Response to Nicolas Young (RC1)

We want to thank the reviewer for their effort and the very helpful comments on our manuscript. Please find below the detailed answers to all comments. The reviewer's comments are in blue, our answers in black. Text from the manuscript is highlighted in grey, and changes to the manuscript are shown in red on a grey background.

This is an excellent paper. A very thorough piece of work that was a pleasure to read. Paper is well written and the figures are excellent. Figures do a great job at conveying information while being aesthetically pleasing. And, as a paleo person (I am not a modeler) I found the LGM and mid-Holocene reconstructions particularly interesting and commend the authors for doing a great job and discussing their results within the context of the geological record. Excellent stuff.

We sincerely thank the reviewer for this very positive assessment of our work. We are glad that the reconstructions, and in particular their discussion in the context of the geological record, were found useful from a paleo perspective. We are also grateful for the careful reading and the detailed comments, which have helped us to improve the manuscript.

Lines 25-27: I found these two sentences oddly structured. Perhaps the word "nevertheless" is making this seem like a bigger mystery than it really is? The authors could even think about just starting with the second sentence minus "nevertheless"

We agree, and we'll remove the "nevertheless".

Line 49: I don't think you need to start a new paragraph here.

We agree, and we'll remove the new paragraph.

Line 71-73: Yes, I agree, but this also seems like a slightly too negative way to cast this. There are tradeoffs with every simulation and no simulation hits all the benchmarks, including the author's own here.

Thank you for the comment; we'll rephrase this sentence as follows:

New version: Across the ensemble, they identified a systematic trade-off, whereby model configurations that achieved LGM-consistent ice extent did not accurately represent present-day conditions and vice versa.

Modelling framework section:

The ensemble combines atmospheric temperature anomalies from Buizert et al. (2018) with precipitation anomalies from Badgeley et al. (2020). Could the authors elaborate on the rationale for selecting the Buizert temperature reconstruction rather than the Badgeley temperature reconstruction, given that the precipitation forcing is taken from Badgeley? The two temperature reconstructions differ during climatically important intervals, including the Younger Dryas and the

early Holocene, which could influence the simulated timing and magnitude of deglaciation. To what extent might the inferred ice-sheet evolution and best-fit simulation depend on the choice of temperature reconstruction? Related, what is the rationale behind the "high-precipitation" scenario from Badgeley vs. the moderate or low scenarios?

The best-performing parameter set is identified using a single prescribed climate forcing (Buizert temperature anomalies and Badgeley precipitation anomalies). Because several perturbed parameters directly modulate the ice sheet response to climate (e.g., melt corrections, precipitation scaling, and ocean sensitivity), to what extent are the inferred optimal parameter values specific to this forcing? If an alternative paleoclimate reconstruction were adopted, would the calibrated parameter values shift substantially? Additionally, because the SHAP analysis identifies parameter importance within the context of the prescribed forcing, it is unclear whether the inferred parameter sensitivities and perhaps even the SHAP importance rankings themselves would be transferable across alternative climate reconstructions. A brief discussion of this limitation would help clarify the robustness and generality of the conclusions. More broadly, I think it's worth mentioning or highlighting that the authors here have essentially taken fixed climatology and then sampled model parameter space to achieve a best-fit to the observations while there is also the near-opposite approach of sampling climate space to see of that alone can produce a good fit to the observations (i.e., Briner et al., 2020; Cuzzone et al., pre-print/in review; https://doi.org/10.21203/rs.3.rs-9962320/v1)

Buizert et al. (2018) was adopted as the primary temperature forcing. Tabone et al. (2024) tested the sensitivity to this choice by forcing a second 3000-member ensemble with the Badgeley et al. (2020) temperature scenario (Fig. 3, Tabone et al., 2024). This comparison shows that the Badgeley reconstruction produces a damped and delayed early-Holocene temperature rise in northeast Greenland relative to Buizert, which worsens the fit to observed deglaciation timings at the sediment-core and moraine sites used in our skill-score evaluation (Fig. 3). Therefore, we selected the ensemble using Buizert et al. (2018) because it better reproduces the reconstructed margin retreat in the northeast.

