Reply to Editor comments

"To the authors,

I believe the manuscript is near ready for publication. You have thoroughly addressed the comments of the editors. Nonetheless, after reviewing the manuscript myself, I find there are several additional minor revisions needed. Please find some more general comments below followed by more specific points. I kindly invite you to submit a revised manuscript with all of these points addressed.

Kind regards,
Alex"

General comments:

General comment 1:
"*The Abstract is quite long and provides many details that are unnecessary at this stage. It is recommended that the abstract fall within 150–250 words, while you have > 300 words. Please revise, so that the overall message and some key information come across, without too many additional details, etc. In particular, L23-30 ("Given large uncertainties...") could be reduced to 1-2 sentences. From L30 on, language could be a bit more concise."*
Author response:
We thank the editor for this comment. We have now made the abstract more concise, following recommendations above. The word count is now 296 words.

General comment 2:
"*Martos et al (2018) geothermal heat flow has a constant value of 50 mW/m2 outside of the PD land area. Do you think this affects the results at all, particularly during the glacial expansion of the ice sheet to the continental shelf? Did you test other values of this background value? Please also document in the text how the map was generated for values outside of the PD land area."*
Author response:
We thank the editor for this comment. We have now added a sentence to document how the data was generated outside of land areas: see our reply to specific comment 13.
We have not tested other background values than the one of 50 mW/m2. However, we do not expect this to have a major impact on the misfit in extent observed during expansion of the ice sheet to the continental shelf: which only concerns the NE sector : a region where our modelled GrIS is not extensive enough according to data during the local LGM. Since we use a low-end value as background (lowest land values from Martos et al., 2018), testing other values would mean most likely increasing the

background GHF value: which would result in slightly more basal mass loss and thus possibly slightly lower GrIS extent during the LGM: which is the opposite of what we want to correct our misfit. However, this low GHF background could introduce a small bias during deglaciation of the GrIS: which could perhaps be slightly more pronounced if a higher background GHF value was used, although given the much lower numbers we obtain in grounded basal mass loss rates relative to sub-shelf and surface mass loss rates during deglaciation, we expect this possible bias in GHF outside land areas would have little impact on the model behaviour during deglaciation. During the Holocene: the actual Martos et al., (2018) PD land area values apply to the vast majority of the GrIS.

General comment 2:

"*It is well known for paleo simulations that accounting for changing insolation in the surface energy balance is important for simulating the SMB well (e.g. Robinson and Goelzer, 2014, doi:10.5194/tc-8-1419-2014; Krebs-Kanzow et al., 2021, doi:10.5194/tc-15-2295-2021).*

It is clear that during the early Holocene particularly at high-latitude sites like CC, the additional contribution to melting from increased insolation would be important. There are now multiple approaches available to incorporate insolation forcing, including the dEBM-simple model that is part of the PISM package. You should acknowledge this deficit in the "The ice-atmosphere interface" sub-section, and include a paragraph about the possible implications of accounting for insolation changes on melting in the Discussion.

Also, please discuss whether the lack of inclusion of Baffin Island in your domain may impact your results. You can refer to the recently published paper by Lauritzen et al. (2025, doi:10.5194/tc-19-3599-2025)."

Author response:

We thank the editor for these insightful comments. In section 5.2, discussing potential reasons for observed misfits in modelled GrIS deglacial retreat, we had written a couple of sentences on the limitations of the PDD and how a more energy-balance-accounting model would likely be more appropriate. In line with these comments: we have now added slightly more detail to it related to past insolation forcing and its importance: with a reference to the dEBM method and study by Robinson and Goelzer (2014).

Note that when we designed this experiment with PISM: in Septembre 2022: the dEBM method had not yet been incorporated to PISM. Had it been the case, we would have most likely considered using it instead of the PDD. However it must be acknowledged that using the dEBM instead would have introduced more uncertain, poorly-constrained SMB parameters which would have needed to be perturbated in the ensemble given their uncertainties: most likely meaning that we would have required to run a larger ensemble than 100: which was pretty much impossible with our timeline and computing resources, given that we aimed for 5 km resolution. Anyhow, in future ensemble experiments: it would be interesting to test a more complex SMB parameterisation like dEBM. Please find below the added text to the discussion section related to this point: which fits the discussion sections better than the method:

