Final response to review #2

This work presents a set of steady-state numerical simulations of the Patagonian Ice Sheet (PIS) during the Last Glacial Maximum (LGM). The applied model is the well-known polythermal ice-sheet model SICOPOLIS, which has been forced by a set of climatic products from the PMIP3 and PMIP4 experiments. The results show that none of the considered forcings are able to build proper ice extent in the northern part of the PIS, in contrast to geological evidence, while there is a tendency to overestimate ice growth in the south. The authors attribute this mismatch mainly to the low resolution of PMIP climate models, which hampers a correct representation of the atmospheric processes and dynamics over the complex topography in Patagonia.

Paleoclimate modelling studies applied to the PIS are certainly scarce compared to the abundance of comparable studies for polar regions. However they have gained recent attention with more papers coming out (e.g. see Yan et al., 2022 but also Cuzzone et al., 2023 in review in TC). The motivation behind this new work is well supported by the recent advance in ice-flow and climate modelling as well as by new information on ice sheet area based on geological evidence (Davies et al., 2020), bringing further understanding of the glacial state /deglaciation of the PIS. However, this work comes after Yan et al., 2022 (LGM steady-state PIS simulations) and mostly at the same time as Cuzzone et al., 2023 (deglaciation simulations of the northern sector of PIS) and it seems it struggles to offer a convincing motivation to be considered as a substantial contribution to the scientific knowledge.

LGM equilibrium numerical simulations of the PIS are already tackled in Yan et al paper, with a higher spatial resolution (1 km), with a more thorough methodology, 21 PMIP model applied, with additional sensitivity tests both on temperature and precipitation as well as on the choice of some model parameters (PDD factors, enhancement factor for SIA and basal friction law exponent).

Response: The publication of Yan et al. (2022) was indeed a shattering experience for our most junior author, since it completely destroyed his manuscript (submission-ready) based on 18 months of work on his diploma thesis defended in 2021 that is not only closely overlapping with the PMIP assessment presented by Yan et al. (2022) but even with their sensitivity tests (Retamal et al., 2022). Thus, we agree that it was difficult to distance this article from that of Yan et al. (2022) without completely redesigning it, which is what we are doing now.

In comparison to Yan's paper, the authors here "provide potential reasons for discrepancies" between modelled and reconstructed ice growth at the LGM associated to PMIP forcings, basically based on low model spatial resolution and unresolved topography. However I think the analysis they provide in the submitted version of the manuscript might not be sufficient to consider this work as a substantial novelty to Yan et al., paper.

Response: To address the concerns of both reviewers, we have enhanced the novelty of this study by refocusing it on long-term transient experiments spanning the last 70 thousand years. In particular, we are testing the reconstructed glacial histories of Patagonia during Marine Isotope Stages (MIS) 4 to 2 derived from regional offshore paleoclimate records against experiments driven by Antarctic ice core records to establish whether local records enable a better fit with the early deglaciation inferred from geological evidence and to tease out differences between local, hemispheric and global variations in the climate and glacial systems. To our knowledge, this has not been done until now and will thus enable new understanding of potential reasons for the asynchrony between the deglaciation modes in the Northern and Southern Hemispheres.

The conclusion “all the ensemble members driven by PMIP products are not able to reproduce the reconstructed ice cover in the northern part of Patagonia” could be already seen in Yan et al., 2022. Still, with a different model setup, they can grow more ice in the north compared to this work (although still underestimating Davies et al., 2020), showing that the conclusion of this paper cannot be drawn that straightforwardly. I still believe that PMIP products provide a bad performance mainly due to the oversimplified terrain as a forcing, which instead is extremely rough in Patagonia, and thus not being able to capture the complex surface atmospheric dynamics shaped by the Andes, but the authors should clearly prove this, tackling other possible sources of mismatch: for example how are their results influenced by the choice of the present climatology and its spatial resolution (ERA5), by the spatial ice-sheet model resolution (8 km), and - less - by the choice of key ice-sheet model parameters (PDD factors, basal friction coefficients)? Following this direction, I strongly suggest the authors to include at least sensitivity tests on the applied present reference climatologies (e.g. ERA5 and CR2MET - with 5 km res), and using a higher spatial resolution for the ice sheet model domain (at least 4 km). By increasing the resolution of both ice sheet model and climate products, despite the PMIP having still a very low resolution, the authors should be able to pinpoint the cause of the bad performance in the north more clearly. Otherwise one could say that the inability of building ice there might be also due to the low spatial resolution of the ice sheet domain, or to the present climatology applied to the anomaly method. Discarding other possible sources of mismatch could help to demonstrate that the unresolved complex topography used to force PMIP climate models is one of the main causes of model-data discrepancy in terms of ice sheet extent.