Precipitation is nonetheless taken from Badgeley et al. (2020) for practical rather than preferential reasons: Buizert et al. (2018) is a temperature-only reconstruction and does not provide a precipitation field. Badgeley et al. (2020) is, to our knowledge, the only spatially complete, 20-kyr precipitation reconstruction for Greenland, so any simulation using Buizert for temperature necessarily requires an external precipitation product, and Badgeley et al. (2020) is the only currently available candidate for this role.

The high-accumulation scenario was selected because it produces the fastest and largest early-Holocene accumulation increase among the three Badgeley scenarios, and best matches the rapid NGRIP accumulation rise inferred from ice-core data, which was the focus of Tabone et al. (2024). Even so, our simulations still underestimate this rise by ~0.08 m/yr (Fig. S14a, Tabone 2024), so the high scenario represents our closest available approximation rather than a perfect match. The low and moderate scenarios would further worsen this early-Holocene mismatch.

We agree that our results, both the identification of the best-fit simulation and the inferred parameter sensitivities (including their effect on the performance of each metric, e.g. via the SHAP analysis), are necessarily dependent on the choice of climate forcing. We therefore agree to point out this limitation in the Discussion section, and to clarify in the Methods the rationale for selecting the forcings used in this study.

We also thank the reviewer for pointing out the complementary approach of sampling climate space rather than model-parameter space (Briner et al., 2020; Cuzzone et al., in review). We will mention both approaches explicitly in the revised manuscript, noting that our study fixes the climate forcing and explores the model-parameter space, whereas these studies explore whether climate uncertainty alone can reconcile simulations with the observations.

Therefore, the modifications in the manuscript will be:

1) In the Method section, L150-151 are modified with these sentences:

As developed in \citet{tabone2024}, \citet{buizert2018} is selected as it best reproduces the observed timing of deglaciation onset in northeast GrIS. Since \citet{buizert2018} does not provide a precipitation field, we take precipitation changes from the high-precipitation scenario of \citet{badgeley2020}, which best matches the rapid early-Holocene accumulation increase inferred from the NGRIP ice core. These changes are applied through the scaling relationship:

2) In the Method section, L163-L165 are substituted with these sentences:

The ensemble consists of 3,000 transient simulations spanning the last 22\,kyr, with identical climate forcing for all members, while 13 key parameters (Table \ref{tab_2}) are perturbed using Latin hypercube sampling \citep{stein1987}. For further details on model physics and experimental setup, see \citet{tabone2024}.

3) In the Discussion section, L563-L569 are substituted with these sentences:

Finally, our results show that, for the forcing and parameter space considered in this study, the performance of the simulations in reproducing deglacial observables is primarily driven by the specific ice-sheet sensitivity to climate forcing. The selected forcings \citep{buizert2018, badgeley2020} appear to effectively capture the reconstructed temperature and precipitation patterns characteristic of the deglaciation. Driving the ice sheet with these climatic fields serves as a robust approach to reduce the degrees of freedom within the system, effectively constraining the ice-sheet evolution to the observational record. However, this choice also conditions our conclusions. Several of the perturbed parameters, such as the melt corrections, the precipitation scaling and the ocean sensitivity, directly modulate the ice-sheet response to climate and may therefore partly compensate for biases in the prescribed forcing. Consequently, both the best-fit parameter values and the inferred parameter sensitivities, including the SHAP importance rankings, are specific to this forcing and may shift under an alternative paleoclimate reconstruction. Our approach, which fixes the climate forcing and samples the model-parameter space, is complementary to studies that instead sample the climate-forcing space to assess whether the associated uncertainty alone can reconcile simulations with observations \citep{briner2020, cuzzone_inreview}.

In addition, the one-way forcing used here neglects the impact of ice-sheet evolution on the large-scale climate, and a clear next step would be to address the problem with fully coupled simulations.

