Response to the review by Fuyuki Saito:

First of all, we would like to thank the reviewer for these comments. We have made these minor changes to the manuscript, and ensured there is a clearer distinction between months and meters in the equations. To go into more detail (the reviewer comments in bold, and ours in regular font-type):

Now I am very happy to see that all the concerns I raised at the first review are solved, except for some minor technical correction. Indeed, they are not the author's but the reviewer's (my) fault, about units in some equations.

Eq. (24) m is used as variable in the left hand side, thus the units m appended to two coefficients (-15, 0.015) are really confusing. I am very sorry to have suggested this, but it is better to remove. Instead, please mention D is expressed in terms of unit meter in the main text.

We have removed the "m" in equation 24 and made sure to mention that both firn depth (D) and the amount of melt previous year (also Eq. 24) are in meters.

Eq. (26) the same as above. Remove m/yr, and please define the unit of s term is m/yr.

We now mentioned that the variables *s*, but also *l*, and *P* have units of *m/yr*.

In addition, please define m corresponds to month (I suppose) somewhere in the text.

We have replaced every instance of m (as months) with *mnth* (in equations).

Response to the review by Niall Gandy:

Re-Review of “Late Pleistocene glacial terminations accelerated by proglacial lakes” by Scherrenberg et al.

Signed: Niall Gandy

Thank you for your careful and detailed response to my reviews of the previous draft of this manuscript. The revised manuscript is much improved, and it is clear that you have invested some considerable effort responding to reviewer and editor comments. Of specific note;

1. **There are significant improvements in the justification of the experiment design. I find the new 2D map plots particularly helpful in understand the modelled behaviour. The improved model description and description of the “Rough Water” simulation is also helpful. Whilst not a material change to the science output, these changes are helpful for the readership.**
2. **The addition of the flux budgeting figure in the supplementary information is also helpful in understand the behaviour of the system.**
3. **The careful revision of Figure 4 is a significant improvement.**

I note that there are some remaining questions from the editor on your response to my initial comments, including on the computational cost on calculating the missing lakes, on the choice of proglacial lake level at 50m, and on a clearer justification of the potential difference between lacustrine and marine dynamics. Assuming a robust response to these points, I would recommend this manuscript for publication.

We would like to thank the reviewer for their comments, and we have now answered the remaining comments from the editor.

Response to the editor's comments by Lev Tarasov:

First of all, we thank the editor for their comments. Editor's comments are listed in bold, ours as regular font type.

 # Final points to address ('#' prefix denotes editor paragraph)

 # ensure you have somewhere indicated your conversion factor for ice volume to sea level equivalent, and whether you account for changing ocean area and exclude floating ice in your sea level calculation.

We have added a sentence to the method section to explain how we calculated sea level. While we do not account for a change in ocean area, we do exclude floating ice in our sea level calculation (see line 127)

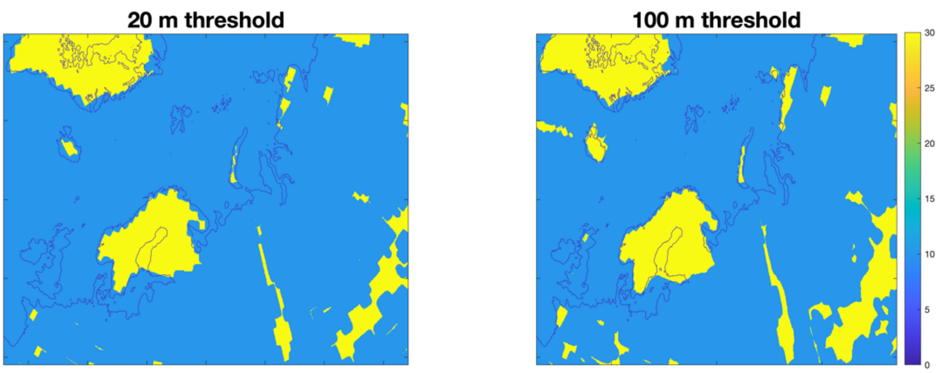
we generated a till friction angle map based on the sediment thickness map from Laske and Masters (1997), where we use till friction angles of 10 degrees for sediment thicknesses below 100 meters and 30 degrees for thicknesses exceeding that threshold.

 # I assume you meant the reverse?

Indeed, this was an oversight in the text and we used high friction in regions without sediment, and lower friction in regions with sediments. This was correctly mentioned in the appendix, but not the method's section. We have now corrected it.

Also I find your 100 m threshold quite problematic as subglacial till deformation is mostly confined to the top few meters. A somewhat larger allowance can be made on the basis of topographic roughness, but I doubt the continental terrain of the Eurasian and North American ice complexes warrants 100 meter except for a few localized regions. You will need to justify the choice threshold in the paper (it may be that the coarse Laske and Masters will give about the same with eg a more defensible 20 m threshold), or at least provide a hindsight caveat.

Indeed, the threshold we used is too large, though the difference between using 20 m and 100 m threshold is small in the Laske and Master's map as it has a very coarse resolution (both spatial and in terms of sediment thickness). We do not expect the 20 m and 100 m threshold to make a large difference as it effects only a small selection of grid-cells. The figure below (which will not be added to the manuscript) shows the difference between a 20 m and 100 m threshold:



Nevertheless, we have change this in the text and stated this as a possible oversight. For North America, we used a mask based on Gowan et al. (2019), which we preferred due to the higher resolution, and as it was specifically made with ice-sheet modelling in mind, but this does not cover Eurasia (line 111).

interpolate between pre-calculated pre-industrial .. and forms a good alternative

to fully-coupled ice-climate set-ups

 # On what basis do you justify "good"? On it's own, I do not see it as a good alternative in this day and age. If "good" means gives the results you want, than state that specifically. Best to avoid subjective claims and provide approximately quantitative or more informative qualitative descriptions. Leave the judgments to the reader.