Response: We are very grateful to the reviewer for their very helpful suggestions and time invested into possible ways to salvage this manuscript. Due to the general shift in our focus from the LGM to the MIS 4-2 glaciation history, many of our conclusions have changed and the robustness of the study design has hopefully improved. Here we have systematised our responses to different aspects of the suggested improvements, while keeping in mind that some of them might have become obsolete.

Present reference climatology: Following the suggestion of reviewer #1, we have omitted the use of present-day climatology as a possible source of biases and artefacts. Instead, we are now directly forcing our simulations with PMIP model outputs. Please, see our response to the related comment in review 1.

Sensitivity experiments: We have also designed an ensemble of additional simulations to explore the sensitivity of the ice sheet configuration and dynamics to different parameter choices related to climate and ice dynamics. Preliminary results of these sensitivity analyses suggest that it is difficult to achieve a good fit between PMIP-driven ice sheet model simulations and geological evidence in the north under the newly adopted forcing strategy if we keep model parameters within realistic ranges. Even though some parameter setups allow for a growth of the ice sheet in the north, they also produce unrealistic expansion of the PIS in the east.

Resolution: Following the reviewer's suggestion, we have adopted a four-fold increase in the spatial resolution in our simulations (from 8 to 4 km). Due to the change in the scope of this research and the consequent computational demands associated with 70-thousand-year long transient experiments, we consider using an even higher spatial resolution unfeasible. However, we believe that this refinement of the spatial resolution will allow us to evaluate the effects of better-resolved topography on the ice sheet inception in northern Patagonia.

LGM state and transient deglaciation simulations are tackled by Cuzzone et al. 2023, although only for the northern part of the PIS. They used the climatology from the Trace21-ka experiment, and it performed quite well, although the climate spatial resolution is lower (3.75°), but perhaps based on a better resolved topography (ICE 5G if I am not wrong) (and different experimental design). I would be curious to see how the Trace21-ka product for the LGM performs for this work. If results improved with Trace21-ka (better performance despite the low resolution), the conclusion stating "We largely attribute these discrepancies between the model-based ice geometries and geological evidence to the low resolution of paleoclimate models" would lose significance and causes of the mismatch should be searched elsewhere.

Response: We would like to thank the reviewer for their recommendation to include the experiment with Trace21-ka. An extensive discussion of its performance based on the anomaly method has been addressed by Cuzzone et al. (in review) and with the right parameter choice and use of CR2Met we would be able to closely match their results. However, we do agree with the reviewer #1 that it is not the fairest way to test GCM outputs. Thus, we have put the 21 ka slice of Trace21-ka to a test by directly forcing our simulations with LGM climatologies and then comparing the modelling outcomes with other forcings. Thus, we will include the GCM outputs of Trace21-ka to test their performance at the global LGM against the geologically constrained extents of the former PIS. The results of this test will surprise everyone. A further discussion of the attribution of the discrepancies between the model-based ice geometries and geological evidence to the low resolution of paleoclimate models will be addressed in the revised manuscript.

Another suggestion to make this work more appealing could be developing more aspects of the manuscript that are discussed but not analysed. For example, the authors discuss extensively about the limitations in approximating the LGM in Patagonia from the global LGM (~21 ka) despite evidence of asynchronous glacial maximum. To overcome this drawback they could think about a a methodology to correct the PMIP climatologies to take into account this spatial and temporal heterogeneity (maybe performing transient simulations between ~35 kyr and ~18 kyr using a climatic index?). This suggestion goes in the direction of the other reviewer's comment. Also, I think there are now available other PMIP4 LGM climate products from other institutions (IPSL, NCAR). These could be added to the analysis.

