Changes in Antarctic surface conditions and potential for ice shelf hydrofracturing from 1850 to 2200

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In this paper, the authors present a reconstruction method to emulate Antarctic surface mass balance conditions from Regional Climate Models from 1850 to 2200. They use this method to assess which ice shelves may be vulnerable to hydrofracture under different future emission scenarios. Developing methods to emulate important ice shelf processes that are under-resolved in Earth System Models is important for ice sheet modelling. However, I find the methods presented in this work to be extremely hard to follow, making it difficult to assess the robustness of this approach and the corresponding results. I believe that substantial clarification and elaboration throughout the methodology are necessary.

Major concerns and general comments

1) The distinction between "meltwater production" and "runoff" is unclear throughout the paper. This is especially the case as the authors investigate both runoff as a contribution to sea level changes and the "emergence of runoff conditions necessary for hydrofracturing". I find these two outcomes of 'runoff' a bit contradictory because if meltwater runs off into the ocean and contributes to sea level rise, then it can't induce hydrofracture events, which result from the pressure of ponded meltwater (i.e. Bell et al 2018). I think the authors should be careful to clarify this terminology throughout the paper. For example, in L27 the authors state: "The exact warming level needed to trigger important production of runoff on a given ice shelf depends on the amount of snowfall and on the snow/firn temperature and density (Donat-Magnin et al 2021, van Wessem et al., 2023)" However, these studies specifically look at *meltwater production*, which is the important component for hydrofracture on ice shelves, not runoff.

2) <u>Methodology</u>

There are many parts of the methodology that are not well explained and remain unclear. This makes it challenging to assess the robustness of the methodology and therefore the results. I suggest a graphical figure detailing the methodology. Below are some specific sentences or sections I did not understand or believe require additional detail:

a) L109: "To extend surface variables to a given local warming or cooling level, we always start from 20 different years (i.e. different values of *Tref*), then we average the 20 extended values." What does this mean? Does this essentially create a smoothed reconstruction?
b) L114: "The *a* and *b* parameters are obtained through a least-mean-square-fitting of an exponential curve for SMB minus runoff on the one hand and the surface melt rate of the other hand." A supplementary figure off this exponential curve would be very helpful.
c) L115: "The fit is done on the original model grid as regridding does not preserve exponential relationships." I find this statement to be concerning. My understanding is that the exponential relationship between the two variables may weaken in the regridding but should remain? Further, if the exponential relationship parameters are fit on the original grid, and this relationship is not preserved in the regridding, is it then appropriate to apply this fit on the regridded data?
d) I am unsure how to interpret the *r* parameter (Eq. 3, 4, L121-124). Is this the percent of excess meltwater production that is converted to runoff (as opposed to that which ponds or refreezes)? If so, I expect that this value might be different on the grounded ice sheet vs ice shelves due to due to higher slopes on the grounded ice sheet.

e) Section 2.2.4: It is unclear how you reconstruct a cooler scenario from a warmer one. Do you use SSP5 to reconstruct SSP1? Or use warmer years as a reference time and reconstruct back in time? Also, what is the purpose of this and how will this be useful?
f) L157: "Similarly as in the previous subsection, each reconstructed year is the average of 20 reconstructions from a reference ranging from 10 years before to 9 years after the reconstructed year." I don't fully understand this sentence and have had to read it several times. It is perhaps related to the point mentioned in a) above?
g) Section 2.2.5: A graphic or schematic outlining the workflow here would be extremely helpful because I don't understand how the emulation is done. Additionally, Figure 3 is not very intuitive for me and should be better explained as I am unsure how to interpret it.
h) Figure 4: In panels e and f, the reconstructed runoff anomaly is too low, despite the melt anomaly (panel c and d) being fairly accurate for ice shelves and too high over the grounded ice sheet. Does this suggest some mis-parameterization in your method? Perhaps the wrong *r* value?
i) L205-208: For these seven simulations… apply a ramping transition between the two methods from 2101 to 2120." I don't follow what is being done here and again, I think a figure or something would help the reader understand the methodology.
j) L280: The choice of a 100kg/m2/yr runoff threshold for triggering hydrofracture seems extreme and is not well-defended in the text. This is 50-67% less than the average meltwater production estimated prior to the collapse of Larsen B (200-300 according to the text). How was this threshold "empirically" chosen? Was it just based on Larsen A/B?
k) L291: How do you define "likely" or "very likely"?

3) Figure 1

L148: It seems a bit of a stretch to say that the extension of the RCM simulation is suitable for 25 years over the grounded ice sheet…. Really, there is just one year anomalously high SMB year at ~2125. Otherwise, the original MAR simulation and the reconstruction have opposite trends, even during this first 25 years.