Figure 1: Did you want the HTM blue box to come all the way down to the x-axis? Double check

No, it is meant to be like that just to avoid covering the "Holocene" label.

Line 252: Slightly confused here. I don't think the fit to the Leger product was weighted more heavily, but here makes it sound like it is? Not entirely sure, very possible it is weighted more towards a Leger fit (which is fine) and I missed it.

You are right; we did not give more weight to any of the individual metrics (nor to the PaleGrIS isochrones). In that sentence, we refer to the fact that the final best simulation actually fits this metric very well, but not because of a higher weight. We will clarify this in the new version.

New version: Although all metrics are weighted equally in the global score S (Sect. 2.3), the best simulation (black line; i.e. that with the highest S) shows excellent agreement with the reconstructed grounded area from Leger et al. (2024), remaining within the observational uncertainty bounds throughout the period covered by the data.

Line 264-265 and Figure 3: I found the relative poorness of fit in SW Greenland extremely interesting as 1) SW Greenland has the most paleo observations in all of Greenland by a comfortable margin (authors might actually mention this) and 2) it's probably the region of Greenland least affected by dynamical processes (primarily land-based, KNS region excluded, at least relative to other regions) and mostly controlled by SMB (which is partly why the fit in Briner et al., 2020 is good)…and yet the fit here is relatively poor. Authors could highlight this point, possibly expanded on this with a few sentences. Or maybe at the very least point to section below where they speculate that the climate input is too warm in this region?

We thank the reviewer for this remark and agree that the relatively poor fit in SW Greenland is noteworthy, precisely because this is the region with the largest network of paleo-observations and one where the margin evolution is expected to be controlled mainly by surface mass balance rather than by ice dynamics. We discuss the possible causes of this mismatch in the Discussion section (Sec. 4.2), where we argue that the prescribed climate forcing is likely too warm (or too dry) in this region during the early-to-mid Holocene. In this regard, as the reviewer mentioned, Briner et al. (2020) achieved a good agreement with the reconstructed margin in SW Greenland using the Badgeley et al. (2020) reconstruction for both temperature and precipitation, whereas we prescribe temperatures from Buizert et al. (2018), which exhibit a slightly stronger HTM warming. Moreover, the original study (Tabone et al., 2024) focused mainly on the NEGIS, which may have biased the ensemble towards poorer fits in the SW, both through the choice of climate forcing and through the climate-forcing parameters explored in the ensemble.

In the revised manuscript, we will:

1) Add this sentence in L268:

While all ensemble members exhibit a broadly similar retreat pattern, these regions are prone to a systematically earlier retreat relative to the reconstructions. The mismatch in the SW basin is particularly noteworthy, since this region hosts the densest network of paleo-observations in Greenland \citep{leger2024} and, being largely land-terminating, its margin evolution is expected to be controlled primarily by surface mass balance rather than by ice dynamics. Possible sources of this mismatch are discussed in Sect. 4.2.

2) Expand the discussion in Sect. 4.2 to emphasise that, since the margin evolution in this region is mainly controlled by SMB, the misfit points to a bias in the climate forcing in that region rather than to the ice-dynamics parameters, consistent with the good agreement obtained by Briner et al. (2020) with a different temperature forcing. For that, we'll substitute L611-L618 for these ones:

Among studies simulating past GrIS evolution, only \citet{briner2020} achieves a good agreement with reconstructions in the southwest, although their simulation was restricted to this sector. This mismatch is especially relevant because the southwestern margin is largely land-terminating, so its evolution is expected to be governed mainly by SMB rather than by ice dynamics. The discrepancy therefore more likely reflects a bias in the regional climate forcing than in the ice-dynamics parameters. This bias may stem from insufficient precipitation in this region during the HTM, which is known to have played a critical role in limiting the ice-margin retreat in response to temperature forcing \citep{thomas2016}, or from an overestimation of regional warming. In this regard, \citet{briner2020} use \citet{badgeley2020} as forcing for both precipitation and temperature, whereas we prescribe temperatures from \citet{buizert2018}, which exhibit a slightly stronger HTM warming and could therefore account for the excessive retreat simulated here. In addition, the climate forcing and the ranges of the climate-related parameters explored in the ensemble were originally chosen by \citet{tabone2024} to reproduce the NGRIP elevation evolution through a correct representation of the NEGIS, which may have favoured a good fit in the northeast at the expense of the southwest. Nevertheless, this remains a common limitation among ice-sheet models simulating the GrIS through the last deglaciation.