We have removed "good" from the text, and replaced the sentence:

"This allows us to implicitly include climate – ice-sheet interactions at low computational costs and forms a good alternative to fully-coupled ice-climate set-ups, which have high computational costs"

With:

'This allows us to implicitly include climate – ice-sheet interactions at low computational costs compared to fully-coupled ice-climate set-ups.'

(See line 147-148)

The modelled LGM extent matches the reconstructions reasonably well, though ice coverage is lacking in the British island.

 # unsupported claim, given missing EA deglacial reconstruction. Include the well-constrained DATED reconstruction of Eurasian deglacial ice margin retreat (Hughes et al, 2015, the first author can provide netcdf raster maps) for figure 3.

We have now included the Hughes et al. (2015) maps to figure 3, and changed the caption of the figure.

 # more figure 3 points
1) Please add the present-day landmask and large lakes contours. It will help readers geo-locate and for instance see evolution of Hudson Bay relative to today.

We have added the present-day coastline and contours showing larger lakes (e.g., the Great Lakes) to the figure as black contours. We have made this change not only in figure 3, but also in any figure showing 2D fields (Fig. 7, Fig. 9, and maps in the supplementary information).

2) As your ice sheet has already deglaciated by 10ka, comparing to 5ka is not illuminating. Please replace the 5ka comparison with 8 ka, especially given your claim:

We have replaced the 5 ka panel with 8 ka in figure 3.

"with full retreat already reached at 10 kyr ago rather than two millennia later"
 # this last statement is incorrect statement (deglaciation is not complete by 8 ka), which a plotted 8ka Dalton et al time-slice would make clear.

This statement should indeed have been 3 to 4 millennia later instead. Full deglaciation is reached roughly between 7-6 ka, so ~3 to 4 millennia later. There is some ice volume loss after, but this retreat is small compared to the main phases of deglaciation. (Line 200).

This is likely due to the absence of a feedback between melt water, the ocean and climate. For example, prior to the Younger Dryas (12.9-11.7 kyr ago),

 # On the surface, this would only explain a max 1.3 kyr discrepancy (length of Younger Dryas), especially since AMOC resumption tends to entail overshoot. More to the point, the above claim implies that ignoring all the physics of eg atmospheric circulation is not important. The absence of a dynamically coupled ocean (or some representation thereof) is definitely one factor, but I see no basis for the implied claim that it is the only relevant factor.

This is correct, it is one of the shortcomings, but there is multiple. We have changed this sentence to explain that (1) there could be multiple reasons for the discrepancy and (2) that the lack of ocean-ice interactions could partially (not entirely) explain it. (see line 201 and line 449).

We find that the modelled sea level matches the reconstructions well

 # Definitely not for last glacial inception. Provide a more precise/accurate claim This point was raised in my previous editor review, and your current response to the editor states: "We have weakened this too strong statement", but clearly this is not the case.

We have removed this statement from the manuscript.

Consequently, ice inception requires relatively low CO_2 concentrations and weak insolation.

 # insert "modelled" before "ice" since MIS5d (according to sea-level inference) is a clear counter example.

Indeed, this should have been specified. We have changed this accordingly.

Note that we apply these sea levels on the entire domain, as determining the exact location of lakes requires a high spatial resolution, as lower resolution can smoothen valleys and therefore miss drainage pathways (Berends et al., 2016).

 # The only relevant meaning of "exact" here is with the ISM grid resolution. And the cited reference has not shown that a high spatial resolution is required to get a lake volume within say 5% of the upscaled high resolution calculation (and even 5% accuracy is over the top given all the other uncertainties and resulting errors in the model to do with climate forcing, basal drag, GIA,..). To show that, one would have to compare upscaled volumes to that calculated with a more appropriate choice of 40km bed topography that accounts for subgrid drainage channels (cf the already cited Tarasov and Peltier 2006). Furthermore, I strongly suspect that even running the code of Berends et al., 2016 at say a cheap 10km resolution every 100 years with uncorrected bed would still give much less error than obtains by the current very crude sea-level approach.

Yes, this may provide a mid-way solution between a lake-model and computational resources. Since this deserves a broader explanation, we have moved the discussion on lake models from the results (line 340) to the discussion (see line 405-410) section.

 # Current wording is confusing. Do you actually raise sea-level by this amount, or just the geoid for lakes? Hopefully just later, in which case please phrase more clearly (eg "terrestrial geoid for lakes" or some such)

Sea level was raised in the entire grid. We have now specified this further in lines 348.

Appendix B: Climate time-slice interpolation

 # how did you choose your relative weighting between CO2, insolation, and albedo? Was this by adhoc tuning or by a specific methodology that led to unique weights. Briefly spell out approach used, especially if latter.

The albedo weighing (Eq. 6-7 and 11-12) are based on Berends et al. (2018). This is now specified in appendix B.

For the CO_2 vs insolation scaling (Eq. 8), we used a new approach: First we conducted a preliminary simulation based on the method by de Boer et al., 2012 (https://doi.org/10.1007/s00382-012-1562-2) and Berends et al., 2021 (https://doi.org/10.5194/cp-17-361-2021) where external forcing is changed to match the modelled and observed benthic $\delta^{18}O$ record (Ahn et al., 2017). As such our model includes a routine to calculate $\delta^{18}O$ from ice and deep-water temperature, again based on the aforementioned works. This method essentially gave us the forcing needed to match the $\delta^{18}O$ record. We then fitted summer insolation and CO_2 to this curve, and we obtained Eq. 8. Since this method is beyond the scope and focus of this paper, we have briefly summarised it in Appendix B.