide Lake Agassiz. Surface drainage solvers have been in some published models for almost two decades. I can understand adding one is non-trivial, but at the very least more thought needs to be put into addressing the some of the uncertainties arising from this model limitation.

To address these issues, we propose an alternative experiment to quantify the effect of this simplification. We select a large region in North America (south of the Hudson Bay to southern margin of the domain) where the water level of potential proglacial lakes is set to 50m above present-day sea level.

The results are shown below compared to the Baseline, Zero BMB and Rough Water simulations. The Lake 50m experiment loses more mass during interstadial periods, but is very similar compared to the Baseline during the deglaciation, advancing the deglaciation by at most a few centuries. This effect is small compared to the Faster GIA and Rough Water experiments.



2) Scientific precision/accuracy/transparency

Eg:

"We find that the modelled sea level matches the reconstructions well"

Your model fails to capture any signal of last glacial inception, so blanket use of "matches the reconstructions well" is inaccurate.

We will weaken this too strong statement. Glacial inception is not the key focus in these simulations, and could be more addressed in more detail.

No where do you show your LGM ice extents (only 11 ka), instead your only LGM ice extent map shows a cited map from Abe-Ouchi et al (2015). This may suggest to the reader that you do not want them to see what our LGM ice extent looks like.

We will add maps of the LGM ice sheet – as well as other time-slices – to the manuscript.

Our ice sheet extent at LGM matches reconstructions reasonably well, compared to other freely-evolving paleo-ice-sheet models (e.g., Berends et al., 2018. Willeit et al., 2019). Though, we lack substantial ice coverage in Scotland and Southern Alaska.

This mismatch is not currently described in our results, but it will be added to section 3.1.

Or

"our simulations tend to have slightly too long interglacial periods compared to reconstructions" From what I can read off of your plot in figure 3, your last interglacial is @ 50kyr too long, that is not "slightly".

Interglacial periods are too long in our simulations and we will emphasise this more.

Adequate referencing of past relevant litterature, eg role of GIA in deglaciation of ice sheets (eg Tarasov and Peltier,
Ann. Glac., vol. 25, 58-65, 1997), impact of pro-glacial lakes on
ice extent (eg Cutler et al, 2001, Geology), or relative sensitivity
of Eurasian versus North American ice sheets to orbital/CO2 forcing (eg Tarasov and Peltier, JGR 1997).

We have added these, and several other relevant references for these topics.

4) Claims in conclusions that havxe not been shown, eg: "3.4 Glacial isostatic adjustment ... The North American ice sheet may not even fully deglaciate during some interglacial periods. This is because proglacial lakes are not created when the bedrock uplift is too fast". This is not shown in your results (which would require a further sensitivity experiment with lakes turned off for soft/hard GIA experiments).

We will add 2D maps with time-slices from the last deglaciation comparing the Fast GIA, Slow GIA and Baseline simulations. These figures show that proglacial lakes become smaller with increasing uplift rates. The Fast GIA simulation will show that the proglacial lakes are much smaller and mostly absent compared to the Baseline simulation.

Additionally, we have conducted the Fast GIA, Rough Water and Baseline simulations with a 50% higher and 50% lower till friction angle. While the till friction angle does have a substantial effect on the timing of deglaciation, it does not change the main conclusion of the paper: The Fast GIA has a slower deglaciation compared to the Rough Water experiments, and the Baseline has the fastest deglaciation regardless of till friction angle.

Results of these additional friction experiments using the Fast GIA and Rough Water will be added to the supplementary information. Similar friction experiments with the Baseline simulations will be discussed in a new paragraph in the Results section.

Furthermore, ice volume is a very limited metric. Lake calving will more directly affect ice extent than ice volume (though the two are obviously related). Without relevant map-plots (at least in a supplement), the reader is unable to adequately evaluate the extent to which you results match your claims.

Indeed, we will add 2D ice thickness maps to the manuscript showing the last deglaciation of the Rough Water, Baseline, Fast GIA and Slow GIA, as well as the newly created Friction experiments. These maps show the Northern Hemisphere ice sheets at different time-periods (e.g., last glacial maximum, 11 ka, 9ka, present-day). Additionally, we will add time-series of ice volume compared to extent to the supplementary information.

The following figure is an example of what these maps will look like. (In this case for the new friction experiments.)