Final response to review #1

Castillo-Llarena and co-authors present numerical experiments to reconstruct the Patagonian ice sheet (PIS) at the last glacial maximum (LGM, 23-19 ka). They use an hybrid shallow ice / shelfy-stream model at 8 km resolution forced by PMIP outputs super-imposed to ERA5. They run 10 kyr under perpetual climate forcing to reach ice sheet equilibrium. Their major finding is that, using this methodology, the PMIP model ensemble does not reproduce the ice sheet extent inferred from geological evidence.

Numerical simulations of the Patagonian ice sheet are scarce, this is a nice motivation for this work. The methodology itself is sound, although not novel since it has been used multiple times, notably to simulate Northern Hemisphere ice sheets at the LGM. However, I have to admit that I am not convinced that it is appropriate in this case. The authors are trying to reproduce an ice field that is much smaller than the other LGM ice sheets (at max. 1/4th of present-day Greenland ice sheet). The topographic setup is also much more complex than in Greenland (for example) since the whole Patagonian ice sheet (PIS) is sitting over the Cordilleran mountains which display an extremely rough terrain. These lead to a spatially highly heterogeneous climate (altitude dependency, rain shadow effects etc.). The authors do not apply any specific corrections to the PMIP models (only a classical vertical lapse rate correction). Worse, they use ERA5 to get a "better" pre-industrial climate but in doing so they keep the imprint of ERA 5 (pretty much linked to present-day topography) at the LGM. With an ice sheet, the topography becomes necessarily much smoother so the complexities seen in ERA5 do no longer apply. The methodology used here has be followed for other much larger objects (e.g. Greenland), and even in this case it is generally accepted that the Eastern part of the Greenland ice sheet (which has a rough topography) is generally poorly represented with such "anomaly method". I think that one reason for the small available numerical reconstruction of this ice sheet in the literature is due to the fact that it is a complex thing to model since it requires an adequate climate downscaling.

Response: We totally agree with the reviewer that the commonly used strategy for the climate model downscaling in regional ice sheet and glacier studies that customarily involves a fusion between global climate model outputs and present-day climate reanalysis data (or alternative present-day datasets) is not entirely realistic and should be eventually replaced by the dynamic downscaling once this is feasible. It is however not the case for most areas in the world right now, at least within the scope of paleo research. Thus, as the reviewer points out, it is currently a standard way to handle paleoglacial modeling experiments that has been utilised in a multitude of published studies, not only focusing on large-scale ice sheets but also on ice caps and even mountain glaciers in Central Asia and elsewhere (i.e., Seguinot et al., 2014, 2016, 2018, 2021; Yan et al., 2018, 2023; Wei et al., 2023), but we also agree that the method proposed by the reviewer (to directly use PMIP outputs without fusion with the reanalysis) is less artificial and more fair when we evaluate the performance of different climate models. We have therefore adopted this fairer method in our new experimental design. We thank the reviewer for this suggestion because it did make the comparison of different climate models more straightforward and also removed potential artefacts that might have been introduced by the valley-scale features in the ERA5 data. In addition, given the substantial uncertainty in the present-day climate reanalysis datasets due to the lack of a robust network of ground-based observations that would allow for a thorough

validation and data assimilation across this region, we acknowledge that the use of present-day climate datasets might introduce further uncertainty in our analysis. We elaborate our response to this comment in the Major Comments section.

A second comment on spatial resolution is that the topography is very complex so we expect a LGM ice flow that displays strong horizontal gradients. The authors use a 8 km spatial resolution which seems very coarse given the spatial scales studied here. To my knowledge (even though I have not run the model in its latest revision), SICOPOLIS is not very demanding in terms of computational cost. If really the computing time is an issue I would suggest to reduce the vertical discretisation since 81 points in the ice (while SIA displays a relatively smooth profile) and 41 in the bedrock (where only temperature diffusion is solved!) seem a bit too much. Although I guess the computational cost is mostly linked to horizontal discretisation (since you might need to reduce the timestep to avoid numerical instabilities).

