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 additional simulations to explore the sensitivity of the ice sheet configuration and dynamics to different parameter choices related to climate and ice dynamics parameters.

In conclusion, we are grateful to the reviewer for taking the time to propose improvements in the experimental design and for their very helpful proposal for how to improve the climate forcing approach. We regard the reviewer´s suggestions both comprehensive and constructive. As it is apparent from the following, we have addressed every single comment as long as it is still relevant in the light of the new research aims and strategies. Our responses are written in blue.

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

Reference

Charbit, S., Ritz, C., and Ramstein, G.: Simulations of Northern Hemisphere ice-sheet retreat: sensitivity to physical mechanisms involved during the Last Deglaciation, Quater. Sci. Rev., 21, 243–265, 2002.

Reference

Aracena, C., Kilian, R., Lange, C. B., Bertrand, S., Lamy, F., Arz, H. W., ... & Kissel, C. (2015). Holocene variations in productivity associated with changes in glacier activity and freshwater flux in the central basin of the Strait of Magellan. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *436*, 112-122.

Boisier. J. (2023). CR2MET: A high-resolution precipitation and temperature dataset for the period 1960-2021 in continental Chile. (v2.5) [Data set]. Zenodo. https://doi.org/10.5281/zenodo.7529682

Caniupán, Ana Magaly; Lamy, Frank (2011): Bulk geochemical and lipid biomarker data for sediment core MD07-3128. Alfred Wegener Institute, Helmholtz Centre for Polar and Marine Research, Bremerhaven, PANGAEA, https://doi.org/10.1594/PANGAEA.771854

Gowan, E. J., Zhang, X., Khosravi, S., Rovere, A., Stocchi, P., Hughes, A. L., ... & Lohmann, G. (2021). A new global ice sheet reconstruction for the past 80000 years. *Nature communications*, *12*(1), 1199.

Haddam, N. A., Siani, G., Michel, E., Kaiser, J., Lamy, F., Duchamp-Alphonse, S., ... & Kissel, C. (2018). Changes in latitudinal sea surface temperature gradients along the Southern Chilean margin since the last glacial. *Quaternary Science Reviews*, *194*, 62-76.

Harada, N., Ninnemann, U., Lange, C. B., Marchant, M. E., Sato, M., Ahagon, N., & Pantoja, S. (2013). Deglacial–Holocene environmental changes at the Pacific entrance of the Strait of Magellan. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *375*, 125-135.

Ho, S. L., Mollenhauer, G., Lamy, F., Martínez-Garcia, A., Mohtadi, M., Gersonde, R., ... & Tiedemann, R. (2012). Sea surface temperature variability in the Pacific sector of the Southern Ocean over the past 700 kyr. *Paleoceanography*, *27*(4).

Kaiser, Jérôme; Lamy, Frank; Hebbeln, Dierk (2005): Sea surface temperature record of ODP Site 202-1233. PANGAEA, https://doi.org/10.1594/PANGAEA.737972

Kilian, R., & Lamy, F. (2012). A review of Glacial and Holocene paleoclimate records from southernmost Patagonia (49–55 S). *Quaternary Science Reviews*, *53*, 1-23.

Mansilla, C. A., McCulloch, R. D., & Morello, F. (2016). Palaeoenvironmental change in southern Patagonia during the Lateglacial and Holocene: implications for forest refugia and climate reconstructions. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *447*, 1-11.

Masiokas, M. H., Rabatel, A., Rivera, A., Ruiz, L., Pitte, P., Ceballos, J. L., ... & MacDonell, S. (2020). A review of the current state and recent changes of the Andean cryosphere. *Frontiers in Earth Science*, *8*, 99.

Moreno, P. I. (2020). Timing and structure of vegetation, fire, and climate changes on the Pacific slope of northwestern Patagonia since the last glacial termination. *Quaternary Science Reviews*, *238*, 106328.

Recasens, C., Ariztegui, D., Gebhardt, C., Gogorza, C., Haberzettl, T., Hahn, A., ... & Team, S. (2012). New insights into paleoenvironmental changes in Laguna Potrok Aike, Southern Patagonia, since the Late Pleistocene: the PASADO multiproxy record. *The Holocene*, *22*(11), 1323-1335.

Retamal-Ramírez, F., Castillo, A., Bernales, J., & Rogozhina, I. (2022, May). Reconstruction of the Patagonian Ice Sheet during the Last Glacial Maximum using numerical modelling and geological constraints. In *EGU General Assembly Conference Abstracts* (pp. EGU22-1774).

Sato, T., & Greve, R. (2012). Sensitivity experiments for the Antarctic ice sheet with varied sub-ice-shelf melting rates. *Annals of Glaciology*, *53*(60), 221-228.

Sauter, T. (2020). Revisiting extreme precipitation amounts over southern South America and implications for the Patagonian Icefields. *Hydrology and Earth System Sciences*, *24*(4), 2003-2016.

Seguinot, J., Khroulev, C., Rogozhina, I., Stroeven, A. P., & Zhang, Q. (2014). The effect of climate forcing on numerical simulations of the Cordilleran ice sheet at the Last Glacial Maximum. *The Cryosphere*, *8*(3), 1087-1103.

Seguinot, J., Rogozhina, I., Stroeven, A. P., Margold, M., & Kleman, J. (2016). Numerical simulations of the Cordilleran ice sheet through the last glacial cycle. *The Cryosphere*, *10*(2), 639-664.

Seguinot, J., Ivy-Ochs, S., Jouvet, G., Huss, M., Funk, M., & Preusser, F. (2018). Modelling last glacial cycle ice dynamics in the Alps. *The Cryosphere*, *12*(10), 3265-3285.

Seguinot, J., & Delaney, I. (2021). Last-glacial-cycle glacier erosion potential in the Alps. *Earth Surface Dynamics*, *9*(4), 923-935.

Yan, Q., Owen, L. A., Wang, H., & Zhang, Z. (2018). Climate constraints on glaciation over High‐Mountain Asia during the last glacial maximum. *Geophysical Research Letters*, *45*(17), 9024-9033.

Yan, Q., Wei, T., & Zhang, Z. (2022). Modeling the climate sensitivity of Patagonian glaciers and their responses to climatic change during the global last glacial maximum. *Quaternary Science Reviews*, *288*, 107582.

Yan, Q., Wei, T., & Zhang, Z. (2023). Modeling the timing and extent of glaciations over southeastern Tibet during the last glacial stage. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *610*, 111336.

Wei, Q., Liu, Y., Yan, Q., Yao, T., Wang, M., Huang, H., & Hu, Y. (2023). The Glacier‐Climate Interaction Over the Tibetan Plateau and Its Surroundings During the Last Glacial Maximum. *Geophysical Research Letters*, *50*(14), e2023GL103538.

Ziemen, F. A., Hock, R., Aschwanden, A., Khroulev, C., Kienholz, C., Melkonian, A., & Zhang, J. (2016). Modeling the evolution of the Juneau Icefield between 1971 and 2100 using the Parallel Ice Sheet Model (PISM). *Journal of Glaciology*, *62*(231), 199-214.