Table 4: Likely due to my lack of model knowledge…how "reasonable" are these parameter fits? Is there any single value(s) that are pushing the limits of what is typically accepted/used for each of these parameters?

We thank the reviewer for this question. Most of the best-fit values lie well within the ranges commonly used in the literature, although a few lie towards the edges of our sampled ranges (Fig. 1). We summarise them below.

Ice dynamics. f_{mid}, f_{low}, z_0 and κ_t lie near the centre of their sampled ranges. The friction law exponent q = 0.9 lies in the mid-to-upper sampling range and is higher than the values typically used for Greenland (q = 0.2 as default; q = 0.33 in Joughin et al., 2019; q = 0.6 in Aschwanden et al., 2016). The parameter δ = 0.0241 lies in the lower range, but is close to the original value of δ = 0.02

proposed by Bueler and van Pelt (2015), and is thus within commonly used values. E_{shear} = 1.0037 also lies in the lower range; this parameter is poorly constrained, and our value falls within the ranges explored in previous studies, e.g. 1–3 in Aschwanden et al. (2016) and 0.5–3 in Leger et al. (2025), who also applied a single enhancement factor to the whole ice sheet.

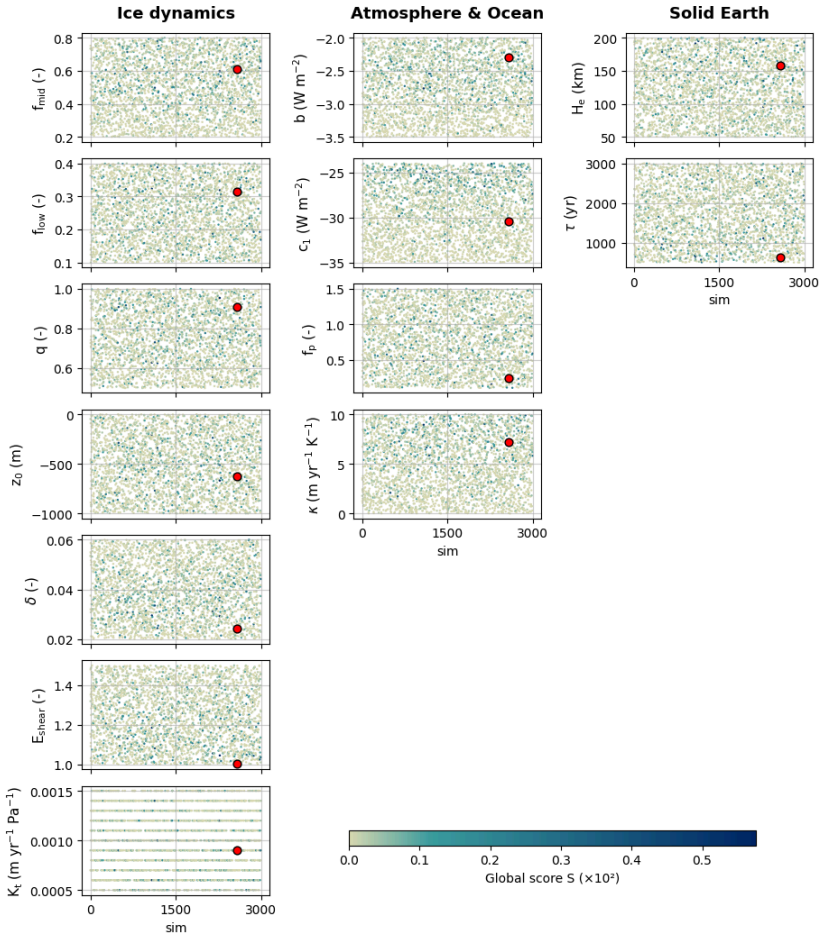


Figure 1: Parameter values of the ensemble simulations. For each of the 13 perturbed parameters, the color indicates the global score and the red marker indicates the value of the best-fit simulation (Table 4 of the manuscript).

