Review of “Projecting the evolution of the Northern Patagonian Icefield until the year 2200” by Schaefer et al.

This manuscript presents simulations of the Northern Patagonian Icefield and its future evolution through the year 2200. The manuscript is generally well written, clearly structured, and suitable for publication in The Cryosphere. However, I found that the Results and Discussion sections could be strengthened, particularly regarding the treatment and discussion of uncertainties. My main comments are outlined below.

General comments:

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SMB

If I understand correctly, you apply a present-day surface mass balance (SMB) from Schaefer et al. (2013) and adjust it homogeneously using a temperature anomaly. If so, precipitation changes derived from the Earth System Models (ESMs) are not accounted for. I would expect that under warmer conditions, increased melt would occur but potentially also increased precipitation at higher elevations. Is it not feasible to run the reference experiments using precipitation fields directly from the ESMs you employ?

The focus of our work is the application of a ice-flow model to the NPI. Regarding the surface mass balance this work strongly relies on Schaefer et al. 2013. In Schaefer et al. a very clear relationship between projected (mean) surface temperature over the NPI and the (mean) surface mass balance was found (explained variance of 80%, Figure 3 left panel of our study). In this work we rely on this relationship between temperature and surface mass balance. Using precipitation fields directly from the ESMs is not possible due their low spatial resolution which does not represent at all the complex topography of the NPI (see Figure 1 below). Another good argument for our procedure is that temperature projection are generally more reliable then projection of precipitation.

Related to this, I suggest addressing the following points in the Discussion section:

● Do you expect the SMB forcing approach used here to respond similarly across the range of ESMs considered?

We do not apply our smb parametrization to the to temperature projections of the individual ESM but to the mean value of the temperature (anomalies) and to the mean +/- one standard deviation.

● Are elevation feedbacks included in your SMB–temperature relationship? From the current description, it does not appear that they are.

Thanks for your question. SICOPOLIS has this feature but we did not not activate it so far. We will activate it for next round of simulations.

● Spatial patterns present in the ESMs may not be captured with the adopted methodology. I understand that downscaling ESM outputs may be challenging, but acknowledging this limitation would strengthen the discussion.

The spatial patters of the surface mass balance fields stem from Schaefer et al. 2013 , where model parameters were adjusted to represent best the spatial variation of point surface mass balance measurements form the Eastern and Western side of the icefield including two ice-cores from the accumulation area and geodetic mass balances of three not calving glaciers. Since the spatial resolution of ESMs is very low, a maximum of four grid cells was used to infer future temperature (mean) anomalies (see figure below) We will give detailed information on model resolution in table A1 in the revised version of our manuscript.