“Furthermore, our SMB parameterisation, based on on a simple PDD scheme (Calov and Greve, 2005), does not capture additional contribution to melting from past changes in insolation forcing, nor certain ablation mechanisms such as sublimation and wind-driven snow layer erosion, nor does it fully capture the elevation feedback between the modelled ice-sheet surface and climate forcing. These missing mechanisms may be important to model deglacial GrIS thinning and retreat accurately at high latitudes (>75°N), where mean summer air temperatures during the HTM remained close to or below 0°C (at least in our forcing data) (Fig. 5) (Plach et al., 2019), and where additional summer melt contributions from increased insolation during the Late-glacial and early Holocene were likely important (Robinson and Goelzer, 2014). In future work, the use of SMB energy-balance models incorporating insolation forcing (e.g. dEBM; Krebs-Kanzow et al., 2021) instead of PDD parameterizations would potentially help reduce model-data misfits in GrIS extent during the last deglaciation.”

Regarding including Baffin Island in our domain: the influence on the GrIS would be limited, we believe, as grounded ice did not coalesce over Baffin Bay between the Laurentide and GrIS, only floating ice shelves from the two ice sheets most likely merged. In our modelling: Baffin Bay is already covered entirely by a floating ice shelf from the GrIS anyway. Moreover, our model-data fit with PaleoGrIS is rather good throughout the Western GrIS: as is shown in Figure 21 and 22. Our misfits mostly concern the NO, NE, and CE GrIS sectors, where including or excluding Baffin Island would have no impact.
In the study by Lauritzen et al. (2025): the misfit with PaleoGrIS and ice-extent markers is much larger than with our modelling (see figs 6 and 7 therein): as they obtain a much delayed deglaciation with Baffin bay still fully covered by ice shelves all the way until 9 ka (by this time the GrIS and IIS should already have started to disconnect, and all floating ice shelves should have disintegrated, and the GrIS should have retreated over much of the western continental shelves). Moreover, to obtain correct modelled ice flux over Baffin Island during the local LGM most likely requires to model a larger chunk (if not all) of the Laurentide ice sheet.

Specific Comments:

1) Editor:
"L18: of paleo → of the paleo"
Author response:
Change made accordingly

2) Editor:
"L29: LGM must be defined here, or written explicitly."
Author response:
Change made accordingly

3) Editor:
"L46, L48: sea level rise → sea-level rise [and throughout, check for compound adjectives.]"
Author response:
Change made accordingly

4) Editor:
"L99: a more direct → more direct"
Author response:
Change made accordingly

5) Editor:
"L111: The acronym "lLGM" has not yet been defined, although I understand it is supposed to mean "local LGM". The acronym should be defined before it is used. Also consider changing to something specific to this domain, like "Greenland LGM (GLGM)" or "G-LGM". The "local LGM" will be different for different domains, whereas the GLGM can be defined as a specific time. Furthermore, the lowercase "l" is easily confused with a 1."
Author response:
We have now made sure the acronym was clearly defined explicitly before being used for the first time. The term "local LGM" is now commonly used in Quaternary glaciology to describe the maximum last expansion of the glacier / ice sheet that is the subject of the study. We believe it would be appropriate to follow this new convention.

6) Editor:
"L125: 24 kyrs → 24 kyr"
Author response:
Change made accordingly

7) Editor:
"Figure 2: I think you could remove "Bed topography" and "Present-day GrIS thickness" from the first row of "inputs", or show them in their own row in the top-right corner, as

these are only used as initial conditions for the simulation in Step 1, and then the model takes over - the other fields remain as forcing, so are materially different.”
Author response:
We have now moved these two boxes upwards and removed the arrows connecting them to the PISM ensemble box. Indeed, we agree this makes more sense as such.

8) Editor:
“Figure 2: This schematic is useful for clarifying the approach, but it is quite busy. Could you perhaps reduce the number of arrows and number labels between boxes. Particularly, you could make the step 2 arrows converge like this of step 1, and then only have one arrow go into the "PISM ensemble simulations" box. It might be a bit cleaner like this. Then you probably only need one "1" circle and one "2" circle. Also the "3" and "4" circles could be reduced. Perhaps eliminate between "Model-data comparison ..." boxes and "100 scores ..." boxes and reduce space between them, since these are linked to each other 1-1.”
Author response:
We thank the editor for these suggestions. Figure 2 has now been modified accordingly.

