Response Reviewer 2

This substantially revised work from Castillo-Llarena, Retamal-Ramírez et al. presents a set of transient numerical simulations of the Patagonian Ice Sheet (PIS) which now extend back to the last 70 kyr to explore both MIS3 and LGM, until the last deglaciation.

I applaud the authors for the extensive revision they made in order to satisfy my and Reviewer#1 requests. I can imagine how much effort changing the focus of the paper might have costed, and therefore I want to express my sincere appreciation. I like very much the idea of performing transient simulations that go back to the last 70 kyr. This surely adds scientific novelty to the manuscript.

Response: We thank the reviewer for taking the time to review our revised manuscript. We greatly appreciate your acknowledgement of the substantial effort invested in addressing the comments and suggestions previously provided by you and the first reviewer. Your recognition of the novelty of our simulations is truly gratifying and inspiring.

Overall, the clarity of the manuscript has been greatly improved and the manuscript reads definitely better. Paleo transient simulations follow the classic approach that uses a climatic index extracted from various paleo records. This methodology has known limitations, but in absence of transient climatological runs for the whole LGP (Last Glacial Period), this approach is possibly the best option the authors have to model the PIS back to the last 70 kyr. Also the climate analysis performed at the LGM is easier to understand now, as it has lost the ERA5 imprint.

Response: We concur that using a glacial index method derived from various paleorecords for transient simulations inherently carries limitations but as the reviewer states and given the constraints, we believe it remains the most viable option for the modeling of the behaviour of the Patagonian Ice Sheet during this time frame.

We are pleased to note your appreciation of the enhanced clarity in the manuscript. We have made concerted efforts to refine the presentation of our findings and streamline the discussion to ensure a better interpretation of our analysis.

The recommended changes have been implemented in the updated version of the manuscript and are addressed point-by-point below.

I still have some concerns, though, that should be addressed before publication.

1. Model description. The work presented here is based on the usage of the numerical ice-sheet model SICOPOLIS, a tool that applies several physical parameterisations in addition to the SIA+SSA solvers to describe the dynamics and evolution of an ice sheet. Still, I am surprised that no equations whatsoever are displayed. Several references referring to important parameterisations (ELRA, climatic index approach, PDD, …) are reported in the Methods, but some laws/parameterisations crucial for the study should be reported. I am referring specifically to the climatic index method, which is the focus of the manuscript. I would definitely include at least eq 5 and 6 of Mas e Braga, 2021, specifying that

preindustrial and LGM climates are taken from PMIP experiments. Also, although the chosen basal friction law and parameter values are of secondary order importance compared to the climatology used, I would at least write which is the basal friction laws employed, and the value set for Cb, p and q (Eq. 5 of Sato and Greve 2012) in Table 1, if not even report the equation in the text. In the answers to reviewers the authors described most of these details (basal friction law, how the hydrology is calculated…), but they haven't reported them to the main text. Also, what about the ELRA model for the GIA? Since you are now performing transient runs for 70 kyr, the GIA model gains in significance. How are the asthenosphere relaxation time (usually the most sensitive parameter in the ELRA model) and the lithosphere flexural stiffness set? Please add these details to Table 1. These details would facilitate a direct comparison of future work to your study.

Response: We thank the reviewer for pointing out what details might be important for the reader. Following the reviewer's suggestion, the revised manuscript will include equations and constants/parameters used to facilitate the comparison with our study.
For completeness, here we provide a set of brief answers to the individual questions in the reviewer's comment. To perform the transient runs, the temperature and the precipitation of the pre-Industrial (PI) and Last Glacial Maximum (LGM) model ensemble have been interpolated by using the glacial index method. The LGM and PI climatologies were obtained from the PMIP model. The weight interpolation has been set as shown in Eq. 1 and 2.

$$T(t) = T_{PI} + GI(t) \cdot (T_{LGM} - T_{PI}) \qquad (1)$$

$$P(t) = P_{PI} \cdot (1 - GI(t)\,(1 - \frac{P_{LGM}}{P_{PI}})) \qquad (2)$$

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). The sliding coefficient is set with a constant value of 10 ma-1Pa-1, while the sliding coefficients p and q are 3 and 2, respectively. The ELRA model has been set by using a time lag of the relaxing asthenosphere of 3000 years and a flexural rigidity of the lithosphere of 10^25 Nm.
As suggested, specific values and equations were added to the revised version of the manuscript.

2. Missing a quantitative description for the climatic analysis. The analysis of the various PMIP LGM climate scenarios is extensive but very much qualitative, as based on the description of the spatial patterns shown in Figures 2 and 3. I think it would be beneficial for the reader trying to summarise the outcomes of sections 3.1.1, 3.1.2 and 3.1.3 in a figure that resembles Fig 3 of the previous submission (scatterplot between the temperature and precipitation for the three PIS sectors, for all ensemble climate members) and/or an additional figure showing the modelled area vs mean temperature/precipitation ratio for all climate members. I think these two figures would help the reader to follow the discussion more easily. I would also suggest to write the spatial mean T (and precip) in each box of Fig 2 (and 3) for completeness.

Response: Following the reviewer's advice, in the revised manuscript we have included scatterplots of the summer mean temperature and winter mean precipitation, as well as the annual mean temperature and precipitation for the 3 zones described in the text. As indicated in the manuscript, the eastward overexpansion of the PIS has been linked with the

temperature and precipitation patterns beyond the field-reconstructed ice sheet geometry. Therefore, it should be taken into account that the selection of scatterplots and/or zonal means might induce erroneous interpretation of the overexpansion of the resulting ice geometry since they could oversimplify the spatial pattern.

3. Best model used for the transient runs. Looking at Fig. 4, I am not very much convinced that MPI-ESM1 is the model performing best at the LGM. What about AWI-ESM for example? Although it underestimates the ice sheet coverage in the north west, it seems to show a better fit elsewhere, as it doesn't overestimate much the ice sheet in the south east. Some more comments on how the best model has been chosen and relative discussion are needed. Perhaps showing a figure as suggested above would Help.

Response: During the evaluation, both MPI-ESM1-LR and AWI-ESM-1-LR showcased better performance relative to other models in simulating the Last Glacial Maximum under steady-state conditions. However, they exhibited significant disparities in their temporal responses to growth. Notably, AWI-ESM-1-LR displayed a slower pace of growth attributed to its temperature field. This slower growth response during the Last Glacial Maximum led to an unrealistic configuration of the ice sheet when performing transient simulations under the model configuration chosen in this study. Given the importance of the transient behaviour and the goal of our study, we concluded that MPI-ESM1 provides the best simulation.

I also have some final specific comments that need to be addressed. I wrote them directly on the manuscript, which I here attach.

The specific comments added by the reviewer to the manuscript have been addressed directly in the revised version of the manuscript.

Besides these concerns, I think the paper has considerably improved since the first submission and I would be happy to see it published in CP after having resolved my above-mentioned final comments.

Once again, we extend our gratitude for your constructive feedback, which has obviously strengthened the quality of our work.