Atmosphere and ocean. All parameters lie near the centre of their ranges, except f_p, which lies in the lower range. Since f_p is only perturbed in the NEGIS basin, this suggests that the Badgeley et al. (2020) precipitation reconstruction does not need high corrections in this basin.

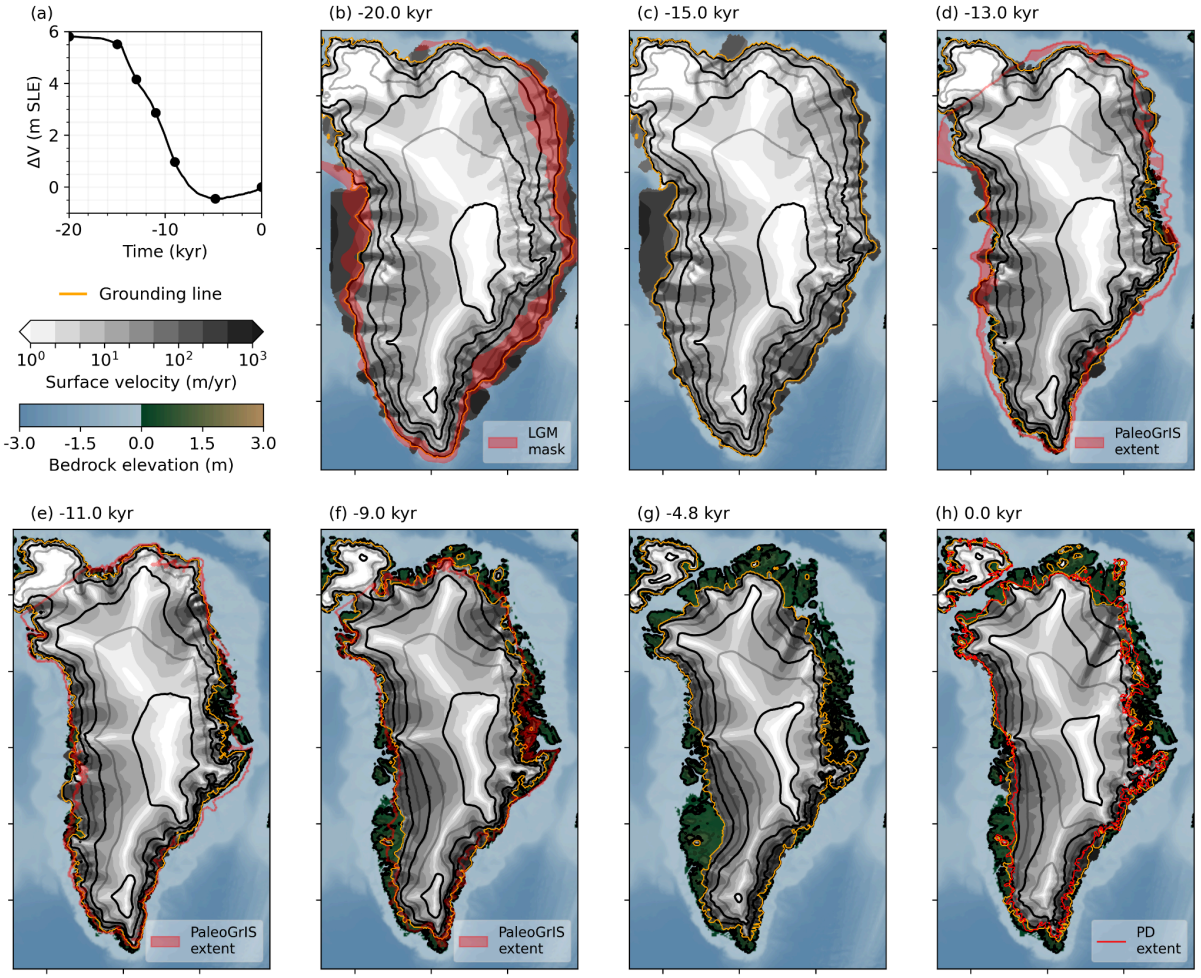
Solid Earth. The effective elastic thickness of the lithosphere, H_e = 158 km (sampled range 50–200 km), is higher than the 30–90 km (with uncertainties of up to ~40 km) inferred for Greenland by Steffen et al. (2018), who found a weakened lithosphere beneath southern and central Greenland. The asthenosphere relaxation time, τ = 620 yr (range 500–3000 yr), lies in the lower range, implying a faster isostatic response than the commonly used value of 3000 yr (Van der Berg et al., 2008). We note that, in the ELRA framework, both parameters are values that partly compensate for the simplified representation of the solid-Earth response, which in reality varies laterally across Greenland (Steffen et al., 2018; Milne et al., 2018).