9) Editor:
“Figure 2: innitialisation → initialisation”
Author response:
Correction made accordingly

10) Editor:
“L230: enabling to model → enabling the modeling of”
Author response:
Change made accordingly

11) Editor:
“L255: controlled by, firstly, → controlled primarily by”
Author response:
Change made accordingly

12) Editor:
“L256: N_till ← Remove italics from subscript here. Check throughout for multi-letter mathmatical subscripts, which should not be italic (e.g. L259).”
Author response:
Change made accordingly across the manuscript

13) Editor:
“L288: Please specify how Geothermal Heat Flux was prescribed outside of the PD land area.”
Author response:

We thank the editor for pointing this important point out. We have now added this sentence just after presenting the dataset to make this point clearer: "As this reconstruction does not feature geothermal heat flux data outside modern land areas, a constant value of 50 mW m^{-2}, the lowest values in the original dataset, is uniformly prescribed in ocean-covered regions (Fig. 1)."

14) Editor:

"Figure 4: I recommend two different color scales for annual and summer temperatures. Then it will be easier to see differences in summer, which are important. Also, perhaps you can saturate at 10 or 15 C, since this is already very warm, and largely irrelevant since it is over the ocean."

Author response:

To our opinion, keeping the same color scale for both annual and summer temperatures is important for the human eye to quickly compare panels displaying mean annual vs summer temperatures and make a quick qualitative comparison between the two. Only by having the same scale can we tell the true difference between these two. If ok, we would prefer not to apply the suggested changes to this figure.

15) Editor:

"L400: physical calving processes → the physical processes behind calving"

Author response:

Change made accordingly

15) Editor:

"L418: floatation → flotation [and elsewhere]"

Author response:

Change made accordingly

16) Editor:

"L483: SMB is forced ← Try to find more precise wording here."

Author response:

Changed to : "The SMB PDD model used here is forced with time-dependent fields of…"

17) Editor:

"L494-500: The glacial index approach you describe is not fully clear from the text alone. Perhaps you could include an equation, which shows which input data e.g. at 10 kyr BP are used to interpolate from 11 kyr BP and 9 kyr BP time slices that are available. Does this lead to a discontinuous index from 11-9 kyr BP and then 9-6 kyr BP, due to some kind of normalization happening for each time window, for example, or is the index continuous over the whole time range? The figure (Fig. 5) is nonetheless helpful in showing that reasonable forcing is obtained, but it would be helpful for others if the method is easily reproducible."

Author response:
In response to this comment, we have now added more detail to this methods section: including an equation to transparently inform the reader of the interpolation scheme used. This method does not lead to a discontinuous signal as the resulting signal features fields exactly equal to the fields obtained by iCESM-derived original data at each time slice when we have data. Here is the new text which hopefully makes this clearer:

"To create continuous forcing over remaining data gaps in time, we apply a glacial-index-type approach (Niu et al., 2019; Clark et al., 2022) and linearly scale our climate fields proportionally to variations in independent climate reconstructions in a space-dependent manner i.e. building an interpolation for each individual grid cell (Fig. 5). Between 24 and 21 kyr BP, we use surface air temperature and $\delta^{18}O$ reconstructions of Osman et al. (2021) to scale variations in temperature and precipitation fields, respectively. For data gaps between 21 kyr BP and the PI (*e.g.* 11 - 9 kyr BP), we use the seasonally-resolved Greenland-wide temperature and precipitation reconstruction of Buizert et al. (2018) as glacial index. The interpolation is computed as such: for each temporal gap in iCESM-derived data (e.g. 11–9 kyr BP), and for each grid cell, we construct a continuous forcing $A(t)$ between two iCESM-derived values C_1 and C_2 at times t_1 and t_2. This is achieved by scaling the linear interpolation between C_1 and C_2 with the relative excursions of an independent reconstruction $B(t)$ (e.g. data from Buizert et al., 2018) from its own linear trend:

$$A(t) = \left[C_1 + (C_2 - C_1)\frac{t - t_1}{t_2 - t_1}\right] \cdot \frac{B(t)}{B(t_1) + \left(B(t_2) - B(t_1)\right)\frac{t - t_1}{t_2 - t_1}}. \quad (8)$$

Note this method requires temperature units of Kelvin to avoid negative °C values causing interpolation distortions. As a result, we produce time-dependent, two-dimensional fields of mean annual and mean summer (JJA) surface air temperature and precipitation rate, continuous between 24 kyr BP and PI (Fig. 4-7). From mean annual and summer temperatures, our SMB model reads a cosine yearly cycle to generate a seasonality signal."

