The authors present a modelling reconstruction of the northern branch of the Patagonian Ice Sheet (PIS) during the last deglaciation, precisely from the last glacial maximum (LGM) to 10 ka ago. The ice-flow model ISSM is used for this purpose. The exercise makes use of various glacial climatologies to reconstruct the glacial state of the ice sheet and of a transient climatology (TraCE-21ka) to simulate the early stage of the deglaciation. The results are then compared with reconstructions available for that region (PATICE - Davies et al., 2020). Ice-climate interactions, sensitivity on the employed climate model, and goodness of the model results are then discussed.

The work is novel and very appealing, as it is the first modelling work trying to delucidate the deglaciation history of a region of the PIS that unfortunately still presents a lot of uncertainty. The manuscript is well written, well organised ad the methodology is mostly sound. Still, I am very surprised that no Glacial Isostatic Adjustment (GIA) is taken into account in their simulations. I understand the authors justify this deficiency as ISSM is currently lacking a GIA solver (even though they claim it's under development), and that they have already successfully applied a similar model configuration - with only relative sea level changes - in previous work in Greenland (Cuzzone et al., 2019 and Briner et al., 2020). However, to me this lack represents a big flaw in the work presented here, as I would expect that GIA has a clear primary control on the evolution of the PIS during the last deglaciation.

It is very well known that the chilean coastal region presents a unique tectonic setting promoting fast response of solid earth to ice mass changes (e.g. Richter et al., 2016, Troch et al., 2022, …). In fact, the thin lithosphere combined with a low upper mantle viscosity and current fast ice retreat leads to extraordinary high uplift rates there. These are found today near the Southern Patagonia Icefield (~4 cm/a) (Lange et al., 2014; Dietrich et al., 2010), for instance, where the current uplift is mostly due to the ice unload after the Little Ice Age. Yet, strong GIA signals are also found for the last deglaciation/early-middle Holocene in:

- Larenas Bay (48°S, between modern Northern and Southern Patagonia Icefields), which rebounded isostatically by almost 100 m between 16-8 ka ago, with a rate of 1.3 cm/yr (Troch et al., 2022), and most of it occurring before 14 ka ago;
- Northern Patagonia Icefield, where GIA rates of 1.5-3.4 cm/a were found for the past ~8 ka (Bourgois et al., 2016);
- More to the south, in the Strait of Magellan, with uplift rates of 0.5 cm/a for the last 13 ka (Rios et al., 2020).

It is true that data of uplift rates for the last deglaciation/early Holocene, especially in northern Chile, is somehow lacking. However, new relative sea level (RSL) reconstructions along the south-central chilean coast (18°S - 44°S, Garrett et al., 2020) reveal interesting informations. Regions where tectonic deformation is associated with the subduction of the Nazca Plate beneath the South American Plate may have experienced uplift rates even higher than 1 m/ka during the mid Holocene (such as Isla Santa María and Isla Mocha). Other regions, such as the southern area of Bío Bío, Valdivia and Arauco, show a sea level highstand of 6-8 m around 8-7 ka ago compared to the present, suggesting a clear local response of solid Earth to the ice unload. This is confirmed by the agreement between such RSL data and GIA model simulations (ICE-5G and ICE-6G), suggesting that the isostatic uplift has a primary control in changes in RSL in these regions (Garrett et al., 2020). This is also in agreement with strong uplift rates (1.5 ± 0.3 m/ka) found in Isla Santa María during MIS3 (Jara-Muñoz and Melnick, 2015), and those found in the region south of the Arauco Peninsula (0.5 m/ka) during the Holocene (Stefer et al. 2010). All these areas are close to the northern branch of the PIS (35°-43°S), therefore it is very likely that the isostatic rebound had a crucial role also in the evolution of the PIS.