Response: Again, we agree with the reviewer that our experimental design would benefit from higher spatial resolution since the topographic forcing in the region is complex (even though not as complex as in other high-mountain regions that have been subjects of similar studies: Seguinot et al., 2014, 2016, 2018, 2021; Ziemen et al., 2016; Yan et al.,2018, 2023; Wei et al., 2023). Thus, we have adopted a four-fold increase in the spatial resolution of our simulations (from 8 km to 4 km). Since in our new experimental design, we have shifted the focus from steady-state to long-term transient simulations (spanning 70 thousand years), we deem a further increase in spatial resolution challenging.

In summary, my major comment is on spatial resolution and, above all, climate downscaling. The authors conclusion (PMIP inability to reproduce LGM PIS) was pretty much expected. This study appears after the one of Yan et al. (2022) but it is much less thorough. Yan et al. (2022) used also the same, potentially controversial, method but they also tested climate sensitivities (temperature and precipitation alone, PMIP anomalies), climate parameters (PDD factors) and ice dynamics (sliding coefficient). I agree that it is better to have different studies on a single scientific question but here it does not bring any new piece of information since the methodology is the same (here with a coarser resolution) but with less discussion on the PMIP outputs.

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 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 and hemispheric variations in the climate and glacial systems. 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 addition to the shift of focus from the LGM to the MIS 4-2 glaciation history, we also include

Major comments

1) Climate downscaling & ice sheet resolution

As explained in the introduction of this review and I am not convinced that the methodology is suited for the LGM PIS. In addition this study arrives later than Yan et al. (2022). For this reason some additional work should have been done to bring some kind of novelty, such as for example, a proper discussion of the limitation of the employed method. It is possible that no regional atmospheric model simulation of Patagonia at the LGM has been run (I am not familiar with the literature) but maybe the authors could have suggested a few possible alternatives? Also, even given the huge biases of the PMIP model, why not running the ice sheet model forced by the PMIP outputs directly with no anomaly (or maybe some domain-wise correction). Or perhaps the authors could have smoothed the ERA 5 climatology to be compatible with the PMIP model resolution before applying the anomaly. Keeping the imprint of ERA5 does not make sense to me. Also an ice sheet resolution of at least 2 km (preferably smaller) should have been employed given the rough topography.

We absolutely agree with the reviewer that potential imprints of ERA5 in the forcing may be harmful to the ice sheet modelling outcomes. As suggested by the reviewer, we have adopted the strategy of directly forcing our model experiments by PMIP model outputs. To estimate the impacts of the lapse rate on the ice nucleation at the valley scale, we have also performed experiments forced by the PMIP outputs without such corrections. We very much appreciate this proposal of the reviewer that has also enabled a compatibility between our steady-state and transient experiments where the design of external forcing should not build upon the fusion between paleo models and present-day reanalysis data. Furthermore, the meteorological dataset for the Andes CR2Met (Boisier et al., 2018) that is based on statistical downscaling using ERA5, meteorological records and satellite data indicates that lack of ground-based validation data in Patagonia limits the skill of reanalysis data in the region (Masiokas et al., 2020; Sauter, 2020). Therefore, after considering their assessments, we have decided to avoid introducing additional uncertainties arising from present-day climatology. As mentioned in our responses above, we have also introduced a four-fold increase in the spatial resolution of our experiments. 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.

2) Climate evaluation.

There is no map of temperature and precipitation under modern conditions nor for the LGM.

Is ERA5 good for the Patagonian region? Do you simulate the extent of present-day ice fields when using ERA5? If not, perhaps ERA5 needs also some correction. The LGM PMIP anomalies (January temperature and precip) should have been shown (in the supplement if too many maps). I liked the proxy data discussion but I found it too weak: where is the pollen-based data? Have you selected a model grid point (outside of the LGM ice mask used) close to this site? This could have been a novelty compared to Yan et al. (2022) to include a more detailed proxy-data comparison (but I am not familiar with this data so perhaps it is qualitative than quantitative).

