Review of Schaefer et al, The Cryosphere

Thank you for this opportunity to review this manuscript, and apologies for the tardy review.

General comments

Schaefer et al present a most welcome analysis of the future of the Northern Patagonian Icefield, a sensitive icefield of some import in Patagonia. The manuscript is clearly presented and provides a careful analysis of the future behaviour of the icefield.

Thank you!

I have some recommendations for improved clarity and to hopefully help elevate the manuscript.

My most significant comment is to do with the surface mass balance approximation of the icefield. The Andes have one of the most extreme climatic divides found worldwide (Sauter, 2020). The strong orographic influence on climate is evident in terrestrial observations. To what extent does downscaled climate data match observations? This needs a clear evaluation.

The main focus of this contribution is the application of an ice flow model to the Northern Patagonia Icefield and realize projections. Regarding climate data and corresponding surface mass balance maps this work relies strongly on the results of Schaefer et al 2013. Comparison between modeled and observed climate are shown in Table 2 and Figures 5, 6, and 7 of this contribution. Temperature biases of the modeled data range from -0.13 and +1.14 deg. Celsius when comparing to individual weather stations. Correlation of monthly precipitation time series range from 0.21 (in El Calafate) to 0.81 (Puerto Aysen). The climate divide is nicely visible in the modeled incoming radiation data (Figure 8 of Schaefer et al. 2013), where the glacier tongues on the Eastern side receive considerably more radiation then the plateau area of NPI.

Specific comments

Bed topography – how does the bed topography compare with that provided by Millan et al. (2022)?

A comparison of both datasets is shown in Fuerst et al 2024 (Fig 3). The bed we used has deeper and more connected troughs then the one obtained from Millan et al. We could add a comparative plot in the supplementary.

In line 136 insufficient detail is provided for the SMB model. How does this calculate SMB? This is important, as differences in SMB may explain the differences in these projections and others in the Discussion.

More details on the surface mass balance model were given now.

Line 138 – the ablation stakes and shallow firn cores are vague, and this evaluation needs more explanation.

More details on the ablation stakes and firn cores and the model validation will be indicated in the new version of our manuscript..

Line 153 – how do these ice velocity measurements differ to Millan et al. (2022)?

The study period of Mouginot and Rignot (2018) is 1984-2014 whilst Millan et al. (2022) are studying 2017-2018.

The modelling workflow is provided clearly, but some more details are required in places.

Line 169 – I agree that the NPI in 2000 was likely not in equilibrium with climate, and there observational datasets of ice recession and thinning at this time that would support this statement.

Data on ice recession and thinning are used in the model calibration phase. The results of this phase are presented in section 4.1: Table 2 & Figure 6

Line 197-198 – to help the reader, clarify again the experiments run (SSPs, to what year, etc.).

Ok, sentences will be rephrased and information on SSPs will be added.

Line 200 – it is not clear to me why the additional experiment forced by ECHAM5 A1B scenario is used. This is quite dated now and has been superseded by CMIP6. Also this is one model, whereas the CMIP6 is a multi model mean. I would recommend just removing this experiment as I don't think it adds anything.

Ok, the outdated scenario was shown in order make the link to the results of Schaefer et al. 2013. It will be removed in the revised version of the manuscript.

Line 202 – to help the reader, and improve usefulness for readers, I recommend also summarising the volume and area change as a % of the 2000 ice volume (or some other standard year).

Volume changes until 2100 as a % of the 2020 ice volume are shown in Figure 10. Two new tables will be added in the supplementary where % of the 2000 ice volume and icefield area values are indicated for the years 2050, 2100, 2150 and 2200 for both SSPs and the committed mass loss run.

Figure 8/9 – the 'm' in the colour bar is rotated. It would be easier to read if it were horizontal. Note the year of glacier outlines shown (2000?).

The unit of the colorbars is rotated in all maplots (Figures 4,5,6,8,9) . This mostly for space efficiency (when the unit is m/year). For consistency we would prefer to keep it rotated. Yes the outlines correspond to the year 2000 outline, information will be added.

Clarify here why the experiments were run to 2200 and not 2300, when CMIP 6 extended model runs are available until 2300 CE (Eyring et al., 2016).

We think that the climate of the 22nd and 23rd is very uncertain and depends crucially on the decisions the humanities takes in the ongoing century. We prefer to use instead a constant climate forcing from 2100 on. Simulation were stopped in 2200 as the ice volume got mostly stable in the simulation by 2200.

Line 204 – clarify here the total area loss in km2 and % change

The area losses will summarized in Table A3 and and reference to the table will be added in the text.

Overall I found the results section rather brief. I would add more details on glacier changes under the two different scenarios. Do glaciers remain calving by 2200 or are all on dry land, above sea level? What is the ice velocity?

Thank you for asking these interesting questions. In the SSP1-2.6 scenario San Rafael Glacier will remain a tidewater calving glacier until the end of the simulation period, whilst in SSP5-8.5 scenario it turns into a land-terminating glacier. Ice thickness reduction is accompanied by ice speed reduction. Information on both points will be added in the results section.

Can you show the simulated surface mass balance on the icefields for the two different SSPs at different timescales (like the ice thickness figure).

The surface mass balance which corresponds to the ice-thickness states shown in Figure 8 and 9 will be shown in two additional figures in the supplementary.

Line 217 – more information on the SMB model used could also be helpful here. Was the SMB a temperature index model or something more complex, and could this explain the differences?

Aguayo et al. 2024 use a temperature index model, which could also be a reason for the observed differences. This will be mentioned in text.

Line 221 – precipitation is crucial for the Patagonian icefield SMB (Sauter, 2020); any biases in the downscaled climate data would have a big effect, potentially larger than calving flux. This needs more careful evaluation.

We agree that solid precipitation input is crucial for the temperate Patagonian Glacier. However there is very few information on the amounts of precipitation falling over the Patagonian Icefields. Schaefer et al. 2013 compared their SMB simulation to firn cores taken on the NPI and obtained satisfactory results (Figure 11).

Line 227 – could these differences also be explained because GlacierMIP2 (Marzeion et al., 2020) is forced by CMIP6 at a broader scale, with temperature index models used to calculate SMB at best. How much more (or less) reliable is the climate forcing data used here given the statistical downscaling?

In this section we say rather try to say that our results are “surprisingly similar” to GlacierMIP2 and indicate that this is rather a comparison then a validation. The comparison is a bit comparing apples with oranges, but we still think that has some value, since we do not have more apples in the region ...

Line 272 – I couldn’t see these results of % change in the results, don’t include new things in the conclusions and put these data in the results section too.

The % changes will be added in Table A2 and mentioned in the results section.

Table A1 – precipitation variation will also be very important, can you also provide this information in the table?

Our surface mass balance projections are solely based on temperature anomalies (see section 3.2.2 and Figure 3, left panel), so we did not analyze precipitation variation.

References used in this review.

Eyring, V., Bony, S., Meehl, G.A., Senior, C.A., Stevens, B., Stouffer, R.J., Taylor, K.E., 2016. Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization. *Geosci. Model Dev.* 9, 1937-1958.

Marzeion, B., Hock, R., Anderson, B., Bliss, A., Champollion, N., Fujita, K., Huss, M., Immerzeel, W., Kraaijenbrink, P., Malles, J.-H., Maussion, F., Radić, V., Rounce, D.R., Sakai, A., Shannon, S., van de Wal, R., Zekollari, H., 2020. Partitioning the Uncertainty of Ensemble Projections of Global Glacier Mass Change. *Earth's Future* 8.

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