In general, Figure 1 is concerning for me. It seems that this reconstruction method cannot be applied in a warming climate. However, the authors do apply some sort of reconstruction to obtain the results in Figures 6-12. How were these reconstructions obtained when Figure 1 demonstrates issues for applying this method in a warming climate? How can we trust the results presented here in light of Figure 1?

4) Relation to previous studies

In general, this manuscript is lacking some references to and context within recent literature. For example, how do the results in section 3.3 add to or fit within the context of previous ice-shelf potential instability studies (i.e. van Wessem et al., 2023; Dunmire et al., 2024; Alley et al., 2018; Lai et al., 2020)).

Additionally, some reverences to previous AIS SMB studies are missing (e.g., Gorte et al., 2020, Noel et al 2023).

5) Finally, the motivation for this work, and specifically how this method could be used in the context of ISMIP7, should be elaborated.

Minor comments

L7: "After correcting the distribution of equilibrium climate sensitivity of 16 climate models…" From just the abstract, it is unclear what this means.

L7: "… we find a likely contribution of surface mass balance to sea level rise of 0.4 to 2.2 cm from 1900 to 2010…". It does not make sense that the contribution of SMB to SLR would be positive for this period so I'm assuming this is with respect to a reference period? Same for the SLR contribution ranges in the following lines?

L25: "Hydrofracturing may strongly enhance the contribution of upstream glaciers to sea level rise." This is a bit misleading. The papers cited (among other work) indicate that *the removal/collapse of ice-shelves (perhaps due to hydrofracture events) causes a speed-up of upstream glaciers*, not just the hydrofracture event itself.

L41: "Because of these difficulties, only… which is generally insufficient to sample the CMIP model diversity." The "- when produced -" in this sentence threw me off a bit and I had to read it a few times to understand what was being said.

L43: "… correct unrealistic Equilibrium Climate Sensitivity…" A brief explanation for this concept would be helpful here.

L45: "Over the years, Antarctic Ice Sheet modellers have often scaled their best estimates of present-day accumulation to temperature anomalies from the CMIP models…)". This sentence fragment is unclear to me.

L67: "The surface mass balance and melting… Donat-Magnin et al (2020) and Kittel et al (2021)." I think a brief explanation of the results of these papers would be helpful here. I am left wondering: And how does MAR do in comparison to observational products?

L101: Assuming that all precipitation is entirely made of snow is a big assumption to make, especially for projections that extend to 2200 in high-emission scenarios. The impact of this assumption should at least be discussed somewhere in the paper.

L132-134: Should this really be interpreted as a 'mass loss rate' if Figure 1 shows anomaly values with respect to a reference period? The SMB for the reference period is positive (although the specific reference period value from MAR should be mentioned somewhere in the paper). Even though the line in Figure 1b decreases throughout the timeseries, mass loss doesn't occur until it reaches the negative magnitude of the reference period. For example, if the reference period SMB for ice-shelves ~500 Gt/yr, then surface mass loss doesn't really occur until approximately 2120 (when the time series reaches -500 Gt/yr).

L138: "… although this is still an improvement compared to the original IPSL-CM6A-LR outputs". I think it would be very interesting to have these original ESM timeseries plotted in Figure 1 as well.

L123: "… covering the aforementioned range, i.e. 0.5 to 0.9." This range is different from that mentioned before (0.6-0.85, L98).

Section 2.2.3: Somewhere in this section it should be clarified that Equation 4 is used to do this reconstruction.

L179-181: "The realistic SMB reconstructions derived from MAR-ACCESS1.3 are mostly compensations between overestimated melt and overestimated accumulation". Why do you say this?

L210: "… with a 20-year transition." What does this mean?

L238: Figure 8 is mentioned before Figure 7

L253: What does “weaker SMB” mean?

L265: “Spatially, a net surface mass loss arises for several ice shelves…” I find this to be a bit misleading since Figure 7 shows SMB anomalies from a reference period, not absolute SMB. Is there actually a net surface mass loss or is it just lower than the reference period?

L294 – It should be mentioned that George VI ice shelf has compressive stresses which do not promote hydrofracture occurance (Labarbera et al., 2011)

L 311: What is the A1B scenario?

Technical corrections

L33: progresses → progress

L62: Citation needed for the pore close-off density.

L180: “ooverestimated”

L185: “emulation” → emulations

L234: “scenario” → “scenarios”

L 254: End parenthesis after “(Fig. 9.”

L262: “to the exception” → “with the exception”

Figure 8 caption: “same as Fig. ?? is also shown”

References used in this review

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