Response: We agree with the reviewer that both the novelty of this study and its experimental design would benefit from expanding our analysis from steady-state to transient simulations in order to capture the early deglaciation of Patagonia. As suggested, our new long-term transient simulations are forced by the glacial index method using regional offshore sediment records and Antarctic ice core records throughout the last 70 thousand years (Marine Isotope Stages (MIS) 4 to 2). In addition, new PMIP 4 and Trace-21ka climate products will be included in the core experiments.

Regarding the work presentation quality, I think that generally the manuscript is well written and clear in most of its parts, besides some sentences that need clarifications and figures that might be added and others modified.

Besides the major concerns I expressed above, here I note down some general and specific comments to the manuscript.

Overall, we are very grateful to the reviewer for taking the time to propose improvements in the experimental design and for the helpful and constructive comments aiming to improve the quality and significance of our work. As it is clear from the following, we have addressed all the comments as long as they have not become obsolete due to change of the manuscript's focus. Our responses are written in blue.

General comments:

Model set-up

I strongly suggest the authors to include more information about the model setup, such as the basal friction law, the applied oceanic forcing (if any?), how ice shelves and grounding line migration are treated. Results of modelled ice dynamics should be better described (there are almost no comments on this in the manuscript): e.g. are ice streams modelled? Where? How are the velocities distributed? Basal stress plays an important role on the ice-sheet capability to advance, therefore having an impact on the area that is glaciated. Some comment on this is required, maybe presenting sensitivity tests on the basal friction coefficient and/or law exponent. Is the basal hydrology taken into account? If yes, how? I also suggest to add a figure that shows the spatial distribution of the horizontal velocity.

Response: We thank the reviewer for pointing this out. The revised manuscript will include a thorough description of every model component relevant to the updated methodology. Following the reviewer's suggestion, we will also present additional simulations and figures to explore the sensitivity of the ice sheet configuration and dynamics to different parameter choices related to climate and ice dynamics. For completeness, here we provide a set of brief answers to the individual questions in the reviewer's comment. We use a Weertman-type power law to enable sliding at the base of the ice at locations where the base is close to its local pressure melting point, as described in Sato and Greve (2012, Eq. 5-6). In this sliding law, basal hydrology contributes to a reduction of the overburden pressure exerted by the ice column at any given location. This contribution depends on the difference between the local bedrock elevation and sea level, similar to the approach in Martin et al. (2011). Grounded ice is allowed to advance until the coast, beyond which any further advance into the ocean is prevented, i.e., we do not allow for the formation of ice shelves. Therefore, potential effects caused by the ocean thermal forcing are not included. Since we utilise a hybrid combination of the shallow ice and shallow shelf approximations, ice streams can form. However, as the reviewer mentioned, whether or not our model results showcase ice stream features will depend on the poorly constrained friction conditions at the base of the ice sheet. These considerations will be added to the discussion in the revised manuscript.

Ice-sheet model and climate model spatial resolution

As stated before, I think one cannot really prove that the main cause of the LGM data-model mismatch is due to the poor resolution of paleoclimate models as long as the spatial resolution of the ice sheet model itself is too coarse (8 km). This applies also to the applied reference climatology (ERA5). I therefore suggest to increase the spatial resolution of SICOPOLIS to at least 4 km (and possibly to try out a modern climate with a higher resolution such as CR2MET) to better capture the interplay between topography and temperature and precipitation.

Response: Following the reviewer's advice, in the revised manuscript we will present simulations at a horizontal grid resolution of 4 km to test whether this has an impact on the inception and advance of the ice sheet in the northern sector of the PIS. Due to Reviewer 1's request to replace our anomaly approach with a direct method, we have excluded the anomaly method (fusing GCM outputs with present-day climatology) making this proposal to include CR2Met obsolete. Nevertheless, we will add a discussion that takes into account this data set, focusing on other factors (besides GCM resolution) that could explain the mismatch between modelled and reconstructed extents of the PIS.