As mentioned above and following the reviewer's suggestion, in the new version of the manuscript we have replaced the anomaly method that uses the ERA5 dataset by direct forcing from PMIP experiments. This is in part because we echo the concerns of the reviewer regarding the performance of ERA5 in Patagonia, to a large part due to the scarcity of direct constraints and ground-based validation datasets (Masiokas et al., 2020). In addition, recent analysis of the precipitation rates over the Patagonian ice fields indicates that ERA5 tends to inflate precipitation sums (Sauter et al., 2020).

There exist few proxies that allow a quantitative characterization of the past climate conditions of Patagonia (Kilian and Lamy, 2012), relying mainly on qualitative comparison (i.e. Mancilla et al., 2016; Moreno et al., 2020). Moreover, most of the existing proxy data in Patagonia are from the eastern side of the Andean climate divide. The majority of the data, both on the eastern and western sides, are limited to the global LGM (Harada et al., 2013: Aracena et al., 2015; Haddam et al., 2018). However, primarily relying on sediment cores, only a few studies extend their focus to the MIS 3 and 4 in lakes (Rocasens et al., 2012), near the coastal area (Kaiser et al., 2007; Caniupan et al., 2011), or further offshore (Ho et al., 2012), although with a low time resolution and in some cases excluding parts of the Late Holocene. As mentioned above, the shift of the focus of this study brings about an analysis of whether the local records allow a better fit with the early deglaciation inferred from geological evidence. Following reviewer´s suggestions, the revised manuscript will feature maps of temperature and precipitation forcings including the proxy data available in the region.

3) At several occasion in the text the authors mention the "chronology" of the PIS maximum.

I generally agree with the authors about non-synchronicity of climate change. However I don't think the present study brings any information about this. Instead of equilibrium simulations, it could have been possible to run transient ice model simulations using an index method (using an index representative of Southern Hemisphere temperature change). This would also have been a real improvement with respect to Yan et al. (2022).

Thank you very much for these suggestions; to better integrate the discussion of asynchronous deglaciation into the context of our study, we have now designed transient simulations. As suggested, our transient simulations are performed using the glacial index method with the best-fit PMIP models from the steady-state LGM experiments. Glacial index curves have been derived from both offshore sediment core and ice core records. Simulations start at 70 ka assuming ice-free conditions that are consistent with the minimum extension of the PIS proposed by Kaiser et al. (2007) and Gowan et al. (2021).

4) Glacial erosion / sediments.

There was large glacial erosion during the last glacial cycle. So it is a simplification to use present-day topography. This is not discussed here. In addition, testing the model response with a different topography map could have been a novelty with respect to Yan et al. (2022). However, the main control here of ice sheet extent is the climate forcing so it is a much minor comment than the other first two.

We agree that using present-day topography in paleo experiments represents a simplification. However, Yan et al. (2022) show small values of glacial erosion over most of the study zone. The largest values are focused on the southwestern part of Patagonia, adjacent to the Pacific Ocean. Additionally, Seguinot et al. (2021) indicated a maximum cumulative glacial erosion of 100 m over the Alps during the entire last glacial cycle. Our interpretation suggests that during recent deglaciation, Patagonia did not undergo such drastic changes that would prohibit the use of present-day topography. Nevertheless, we will raise this point in the discussion of the revised manuscript.

Minor comments

- I do not agree with your sentence on P1L16: at the LGM the topography is much less complex than for present-day conditions since the ice sheet is expected to drastically smooth the topography.

Here we mean that at the LGM the topography is less complex, and the then-existing topographic barrier is expected to be much larger due to the ice mass lying on top of the Andes. We have rephrased this for the revised version of the manuscript to make this clear.

- Sentence P2L48-50 seems a bit out of context here since mostly EMICs use interactive ice sheets and the authors mostly describe GCM models through PMIP experiments (with no interactive ice sheets).

We want to emphasise the significance of paleoclimate models as a key instrument for understanding climate feedbacks and their connections with, for example, ocean and ice sheet dynamics. In addition, by prescribing different ice sheet topographies, we can enhance our understanding of how these topographies impact the modelled climate. This will be rephrased in the revised manuscript.

- Sec. 2.1: dragging law used?

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.

- Sec. 2.2: how do you compute the anomalies? You should have put the LGM outputs and the PI outputs to the same topography (since the GCM sees different topographies). You mention the lapse rate on the previous section, is it also used to compute the PMIP anomalies? What has been done to precipitation do you account for the drying linked to temperature decrease?