In summary, only q and, to a lesser extent, H_e lie above the values typically used in the literature,

while T lies below them.

Figure 5: Why not change the timing of panel G to the actual minimum you reconstruct? I know the difference between the 5 ka and the actual minimum at 4.8 ka is negligible, but it seems odd to me to have 5 ka instead of the 4.8 ka that most people are going to care about.

Thank you for the suggestion; we have modify this in the revised manuscript. The resulting figure is shown below:



Line 331: Could add the Sbarra reference in here, and probably Jennings et al., 2017 (v. 472; EPSL) and Ó Cofaigh et al., 2013 (v. 41 (2) Geology). All of these pretty strongly support LGM ice at the shelf edge.

Indeed, Sbarra et al. (2022) provide good support for this point. We were not previously aware of Jennings et al. (2017) or Ó Cofaigh et al. (2013), and we thank the reviewer for bringing these studies to our attention. The three references will be included in the revised manuscript.

Line 353-355: I don't think this is necessarily the best way to cast this. I think it's fairly well known that the minimum extent, whenever it is, falls after peak HTM temperatures but…of course it does? The ice sheet is still so large and has so much inertia that the minimum almost must fall after the HTM. And, of course the ice sheet was indeed retreating/losing mass during the HTM. A more

appropriate way to think about this might be looking at the ***rate*** of mass loss. Rates are almost certainly higher during the actual HTM.

We thank the reviewer for their comment and we generally agree. Given the large inertia of the ice sheet, the minimum extent necessarily lags the peak HTM warming, and the ice sheet was losing mass throughout the HTM. Nonetheless, this inertia is often something that the wider Earth science community is not always aware of. With “maximum impact” we referred to the maximum cumulative contribution of the GrIS to sea level, which is reached at 4.8 ka, when the ice volume is at its minimum (0.45 m SLE below present). We acknowledge, however, that this wording may be misleading, and that the rate of mass loss is a more appropriate measure of the ice-sheet response to HTM warming. In our best simulation, the fastest Holocene warming occurs at the onset of the Holocene (~11.6 kyr ago) and temperatures peak at ~7.8 kyr ago, while the highest Holocene rate of mass loss (~115 mm SLE per century) is reached at ~9.8 ka, at the onset of the HTM. Mass loss then continues at decreasing rates throughout the HTM, until the ice sheet returns to mass balance at 4.8 kyr ago. However, these values are approximate, as they are derived from model output saved every 200 years, which limits the temporal precision of the timings to about ±200 years and smooths the peak rates.
We will therefore rephrase L352–L355 as follows:

Three different times must be distinguished when describing the ice-sheet response to HTM warming: those of maximum temperature, maximum rate of mass loss and minimum ice volume. In the best-fit simulation, the Holocene summer temperatures peak at 7.8\,kyr ago. The highest Holocene rate of mass loss (~115\,mm SLE per century) is reached earlier, at ~9.8\,kyr ago, near the onset of the HTM, and mass loss then continues at progressively decreasing rates throughout the HTM. Owing to the large inertia of the ice sheet, the minimum ice volume is therefore only reached at 4.8\,kyr ago, roughly 3\,kyr after the thermal maximum, with a total volume 0.45\,m SLE below the present-day state (Fig. 7d). Note that these timings are derived from model output saved every 200 years, which limits their precision to about $\pm$200 years and smooths the peak rates.