Impact of climate forcing on area change

To me the manuscript lacks a clear figure showing how the glaciated area changes with respect to temperature and precipitation thresholds, as inferred from climate outputs for different latitude ranges. Figure 3 provides an interesting perspective on climate specifications at various latitudes, but it is very difficult to estimate from this figure which are the minimum conditions in terms of temperature and precipitation required for ice growth over a certain region. I would like to see a clear figure (maybe a scatterplot) that shows area change versus model precipitation and temperature averaged over a certain latitude interval. This information could also be stored in Figure 3 somehow, so that the models that satisfy the required ice sheet advance are indicated with a different symbol, maybe. Also, how are these thresholds established? I would suggest to think about a more thorough mathematical description to identify such temperature and precipitation thresholds. This could simply be the relative error between the modelled and the reconstructed area being lower than an error bound (e.g. 20%), or the ratio between the number of grids cells where ice is both (or neither) modelled and reconstructed and the total number of grid cells, being higher than a certain value (e.g. the temperature and/or precipitation that ensures that 80% of cells are in agreement with data corresponds to the minimum threshold).

Response: We thank the reviewer for the great ideas on how to analyse the sensitivity of ice sector areas to the temperature versus precipitation forcings. Additional maps with the temperature and precipitation conditions will be included in the revised manuscript. We will also perform an analysis of area changes under different temperatures and precipitation rates for each of the latitude intervals studied, highlighting the conditions that allow ice to grow, and comparing them against the geological reconstruction. The thresholds have been established following the climate conditions that allowed the ice inception and the consequent build-up, however, we agree with your suggestion to explore these in a more mathematical manner and we will provide a mathematical definition to identify these thresholds in the revised manuscript.

Clarity of the results

As I wrote before, I think the authors overall do a good job in describing and discussing the results. Still, I see quite confusing the fact that the investigated latitude ranges change over the manuscript: sometimes is the 38-39°S and the 40-42°S, sometimes is the 38-42°S, sometimes is “below 44°S”. I think a more homogeneous description would be beneficial for the paper.

Response: We have gone through the manuscript and have indeed noticed the lack of systematics with respect to geography. We will rectify this in the revised manuscript.

Specific comments:

P1 L11-12, but also P20 L409-410 and within the text in several paragraphs. As I wrote in my general comments I think it is difficult to prove that a principal source of mismatch between model and data is due to the low resolution of paleoclimatic models, as long as the low resolution of the ice-sheet domain and the present day climate also contribute to smooth the climate gradients over the complex topography. Please refer to my comments above.

As mentioned in our responses above, and to assess the influence of the ice-sheet model resolution on our results, the revised manuscript will present a new set of simulations that introduce a four-fold increase in the horizontal grid resolution. This choice represents a compromise between the computational feasibility, a new focus on transient simulations and the enhanced focus following the suggestions of Reviewer #1 related to the shortcomings of the anomaly method used to derive the climate forcing in the original version of the manuscript. Since our experiments will now use the direct output from the PMIP models, the uncertainties associated with the choice and resolution of the present-day climate have been hopefully minimized.

P2 L26. "...consequently global sea level dropped to 120-134 m…", of course the sea level drop from all vanished ice sheet doesn’t sum up to 120-134 m because of the contribution from AIS and GrIS that here is not described. Please clarify this.

Here we mean that according to current estimates different ice masses contained around 113.9 m of sea level equivalent at the LGM. This estimate includes different ice masses such as: the North American ice sheet complex, the Eurasian ice sheet complex, the Antarctic and Greenland ice sheets, and smaller ice caps. This statement will be reformulated for clarity in the revised version of the manuscript

P2 L29-30. Unclear description, please change to something like “The latter triggered a lowering of the global mean surface air temperature by 3.2°C to 6.7°C with respect to the preindustrial level…”.

Agreed. We will follow the reviewer’s suggestion in the revised manuscript.

P2 L35-42. Why do you describe in detail the asynchronous occurrence of the glacial maximum when you actually consider the PIS as in a steady state during the global LGM (~21 ka)? Please refer to my comment above about possible transient simulations.

We have included transient simulations of the PIS spanning the last 70 thousand years, as mentioned in our responses to the general comments above. These long transient simulations will be forced by the PMIP models that provide the best fit to the reconstructed PIS extents in the updated steady-state simulations that are driven by direct climate forcing from PMIP.