LGM-PI temperature anomalies are computed using a lapse rate of 6.5 ºC/km. Topographic differences were calculated based on their respective orographies. LGM/PI precipitation anomalies were corrected with a factor of 7.3% per degree Celsius.

- Sec. 2.2: show ERA 5 January temperature and annual precipitation. If there is a strong correlation with topography (which I expect) then the anomaly method is not suited (since the topography is much flatter at the LGM given the presence of the ice sheet).

As suggested by the reviewer, the anomaly method has been replaced by the direct method, making this suggestion obsolete.

- Sec. 2.2: resolution of ERA5 used?

As mentioned above, in the revised manuscript we focused on the implementation of PMIP without fusing them with the present-day climate. Therefore, we are not considering the present-day climate from ERA5 anymore. Nevertheless, the response to this question is that ERA5 has a horizontal resolution of 0.25º but in the initial version of the manuscript, lapse rate corrections were applied.

- Sec. 2.2: should cite previous papers that use the anomaly method such as, e.g., Charbit et al. (2002)

This will be corrected in the revised version of the manuscript.

- Sec. 3.x: I am not sure that Fig. 3 is the best illustrative figure. What we see in this figure is that the model strongly disagree, it is hard to see an emerging pattern. Some maps might help to understand the signal better. Maybe maps of surface mass balance for present-day topography or on ICE6G-C topography could have been useful.

Additional maps of the temperature and precipitation conditions will be included in the revised manuscript. Additionally, surface mass balance analysis and sensitivity tests will be added and discussed.

- Sec. 3.2: the eastern extent is probably due to a combination of the strong rain shadow effect in ERA 5. Is this rain shadow effect is expected to be so strong at the LGM given the flatter topography and different zonal winds? This is something to discuss.

Precipitation anomalies (LGM/PI) show a strengthening of the orographic precipitation, with larger precipitation on the western side of Patagonia in both annual and summer means and a reduced precipitation towards the eastern side.

- P8 L148-150: should we need a model to draw this conclusion?

What we mean here is that, among the PMIP models analysed in the study, those that present colder and wetter conditions produced a much thicker ice sheet, but not necessarily overextending the covered area. This will be reformulated in the revised manuscript in agreement with the updated results based on the direct method.

- Ice mask and topography used in PMIP models. I think this point is really important and it is good that it is brought forward. It should be clearly stated if the PMIP models use an ice

mask in agreement with the reconstruction or not. Generally there is a strong albedo effect so the local temperature simulated by the model is strongly impacted by the presence of an ice sheet. If the GCM does not have an ice mask in its boundary conditions it will be hard to reconstruct an ice sheet there. Is this the case here? Do you have a strong correlation of the January temperature anomaly and the ice mask? It seems that the ice mask is shifted to the East in models that do have an ice mask. This is probably the reason of the too large simulated extent there?

Several models demonstrate a strong spatial correlation of the 0ºC isotherm with the prescribed ice geometry. For the summer mean, the 0ºC isotherm obtained is significantly reduced and, in some instances, it is nonexistent. Additionally, even for the annual mean temperature, the 0ºC isotherm does not align with the PATICE geometries for 20 ka in most cases. We attribute this discrepancy in part to the coarse resolution of paleoclimatic models, which induces topographic flattening, resulting in elevated temperatures and preventing the generation of an ice mask that agrees with in situ observations. Nevertheless, the prescribed ice mask also plays a crucial role, particularly in Patagonia, due to its impact on albedo. A deeper discussion on this matter will be included in the revised manuscript.

Technical comments

- P5L86 Lambeck et al. (2014) have a drop of about 140 m of sea level.

This will be corrected in the revised version of the manuscript.

- Fig. 1: it is hard to see the topography with this grey colour gradient (while we see fine the ocean, but we don't need it). Generally the figures are a bit hard to read.

We will work on the improved figure design and colour scales

- Fig. 2: the green line is barely visible when there is a simulated ice sheet.

We will work on the improved figure design

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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

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

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.

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.

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