18) Editor:
"L519: "We however use linear interpolation rather than a glacial index scheme ..." ← Why? Please add clarification."
Author response:
We clarified by adding this detail: "Due to a lack of independent proxy data for ocean salinity, we however use linear interpolation rather than a glacial index scheme to bridge the temporal data-gaps in salinity data (Supplementary Fig. 1), … "

19) Editor:
"Figure 6: I am sure temperature is the more relevant forcing variable, but I am surprised

not to see the salinity forcing too. Since it is described, it would make sense to include panels here for completeness between Figs. 4-6."

Author response:

We thank the editor for this comment. We agree showing the salinity data somewhere would be more appropriate. However, the paper is already very long and presents a large number of complex multi-panel figures. Since salinity forcing is much less impactful than temperature and not further mentioned other than in the methods: we decided to include it but in the Supplementary materials: where we added (supplementary figure 1) a new figure presenting sea-surface salinity time series from the same locations as for SST data presented in figure 6: with a reference to these data and supplementary figure added to the caption of figure 6: to guide readers.

20) Editor:

"L562: use present-day GrIS thickness → start from present-day GrIS thickness and bedrock topopgraphy"

Author response:

Change made accordingly

21) Editor:

"L563: parameterisations → the parameterisations"

Author response:

Change made accordingly

22) Editor:

"L572: spun up GrIS → spun-up GrIS"

Author response:

Change made accordingly

23) Editor:

"*L576: equilibrium with parameterisations is likely reached ← This is something that can and should be checked. Please confirm and state something with more confidence if accurate."*

Author response:

Yes indeed, the fact the model output metrics diverge notably between ensemble simulations before 23 kyr BP provides evidence for this. We thus added this additional information to the text to support this: "Although adjusting parameters in subsequent transient runs can generate instabilities in the first simulation years, equilibrium with parameterisations is reached within the first centuries, as evidenced by model outputs (e.g. GrIS volume) from different ensemble runs diverging notably prior to 23 kyr BP, and should thus not markedly affect the modelled lLGM or deglacial dynamics."

24) Editor:
"L585: perturbate → generate"
Author response:
Change made accordingly

25) Editor:
"L590: we → We"
Author response:
Change made accordingly

26) Editor:
"L592: permitting to continuously alter → which permits continuously altering"
Author response:
Change made accordingly

27) Editor:
"L596: to → To"
Author response:
Change made accordingly

28) Editor:
"L606: preliminary → Preliminary"
Author response:
Change made accordingly

29) Editor:
"L611: paleo-climate data → Paleo-climate simulations"
Author response:
Change made accordingly

30) Editor:
"L612: paleo-ice-sheet forcings displaying inaccurate geometries → possibly inaccurate geometry of the paleo-ice-sheet boundary conditions"
Author response:
Change made accordingly

31) Editor
"L613: these → this"
Author response:
Change made accordingly

32) Editor

"L668: ice shelf extent → ice-shelf extent"

Author response:

Change made accordingly

33) Editor

"*L704: "<19800 misfit pixels" ← This value in pixels is very specific to your model setup. Could you give additional information, as to what area in km^2 of misfit that is, or as % of total area, or both? Something to ensure that others could understand this metric, and even translate it into their own model setup, from a physical basis*."

Author response:

We thank the editor for this comment. We have now added the corresponding value in km^2 for a more tangible representation of this metric: "Of these, only simulations with an extent misfit area $< 495 \times 10^3$ km^2 (i.e.<19800 misfit pixels) at the *Pre-Industrial extent* test, are retained."

34) Editor

"Figure 9: worse → worst"

Author response:

Change made accordingly

35) Editor

"L850: (Figs. 13, 16). → (Figs. 13, 16), respectively."