P2 L42. Please rephrase “zooming in on the global climate…”

Here we mean that “This approach focuses on examining the global climate that exhibited the closest approximation to equilibrium during the last glacial cycle”. We will use this formulation for clarity.

P3 L56. Which PMIP outputs? Phase?

Yan et al. (2022) used the temperature and precipitation from 21 PMIP model from the phases 2, 3 and 4. We will clarify this in the text.

P3 L59-63. First, I really don’t see that PMIP4 models perform better than PMIP3 in Yan et al., 2022; please consider a reformulation. Second, if PMIP3 clearly performs worse than PMIP4 from Yan et al., 2022 paper, then why do you also investigate PIMP3 climate outputs? Third, Yan et al., 2022 look in total to 21 model products from PMIP2 to PMIP4, you should write that.

Using the anomaly method, fusing the PMIP models with Word-Clim, Yan et al. (2022, Figure 13) showed that PMIP4 models tend to have a better agreement with the PATICE reconstruction for 20 ka in detriment of PMIP3, both overestimating the covered area in a similar percentage. Now, since the focus has shifted to forcing our simulations by the direct method (not the anomaly methods), we have decided to keep them in our study to address their performance under this different approach. Suggested changes regarding the number of models used by Yan et al. (2022), as well as the phases considered will be addressed in the manuscript.

P3. Are there other previous model experiments/reconstructions besides what you described here? You could mention for instance the new PIS thickness and volume reconstruction from a perfect plasticity assumption from Wolff et al., 2023. This is actually an interesting paper, that could also be mentioned in the discussion to compare your results against, as it shows a PIS further extended to the north (almost to 36°S, as based on an early reconstruction from Hubbard et al., 2005) with respect to Davies et al., 2020.

The most up-to-date reconstruction of the PIS at the local LGM and its deglaciation has been published by Davies et al., (2020). However, several works were previously conducted towards refining the understanding of the ice extent and the dynamics (i.e., Hulton et al., 2002; Sudgen et al., 2002) and more recently on a glacial chronology of specific sectors of the PIS (i.e., Leger et al. 2021; García et al., 2021; Soteres et al., 2022). We would like to thank the reviewer for highlighting the recent publication from Wolff et al. (2023). Indeed, this is an interesting paper that, in line with our work, emphasises the necessity of addressing the performance of the paleoclimate models towards the northern sector of the PIS which might have reached even more northern than previously proposed (Davies et al., 2020). A discussion on this matter will be included in the revised manuscript.

P5 L85-96 and Table1. How are the PDD factors chosen? Are they calibrated for the Patagonian region at the present or simply taken from the literature?

Due to the scarcity of studies focused on paleo ice sheet modelling in Patagonia, the PDD factors chosen for the experiments presented in our first version of the manuscript are roughly based on present-day and paleoclimate studies of other ice sheets (Bernales et al., 2017a,b; Lofverstrom et al., 2018; Seguinot et al., 2018, 2021; Niu et al., 2019; Mas e Braga, 2021) which do not deviate much from values previously used in ice masses in Patagonia (Möller and Schneider, 2008; Fernández et al., 2016; Bown et al., 2019). We do believe that observations and modelling studies of the present-day ice sheets provide a rich ground for the studies of the past ice sheets since their dynamics are constrained by observations. However, as mentioned above, we will include a discussion of the sensitivity of our model results to the choice of the climate factors on the resulting geometry of the Patagonian ice sheet.

P5 L98. Are ice shelves allowed to grow in this model setup? How are the other key model parameterisations considered (basal friction, basal hydrology, enhancement factor, …)? Please refer to my general comment.

As stated in our response to the general comment "Model set-up" above, the revised manuscript will include a detailed description of all model choices.

P6 L103. "During phase 3 and 4 of PMIP" please specify which is the time considered (~21 ka).

Corrected. Included: "that participated in the LGM experiments during phases 3 and 4 of PMIP for ~21 ka".

P7 L122. "…relative to the PATICE reconstruction" please add "Figure 1".

Corrected.

P7 L126. Change Fig 2 k, l to Fig 2 k,m.