The lack of a clear response of the lithosphere to the ice unload during the deglaciation might well affect the results presented in this paper. In fact, the exclusion of an isostatic rebound due to ice melt might partly explain why the area modelled after 16 ka is well below the reconstructions from PATICE: the modelled surface elevation might be too low to sustain the existence of an extensive ice field for increasing Holocene temperature. This is somehow - although indirectly - shown in the sensitivity experiment, where a constant, LGM precipitation (higher than today) is applied to the whole Holocene. In this test, we see that even a small variation in the precipitation might strongly affect the retreat in terms of timing and deglaciation rate. This is because - here - a higher winter accumulation rate helps to sustain the presence of glaciers even with warmer temperatures. Still, a delayed retreat could also be the result of an uplifted topography, which ensures temperature lapse rate near the glacier surfaces to decrease more rapidly. It would be interesting to see if the authors can reproduce similar results in the retreat either by reducing the tropospheric lapse rate (i.e. making the atmosphere cool more rapidly at increasing elevations) or by considering a synthetic higher topography (+50 m, +100 m, for example) for specific deglaciation times. Therefore, the deglaciation history might not be only defined by the applied paleo climatology, but also by climatology/topography effects that are not taken into account for the moment due to the lack in the GIA treatment. In such a case the whole discussion on climate-ice sensitivity as described in the manuscript risks becoming pointless. Put it in another words, it might be that the simulation experiment with a higher precipitation set for the whole deglaciation matches better the geological reconstructions, but for the wrong reasons.

Finally, the glacial outline from PATICE (Davies et al., 2020) is really uncertain in the northern part of the domain (in fact only very few radiocarbon dates were taken in the Lake district). It might be that the PIS was covering a region further north than what is presented here, as suggested by previous work (Rabassa & Clapperton 1990, Garret et al., 2020). In that case the reconstruction from Trace-21ka would not be sufficient to cover those areas and the whole discussion comparing different climate model outputs and comparing model results to the reconstructed ice retreat during the deglaciation becomes sterile. Therefore, I suggest to clearly discuss the uncertainties in the glacial reconstruction of the northern boundaries before comparing them to the model simulations.

In summary, I am afraid I cannot recommend the publication of this work in The Cryosphere until the lack of a GIA treatment in the model is either exhaustively discussed, or it is taken into account presenting a new set of simulations from the same ISSM model, when the GIA module becomes available, or from another ice-flow model which already computes the interactions between the solid Earth and the ice sheet.

Here I note down specific comments:
Line 71: please describe how the SWW position and strength changed during the last deglaciation with more details.
Line 85: what do you mean by “climate?”
Line 113-115: This is already written some lines above. Please rephrase to avoid repetitions.
Line 145: how is N calculated?
Lines 156-161: this paragraph about the missing GIA model definitely needs further development. See my main comments above.
Lines 228-229: why not simulating calving at the ice-lake interface too? Could you apply the von mises stress law also there?
Line 248: please change “grounded” to “tidewater”.
Line 276: MIROC has drier winter conditions only in the southern part of the domain, Please correct.
Line 399-413: I don’t see the point of this sensitivity test. Yes, the experiment might be interesting to see the effect of the increased precipitation in the retreat. But what do we learn from this? Does this mean that the reconstructed precipitation is wrong? Or could this be related (also) to the missing uplift upon ice unload in your experiments? Please, discuss this further and think about other possible sensitivity tests about atmospheric lapse rate/synthetic elevation (see paragraph above).
Line 450 onwards: I am missing at least a large paragraph concerning the model limitations, such as lacking GIA effect, lake-terminating calving, …, and their possible influence in the results.
Line 478: why did you choose this “small sample” of PMIP4 climatologies? Why these models, precisely?
Lines 528: again, could not this be related to the missing regional uplift too?
Lines 557-565: I suggest to work on these conclusions as they are only partly corroborated by your sensitivity test.

Figures:
Figure 1, figure 2: it would be helpful to add some reference locations to the map (e.g. gulf of Ancud, Seno de Reloncaví, …) and lat/lon.
Figure 3: I would like to see a further discussion about the reasons that explain the main differences between the PMIP4 models (model parametrisations, …) or at least citing some papers that point to that.

Figure 6: why not plotting the same figure with respect to the glaciated area and comparing it to PATICE reconstruction? It could be also interesting to plot the same figure, but separately for the northern and southern parts of the domain (e.g. north and south of 40°S) since climatologies present a strong latitudinal pattern.
Figure 7: please choose a different color scale as ice lost from the LGM to 17 ka ago is very difficult to see.
Figure 10: please use different colours for the simulated outlines (orange, red?) otherwise they can be confused with the topography.

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