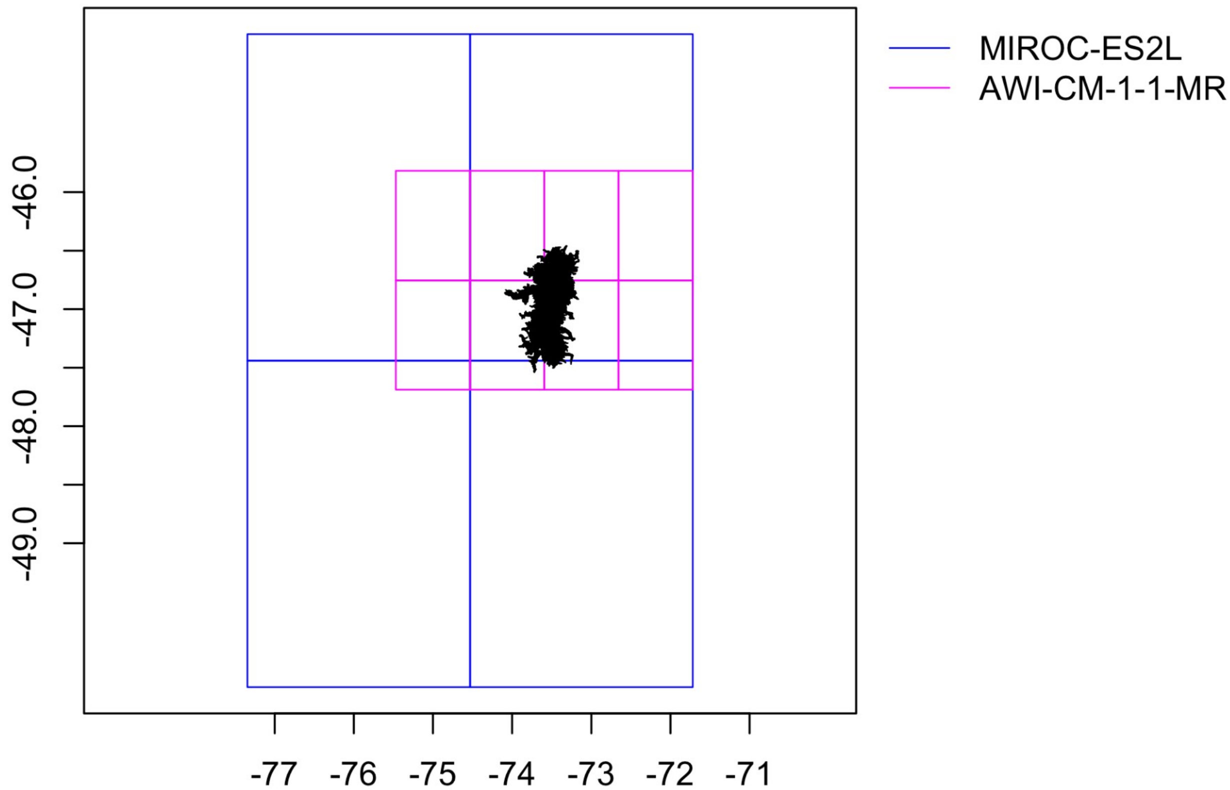

Figure 1: Illustration of the ESM model grid cells and NPI extension for two ESMs with different spatial resolution.

● What is the temperature spread of each ESM over the Northern Patagonian Icefield?
We did not check that explicitly, but as there only two grid cells involved it should be very low!
As I understand it, the standard deviation you apply corresponds to the variability of the mean temperature values across models. However, I would expect substantial spatial variability in temperature within the domain. In that case, applying a spatially uniform standard deviation may not fully capture the uncertainty. Would it be more appropriate to use a spatially varying measure of variability instead?
We agree that assuming spatial uniform temperature spread is a simplification of the reality. However as there are only two grids involved we argue that the impact of this simplification should be low.

Spin-up

I was also uncertain about certain aspects of the spin-up procedure. As I understand it, you use the SMB field from Schaefer et al. (2013) but increase the input SMB by reducing global temperature by 1°C—is this correct?
Yes!
Additionally, it seems that only one spin-up is performed during 500 years, and then followed by 20-year runs with varying parameters for calibration. In that case, would part of the diagnosed drift between 2000–2020 arise from parameter changes rather than the applied forcing? Some clarification would be useful.

For every calibration run a separate spin-up was realized with the same parameter set, so no model drift is expected which stems from parameter changes. We will state this more explicitly in the new version of the manuscript.

Basal friction

Regarding the basal friction law, you define a relationship dependent on basal temperature and basal water thickness, and Figure 2 shows the effect of varying these quantities. However, no sensitivity experiments are presented for these parameters. The spin-up varies only C_b^0, while C_w, γ, and H_ware fixed. Under this configuration, the relevance of Figure 2 is unclear, and removing it may improve the manuscript's focus.

We think that Figure 2 represents a useful visualization of equation (2), but we also agree that slip parametrizations are not the main focus of our study. We will move the figure to the appendix.

That said, I believe a sensitivity analysis of H_w or geothermal heat flux would be more informative than focusing solely on friction coefficients, because these could significantly influence basal sliding. Could you include at least one or two sensitivity tests to illustrate this?

Thank you for your suggestion. We will realize a sensitivity analysis of the parameters you mentioned and inform the results in the new version of our manuscript.