Corrected.

P8. As I mentioned in the general comment, sections 3.1, 3.2 and 3.3 should describe the same latitudinal intervals as in Figure 3 c-f for consistency. Also Section 3.2 could be split into 38-42°S, 42-44°S and 44°S-52°S to better describe the performance in the northernmost part.

In the initial version of the manuscript, the chosen latitude bands were established to highlight the zones that better describe different performances of climate models in our simulations. We thank the reviewer for the suggestions to subdivide the study zone into 3 major sectors. In the revised version of the manuscript, a more consistent subdivision of the latitudinal bands will be implemented.

P8 L151. "Performance of these models north of 44°S", do you mean north of 44°S but south of 42°S?

Here we mean in the full study zone, until 38ºS. We apologise for the inconsistencies in the description. Changes have been made accordingly in the revised manuscript.

P8 L153. "The resulting ice sheet temperature" should be "the resulting ice sheet extent", I think.

Thanks for pointing out this typo. Corrected

P8 L153-155. These cited thresholds should be argued with a more precise definition, e.g. considering the relative error of the glaciated area and with a figure showing how the area changes when these minimum conditions are met (see my comments above).

This will be implemented in the revised version of the manuscript. Please, see our response to your comments on: “Impact of climate forcing on area change” above.

P9 L173-176. What do you mean by “northernmost margin”? 38-40°S? What do you mean with “In this part of Patagonia”? Why MPI-ESM1-2-LR “THUS stand out as the only PMIP products providing…”? You should describe better the climatic condition of MPI-ESM1-2-LR as you did with INM-CM4-8.

In this sentence, we refer to the region between 38-40ºS. Meanwhile, when we stated “In this part of Patagonia” we are indicating the area between 40 and 42ºS. Thank you for bringing these inconsistencies to our attention. They will be addressed and clarified in the revised manuscript. Moreover, we will enhance the description of the best-fit models in the comparison with the geological reconstruction based on the shifted focus of our manuscript.

P9 L181-184. Again, I struggle to see this from Fig. 3. Also, this is a repetition of P8 L153-155. Finally, is this threshold computed for the region 42-44°S or for north of 42°S or for what? Please clarify.

Corrected

P11 L209. “38-42°S” should be consistent to the sectors described in the results and in Figure 3 c-f.

Corrected

P9 L220. Please change 4°C to -4 °C.

Corrected

P9 L221. Add a closing bracket to Fig. 5.

Corrected

P13 L240-241. “The cooling of ~12 °C observed in INM…during summer months…” I don’t really see this clearly. Maybe Figure 5 should be for 40-42°S or think about producing another figure to be consistent to latitude sections of Fig 3 c-f.

As Figure 3 c-f will be changed to 38-42°S, Figure 5 will be consistent. However, the cooling of ~12 °C in INM is observed during January and February (-12.2 and -11.6 °C respectively), and the value for December is closer to -11 °C (-10.8 °C). We will rephrase the first sentence in the updated version.

P14 L244. “Infers a value of around -8°C”, which value? Annual temperature at 40-42°S? I don’t see this from Figure 3d (to me the temperature anomaly is more around ~11 °C). Maybe the 8°C anomaly is calculated at 38-42°S?

We referred here to inferring an annual mean temperature of around -8 °C, which overestimates the reconstructed cooling by 1 °C (Fig. 5a). This will be corrected in the new version of the manuscript following the aforementioned shift of focus.

P14 L245-246. Please refer to the figure where we can see this.

Corrected. Figure 3. Additional maps with the temperature and precipitation have been included.

P14 L254-255. “To achieve a good fit with geological evidence, the PDD factors in the SMB were reduced to promote ice sheet growth”. To my understanding this is not true. As I get from Yan et al., 2022 paper, the PDD factors were tuned to reproduce modern glacier geometries. Please rephrase.

What we mean is that to achieve a good fit with the glacier geometries, the PDD factors in the surface mass balance (SMB) model were tuned to values that are lower than what the literature commonly suggests. This will be reformulated in the revised version of the manuscript.