Line 359: You can just start this paragraph with ‘At 4.8 ka..”

We will modify the manuscript accordingly. L359 will start:

At 4.8 kyr ago, surface elevations at …

Line 400-403: Does this suggest the best-fit LGM sea-level equivalent is not a robust estimate?

We think this is not the case. The independence of the LGM score from the other metrics indicates that the parameters controlling the performance of LGM reproduction are decoupled from those governing deglacial and present-day performance, i.e. certain parameter combinations can produce an accurate LGM extent while failing to reproduce, for instance, the present-day ice-sheet state or the elevation change in specific locations. This does not undermine the robustness of the LGM estimate itself: rather, it justifies our scoring approach, in which a poor score in any single metric leads to a low overall score. The resulting LGM sea-level-equivalent estimate is therefore robust

precisely because it comes from parameter combinations that not only place the LGM extent within the known constraints, but also reproduce the remaining metrics.

Line 529-530: Could add Cuzzone et al., 2022 (The Cryosphere, v. 16) here. They investigated this very idea in the KNS system and still couldn't achieve a great fit with the observations.

Thank you for the suggestion. We will add Cuzzone et al. (2022) here, who simulated the KNS region with a high-resolution regional model and still could not fully reproduce the observations, indicating that unresolved fjord geometry is not the only source of model–data mismatch in this region.

Line 563-564: See the last 1/3 of my second modeling framework comment above.

Following up on the comment above, we will reformulate this sentence to reflect the dependency of the results on the selected forcing as commented above (pag. 3).

Figure 14 (and related text): It seems like the authors' model is perhaps more sensitive than other set-ups? Their LGM SLE is towards the higher end of the range of estimates and, conversely, their SLE for the Holocene minimum is also towards the high end? Best fit = both "big" LGM ice and "small" mid Holocene ice.

We thank the reviewer for this remark. Our simulated GMSL contribution does lie in the upper or upper-to-middle range of published estimates both at the LGM (in negative terms) and at the HTM (in positive terms), and therefore implies a larger glacial–interglacial amplitude than most previous reconstructions. We will state this explicitly in the Discussion, in L571-L573:

We now show the contributions to global mean sea-level change (ΔGMSL) during the LGM and the HTM from the best-fit simulation in the context of the broader ensemble (Fig. 14b and d). As discussed below, our ensemble and best simulation lie within the upper-to-middle range of previous studies (Fig. \ref{fig_gmsl}a and c) for both periods — in negative terms at the LGM and positive terms at the HTM — and thus imply a larger glacial–interglacial amplitude than most previous reconstructions.

We are nevertheless unable to establish whether this reflects a higher intrinsic sensitivity of our model or rather reflects the forcing and the overall experimental set-up, which may simply allow larger changes during these periods. Each of the studies compared in Fig. 14 uses its own forcing — and some of them additionally impose a maximum LGM margin that prevents the ice sheet from expanding beyond a prescribed limit (e.g. Lecavalier et al., 2014) — so the spread shown there aggregates differences in model sensitivity with differences in forcing amplitude. The applied LGM temperature anomaly alone illustrates this: it amounts to approximately −5 to −10 °C at GISP2 in Leger et al. (2025), about −15 °C at GRIP in Lecavalier et al. (2014), and roughly −12 to −22 °C in central Greenland between summer and winter in Buizert et al. (2018), from which our own forcing is taken. Attributing the amplitude to model sensitivity would therefore require applying a common forcing across models, which is beyond the scope of this study and is left for future work. For consistency, we have also rephrased the corresponding sentence in the abstract (L17–18), which previously referred to a "mid-to-high GrIS sensitivity". It now reads:

This is in the upper range of previously existing estimates, implying a comparatively large glacial-interglacial amplitude of GrIS volume changes over the last deglaciation.

