

Reply to Editor comments

General comments:

Editor:

“Figure 18 and discussion: Several issues remain with this figure and analysis. Now it is clear that panel (a) does not add to the paper and I recommend you to remove it. While potentially some relationship can be seen, it is rather spurious. It is contaminated by the different factors we mentioned, but most importantly, it does not show convergence to a specific volume as resolution increases. This would be easier to see (and more typical shown) with the axes transposed. As resolution goes to zero, one would like to see the LGM volume asymptote to a specific value, whereas this fit asymptotes to infinity. I also note that Yang et al. (2022) use the same ice-sheet model at the same high resolution, and get a much lower value. This clearly points then to model calibration or boundary conditions playing a more dominant role in this uncertainty, rather than the resolution. Also note that Niu et al. (2019) state they run at 40km resolution. Please simply remove panel (a), and restrict the discussion to some plausible reasons for differences, like resolution (more generally), but also boundary conditions and model choices, and in particular, contrast with the last case of Yang et al (2022).”

“In addition, there are issues with panel (b). First of all, all values in panel (b) appear to be offset or wrong. Many do not match those in panel (a) or the original publications. Second, the distinction of SIA+grounded ice models is incomplete. I noted as examples all of the studies before 2005, which used SIA and the ocean-kill parameterization, essentially from my own memory. Since you choose to make this distinction in the plot, it would be necessary for you to confirm which models conform to these assumptions. Simpson et al. (2009), for example, uses the same model as Huybrechts (2002), and notably write "Ice-dynamics are simplified to the shallow ice approximation for large ice masses (Hutter, 1983). Grounded ice flows through internal deformation and basal sliding. Longitudinal stress is ignored and grounding-line dynamics are not modelled." So essentially "SIA and grounded ice only". Lecavier et al. (2014) also use an SIA-only model and grounded ice only (applying the same model as Huybrechts (2002), with a modification at the GL). This is a non-exhaustive list. I recommend a careful review. Likely, in the end, these models do not need to be singled out, since there is no systematic pattern as far as I can tell, either with resolution or with model choices. The only real conclusion one can make is that the current study is a high outlier, with much higher volume than previous work, which you note in the discussion deserves further research. I understand that the ice volume simulated on Ellesmere Island was not included (L604), correct?”

Author response:

We have now removed panel **a** from Figure 18 and removed the descriptions of a correlation with model resolution from the manuscript and captions altogether. In panel

b, there seemed to have been an error with the axis formatting in the previous version of the figure: causing values to be offset. Thanks for spotting this, this has now been corrected. We have now removed the distinction of models with SIA only or no floating ice from the figure and only included a sentence referring to this in the relevant manuscript paragraph. We double checked the numbers from the compiled literature regarding reported LGM volumes in SLE and we believe figure 18 now agrees with those (we found one error had been made with the value reported for Yang et al. (2022)). Indeed, our volume estimates exclude ice from the IIS (Ellesmere Island), as mentioned in the text on several occasion.

Editor:

“LGM GrIS volume, or volume anomaly: In previous readings, I overlooked this lack of precision, but I believe throughout you mean to refer to the volume anomaly relative to PD. So e.g., on L65 “maximum GrIS volume ... differing by a factor of up to 2.5”, this only makes sense as the anomaly. Likewise, Fig. 10 caption (and Fig. 12 caption), you could write “relative ice volume” instead of “ice volume”, since later you make clear that this is relative to PD. Also this should be revised in the section “GrIS volume and thickness during the LLGM”. Perhaps it would be most clear if you would simply refer to “GrIS sea-level contribution”, which is inherently relative to PD sea level. Otherwise please make sure this language is precise to avoid confusion. Finally, Fig. 18 and caption also needs to be modified. Please go through the manuscript and ensure the wording is precise in this way.”

Author response:

We have now made sure the text was clear when referring to GrIS LGM volume as anomalies relative to present, or sea-level contribution in m SLE, across all text, figures and captions.

Minor comments:

Editor:

“L604: ISS ← Please define before using.”

Author response:

This was a typo and should have been IIS, which is defined earlier in the text as Innuitian Ice Sheet. We thank the editor for spotting this mistake.

Editor:

“Please make sure somewhere in the text to also mention the total volume of grounded ice (in units of e.g. m^3) at the LGM and other times, so that both SLE and volume are provided to the reader.”

Author response:

We have now added the number for total grounded GrIS ice volume (excluding IIS, peripheral glaciers, and including ice below flotation), in $10^{15} m^3$ to section “GrIS volume and thickness during the LLGM”.