P15 L257. “This evaluation may be biased due to the choice of model parameters”. I would be careful here: they did indeed a sensitivity study to show how the results are affected by the choice of PDD factors, so you cannot really say that the results are biased.

The model can be biased due to SMB parameters chosen for this specific model configuration using Worldclim. Cuzzone et al. (In rev) showed a better north extension with CR2MET and higher ablation on the SMB.

P15 L259-263. I think this is really difficult to say since their PDD factor for ice (4 mm/d/°C) is very close to yours (3 mm/d/°C). True, the PDD factor for snow is lower than yours to reduce ablation, but their sensitivity test shows that it doesn’t impact significantly the total modelled ice area.

Yan et al. (2022) have proposed a strong sensitivity to the area coverage associated with the PDD factor of the ice, which is slightly higher than the one we have implemented in this study. However, they did not explore the sensitivity to the standard deviation of surface air temperature which is an important parameter of the SMB, e.g. Fausto et al. (2009) demonstrated that an increase of the standard deviation from 2.5°C to 4.5°C results in a 33% increase in the modelled melt area over Greenland. In our experiments we used a standard deviation of 3 °C, while Yan et al. (2022) used 2 °C, reducing melting. However. we acknowledge the concern in this matter and this statement will be reformulated in the revised version of the manuscript.

P15 L273: do you mean “topography” instead of “forcings”? Also could you clarify for which models/PMIP phase the ICE-6G and GLAC 1D reconstructions were used?

Here we mean topography. Models from PMIP3 use the composite means of three ice sheet reconstructions; ICE-6G v2, GLAC-1a and ANU (Abe-Ouchi et al., 2015). Models from PMIP4 use the ice sheet reconstruction from ICE-6G_C (Peltier et al., 2015). This will be better described in the revised manuscript.

P15 L285. I don’t understand “has undergone visible substantial modifications”. Please rephrase.

Here we mean that “has been significantly simplified and flattened”. We have modified this in the revised manuscript to avoid confusion.

P17 L309. Please add “Figure 1”.

Agreed.

P18 L338. Please change “between 700 and 800 (Fig. 7a) m” to “between 700 and 800 m (Fig. 7a)”.

Corrected.

P18-19 Section 4.3. I see this section interesting to be discussed but it is also somehow a reasoning for the sake of it, as it is put at the end of a whole work based on the assumption of a steady-state condition during the LGM. Perhaps this discussion would gain more interest if you consider investigating the local LGM depending on the latitude through a series of transient LGM runs (see general comments).

As mentioned before, using the glacial index method, transient simulations based on the best-fitted models for the LGM will be included and discussed in the revised manuscript.

Figures

Figure 1: Please change the colour for the present day ice fields, LGM reconstruction and coastal lines to a list of colours that clearly differ from the topography colour palette (e.g. orange, red, magenta…).

Agreed.

Figure 2: Could you show where are the ice shelves located (if there are any?). Also, instead of showing the velocity streamlines I would add a figure showing the 2D velocity fields. Where are the ice streams?

Agreed. A figure with the velocity field will be added to the revised manuscript.

Figure 3: as pointed out already above, I see figure 3 c-f quite confusing: 1) I would add the values of the x axis to all subplots; 2) I cannot clearly see which are the models from PMIP4 and which are from PMIP3; 3) where is the subplot for latitudes 42-46°S? 4) which are the models that allow ice growth as expected from PATICE? (I would mark them somehow in the plot) 5) I am missing a scatterplot of area change versus temperature and precipitation anomaly (see my general comment) to clearly see which are the climatic thresholds that satisfy ice growth in the north.

Agreed. Suggested changes in the figure will be implemented. The analysis in the mentioned latitudes will be added in the revised manuscript. Additionally, an analysis based on the match of the mask against the PATICE reconstruction will be included.

Figure 7: as in figure 3, why latitudes 42-46°S are missing?

Agreed. The mentioned latitudes will be included in the figure and in the analysis.

Also, could you add one figure showing the spatial variability of temperature, and another one for precipitation, for different PMIP climate models (panels like Fig 2). This should add more information to Figure 3, where, for instance, west-east precipitation gradients cannot be seen.

Corrected. Maps of the PMIP temperature and precipitation have been included.

References:

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