Line 595-597 and again down ~619-622 and ~635: I might couch all of this a bit differently. I think the authors reconstructions are consistent with all other simulations, that is, they tend to show quite a bit of retreat in SW Greenland during the Holocene. But I also think all of the geological benchmarks over the last ~15 years point to rather limited retreat. So if the question is how far exactly did the ice margin retreat, then it's a bit unknown, but if the question is did it retreat "a lot" (models) or "not that much" (paleo observations) then I'm not sure I'd call it an "open question" (its not that much).

We thank the reviewer for this point. Like most previous model reconstructions, our best simulation shows substantial inland retreat in SW Greenland during the Holocene (~120 km in the Kangerlussuaq region), whereas the geological evidence gathered over the last ~15 years consistently points to a much more limited retreat (2–26 km; Sect. 4.2). We will rephrase these passages accordingly: rather than presenting the magnitude of retreat as an open question, we will state that the geological evidence indicates limited retreat, that our simulation (like other models) likely overestimates it, and that the exact position of the minimum margin remains uncertain.

In L599 we will add:

As in most previous model reconstructions, this retreat is larger than inferred from geological evidence, as discussed below.

In L609–L610:

The southwestern retreat in our simulation, while spatially consistent with observations, is therefore likely overestimated, particularly in the Kangerlussuaq region, [...]

L619–623:

Furthermore, Lecavalier et al. (2014) demonstrated that a sufficiently large retreat and consequent readvance of the GrIS is required to reconcile RSL observations from west Greenland, suggesting that the precise magnitude of HTM retreat could be larger than what indirect observations show. However, the geological evidence gathered over the last ~15 years consistently points to a limited Holocene retreat in southwest Greenland \citep{young2015, lesnek2020, young2021}. Our simulation, like most previous model reconstructions, therefore likely overestimates the retreat in this sector, although the exact position of the margin at the minimum extent remains uncertain. Reducing this model–data mismatch will require an improved representation of regional temperature and precipitation patterns and of ice–climate interactions.

L635-L637:

This clustering suggests that the limited retreat in the Kangerlussuaq region inferred from geological

evidence could provide a critical observational target for narrowing uncertainties in GrIS sea-level estimates during the Holocene minimum. It also implies that the simulated HTM contribution of 0.45 m SLE may be overestimated, and that the ensemble range should be regarded as an upper bound.

Holocene minimum SLE – if the best-fit simulation likely overshoots SW Greenland inland retreat, the same region thought to be the primary contributor to SLE during this interval (which the authors fully acknowledge), is the 0.45m SLE itself too high then? Should this (and the full range presented by the authors) be considered a maximum value?

Yes, this is a fair point. As discussed in Sect. 4.2, the records indicate that our best simulation likely overestimates the inland retreat in SW Greenland. Given the close relation between the retreat in this region and the post-HTM sea-level contribution (Fig. 15 in the manuscript), the 0.45 m SLE may overestimate the actual contribution, and the ensemble range should rather be seen as an upper bound. We have added this point at the end of Sect. 4.2 (L635–637, see our reply to the previous comment) and in the Conclusions (L648), which now are:

This best-fit simulation yields a GrIS contribution to GMSL of -5.75\,m SLE at the LGM, with an ensemble uncertainty range of -3.93 to -6.31\,m SLE. During the HTM — a period during which Greenland summer temperatures exceeded present-day values — the simulated contribution reaches 0.45\,m SLE, with an ensemble uncertainty range of 0.43 to 1.18\,m SLE. These values are in the upper-middle range of previous estimates and, for the HTM, may overestimate the actual contribution and should therefore be regarded as an upper bound, given the likely overestimated retreat in southwest Greenland.

Note that we have taken the opportunity to correct a typo (the range for the HTM was listed as 0.45–1.18 m, but it is actually 0.43–1.18 m).

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