Author response:

Change made accordingly

36) Editor

"L944-947: I think that you should remove the statement "display an increasing trend with time". It is not well justified (see comment below on Figure 18), nor is it particularly important. The relationship with resolution is worthwhile, as well as the further discussion below about why some old simulations gave low values."

Author response:

We thank the editor for this comment. This part of the sentence was removed accordingly.

37) Editor

"Figure 10, caption: floatation → flotation"

Author response:

Change made accordingly

38) Editor
"L1076: ice divide → ice divides"
Author response:
Change made accordingly

39) Editor
"L1138: upflow → upstream"
Author response:
Change made accordingly

40) Editor
"L1207: covers → cover"
Author response:
Change made accordingly

41) Editor
"L1345: modelling margin → the ice margin"
Author response:
Change made accordingly

42) Editor
"L1353: model → produce"
Author response:
Change made accordingly

43) Editor
"L1374: model → produce"
Author response:
Change made accordingly

44) Editor
"*Figure 18: I find little value of the main panel (volume versus year of publication). There are likely obvious reasons for pattern, partly discussed in your paragraph above (L940-954). Most critically, the 2005 and earlier studies, which give the lowest estimates, were most likely SIA-only models and were surely limited to the land-area above sea level (using the so-called "ocean kill" parameterization). If you ignore these simulations with a known bias, and your own study, there is no discernible trend. Therefore I suggest removing the trend line entirely. I find the plot of model resolution versus volume potentially much more interesting, though again the models at 50 and 40km resolution could again likely cannot be put in the same group due to lacking physics. I would*

propose that you make this figure into two panels of equal size, using the same coloring for dots in both panels associated with the legend. Furthermore, please ensure the legend lists publications in chronological order. Finally, try to change the symbols of Zweck and Huybrechts (2005) and Quiquet et al (2021), since the "x" qualitatively much different than the rest. Could you make "SIA-only" models be triangles and models that allow full glaciation as circles, for example? Also, modify panel title since "previously-published" is not accurate (this study is included) - perhaps this title can be removed entirely."

Author response:
We thank the editor for this comment. Following these, we have now modified Figure 18: splitting it into two panels of equal size: one comparing LGM GrIS volumes against model resolution and one against year of publication. We also removed the linear regression trend line of relationship with year of publication, and only kept the one for model resolution correlation. We modified the symbols of Quiquet et al (2021) and Zweck and Huybrechts (2005) to not use the "x". We added a dashed circle that enables to highlight the models with SIA only, and grounded ice only: on the right-hand panel. We removed the statement of the correlation with time from the figure caption, and instead only focus on the correlation with model resolution. We ordered the legend in chronological order, furthermore.

We agree with the point made that model physics complexity is a big factor and that all models are not in the same category: if not considering the ones before 2005 there would be not relationship. However, this is partly our purpose with this comparison using the year of publication: i.e.: with time, ice sheet models have become more complex, which includes the representation of more complex ice-flow physics, and this has led to higher GrIS volumes in simulations. That in itself is an interesting observation we believe. Also: the point made above is not always verified: for instance, the Quiquet et al., 2021 model is Grizzly: a hybrid ice-sheet model (SIA/SSA) with floating ice included and similar physics-order as PISM: however their resolution of 40 km is associated with very low reported LGM volume of ~2.6 m SLE.

45) Editor
"L1532: models → produces"
Author response:
Change made accordingly

46) Editor
"L1541: whilst → while"
Author response:
Change made accordingly

47) Editor
"L1727: Investigations → Studies"
Author response:

Change made accordingly

48) Editor

"L1770: global → the global"

Author response:

Change made accordingly

49) Editor

"L1994, L1996, L2000: precipitations → precipitation"

Author response:

Changes made accordingly

50) Editor

"L1998: ice extent overestimations → ice-extent overestimation"

Author response:

Change made accordingly

51) Editor

"L1999: has been shown to also → has also been shown to"

Author response:

Change made accordingly

52) Editor

"L2000: overestimations → overestimation"

Author response:

Change made accordingly

53) Editor

"L2179: enable to accurately capture → enable accurately capturing"

Author response:

Change made accordingly

54) Editor

"L2182: bias accumulations → biases"

Author response:

Change made accordingly