Parameter values

I also recommend citing references supporting your chosen values for C_w, γ, and H_w.

These parameters were optimized in order to obtain the best agreement between modeled icefield and current (year 2000) NPI state.

Similarly, the source of the geothermal heat-flux value should be given.

We chose 65 mW/m² since it is in good agreement with the mean heat flow in South America of 63+-36 mW/m² (Hamza et al. 1996). Reference will be added in the new version of the manuscript.

Different choices for geothermal heat flux could warm or cool the bed and potentially alter the dynamical state, so discussing this in the manuscript would be valuable.

Ok, discussion will be added depending on the results of the sensitivity tests.

Calving law

Concerning calving: How is the calving front represented in the model? Do you employ a level-set method, or do you apply a basal melting rate to ice-front nodes? The applied calving rate of 1000 m/yr appears high. When you say this value "was found to match the current observed calving flux," do you refer to flux magnitude or to the present-day terminus position? If it refers to flux, please provide the corresponding observed calving flux value.

Inferred calving fluxes in literature range from 0.6 to 1.85 Gt/year. Minowa et al 2021 found 1 Gt/year. With our our current calving parameter a_calv=1000m/year at 900m grid cells size the maximum model calving flux per grid cell is 0.9x0.9x1.0Gt=0.81 Gt/year. During the calibration (as well in the projection) period the ice tongue is mostly represented by two grid cells which would give a maximum calving flux of 1.62 Gt/year. The "real" calving fluxes are often lower then this value since sometimes there is not enough ice to calve of (no negative ice thickness is allowed). More details on the calving parametrization and model resolution will be given in the re-submission of our manuscript.

Future scenarios

It appears that only one calibration ensemble member (cal6) is used for the forcing. Relying on a single member may limit the robustness of the conclusions. Since all ensemble members show broadly similar present-day tendencies, could additional members be included to assess the sensitivity of future projections? You could add a weight to these simulations based on their present-day performance. This may also help evaluate the influence of parameters such as friction coefficients or enhancement factors.

Thank you for this suggestion. We will test how the projection changes when using other calibration parameter sets (e.g. cal4 and cal9, which have bias in dH/dt with a similar magnitude). Depending on the result of this sensitivity test we will decide if and how to incorporate different calibration parameter sets in our projections.

Comments on Figures

● Figures 4 and 5: Instead of plotting model results alongside observations, it may be more informative to show the anomaly (model − observation). This could highlight spatial biases that are otherwise difficult to identify.

Thank you for the suggestion. We will show the differences in the new version of our manuscript.

● Figure 7: Consider including the simulated ice-covered area for completeness.

We will add information on area changes in a new table in the supplementary part of the paper and add an area evolution plot in the supplementary as well.

● Figure 10: It appears similar to Figure 7 but expressed in percentages. If so, you may consider merging or simplifying.

Yes, both figures look similar but fulfill a very different purpose: Figure 7 presents the results of our simulations, but figure 10 compares our results to the results of other contributions. Relative volume values are shown here since we are also comparing to the simulation with include other icebodies.

Minor Comments

● Table 1: Please clarify the role of the "residual stress parameter." I could not find further explanation in the manuscript.

The residual stress parameter appears in the regularized Glen flow law, which avoids the infinite-viscosity limit for zero effective stress (Greve and Blatter 2009, Sect. 4.3.2).

● Line 272: It should be SSP5-8.5, not SSP1-8.5 (also in Table A1).

Ok thanks, changed!

● I agree with the other reviewer that removing the ECHAM5 scenario would improve clarity and facilitate the reading of the manuscript.

Ok, we will remove the Echam5 model in Figure 3, right panel and in Figure 7.

References:

Greve, R. and Blatter, H.: Dynamics of Ice Sheets and Glaciers, Springer, Berlin, Germany etc., https://doi.org/10.1007/978-3-642-03415-2, 340, 2009

Hamza, V. M. and Muñoz, M.: Heat flow map of South America, Geothermics, 25, 599–646, https://doi.org/https://doi.org/10.1016/S0375-6505(96)00025-9, 1996