Review of egusphere-2025-2192: Investigating the multi-millennial evolution and stability of the Greenland ice sheet using remapped surface mass balance forcing

Rahlves et al. adapt an SMB remapping method developed by Goelzer et al. (2020) in order to parameterize the effect of the melt/SMB-elevation feedback in long term standalone Greenland ice sheet simulations.
Fully coupled climate and ice sheet simulations are not possible on long time scales and uncoupled ones don't take into account the effect of the melt/elevation feedback due to the use of a fixed ice geometry. As confirmed by Rahlves et al. in this study, while not taking this feedback into account doesn't matter much over the next century, ignoring the increased melt resulting from it leads to important differences in projected mass loss on millennial time scales. Surface mass balance is the link between the atmosphere and the ice sheet and finding an efficient way to parameterize the effect of the melt/elevation feedback on the SMB when it is used as forcing in an ice sheet model simulation is therefore crucial for the study of the long term stability of the GrIS. This new computationally efficient way of including the melt/elevation feedback in standalone ice sheet simulations proposed by Rahlves et al. is therefore extremely relevant to cryospheric science.

The paper is generally very well written and justified throughout. In particular, I really liked the thorough summary of available methods and their shortcomings and advantages. I only have relatively minor comments and recommend that this paper is accepted, provided that the authors address the following comments.

1. Minor comments

1.1 General comment on reanalysis/ESM-forced MAR SMB and CISM

The SMB you're using in this study is an output of MAR, whose boundaries have been forced by a range of ESMs and reanalysis, but it's sometimes a bit vague in the text after the initial mention. Even if, here, the models are not the primary focus of the paper, it's easy for the reader to forget the SMB doesn't directly come from the reanalysis or ESMs (especially since at least UKESM and CESM can compute their own SMB) without a reminder here and there in the text.

In particular, I would modify the following sentences and sections:

- p5, line 137: **The historical run is forced with MAR-dowscaled ERA5 SMB and ST.** Written this way it makes it sound like MAR directly downscales the SMB calculated in ERA5 whereas it downscales fields like temperature and humidity and calculates SMB in its own surface/snow module. Can you rewrite the sentence to make it less ambiguous?

- p6, section 2.3.1: various things in this sections are a bit confusing.
  First, I would change the first sentence a bit (see below).
  Then, below eq. (1) I would change SMB_ref_ERA5 to SMB_ref to keep the description of more general and not attach it to a specific reanalysis/model.

Also, anomaly-based SMB methods are pretty common to e.g. address problems linked to possibly large biases in ESMs but it would still be nice to have a mention of why.

→ You could modify the text with something along the lines of
As a baseline experiment, we compute the SMB at time step t as the reference SMB (here ERA5-forced MAR SMB over 1960-1989) to which we add anomalies of the respective ESM SMB with respect to its mean over the reference period (1960-1989):

SMB(t) = SMB_ref + SMB_anomaly(t) (1)

where SMB_ref is the reference SMB and SMB_anomaly(t) is the ESM anomaly at time step t, i.e.

SMB_anomaly(t) = SMB_ESM(t)-SMB_ref_ESM (2)

In this approach, … not accounted for. As is often the case (refs), we also use anomalies with respect to a reference SMB field because *…* .

- p14, section 3.3 Sensitivity to ESM and SSP: After the list of forcing ESMs and scenarios, you could mention that the MAR SMB and CISM forced simulations are later referred to by the name of the forcing ESM and scenario to remind the reader one last time that the SMB is computed in the ESMs themselves.

Finally, as some of the forcing ESMs also work as fully coupled climate and ice sheet models, I would mention CISM here and there as well to further remind the reader that, when they read e.g. UKESM1-0-LL-SSP-8.5 in the legend of figure 9, the forcing ESM is just the first step in a "3-part simulation", i.e. the UKESM climate forced MAR boundaries, which computes the SMB that is remapped and finally used as forcing in CISM.

1.2 Specific comments

- p2, lines 25-29: here you mention that not taking into account the melt-elevation feedback leads to large biases in mass loss over large timescales but you only mention that SMB from RCMs is mostly computed on a fixed geometry much later in your review of methods. If possible, I would move it forward to this part of the introduction — if you can manage to do that without disrupting the flow later in the introduction.

- p2, lines 44-51: Sellevold et al. (2019) and Petrini et al. (2025) both use an elevation class downscaling method but in a 1-way coupling where the ice sheet geometry changes aren't known by the atmosphere and land surface (either because the ISM isn't communicating back to the atmosphere in the case of Sellevold or because the outputs of the ESM force a standalone ISM simulation in Petrini). As shown by Feenstra et al. (2025) in their comparison between a 1-way and 2-way coupled CESM-CISM simulation, this can lead to biases in

simulated SMB and mass loss. Since the elevation class method is also commonly used in fully coupled ESM-ISM like UKESM-ice (Smith et al., 2021) and CESM-CISM (Feenstra et al. 2025) and, as you already mention fully coupled ESM-ISM earlier in the introduction, it would be worth mentioning this distinction.

Feenstra et al, 2025: Role of elevation feedbacks and ice sheet–climate interactions on future Greenland ice sheet melt, https://doi.org/10.5194/tc-19-2289-2025

- p5, line 148: **Beyond 2100, the forcing is extended by averaging the final 20 years (2080–2100) and repeating this mean value at annual time steps. We verify that shuffling the sequence within this window does not significantly affect the results.**
  I only understood that you meant that it doesn't really matter wether you use a 20-year average of SMB or if you use SMB from individual years randomly shuffled within that time period in the discussion (when you write **compared to a repeated shuffling of the yearly forcing**). Could you rewrite the second sentence to make it more clear?

- p6, eq 3 + L169: use Δh instead of dh as you did in equation 5. If I remember my calculus classes correctly, d or ∂ are used for rates (as in dRU/dz) whereas differences/ranges should be written as Δ.

- p8, line 222: Figure 3 is referred to in the text before figure 2. I'd put a reference to figure 2 earlier in the text (when you first mention dividing the ice sheet into 25 basins or in step 1.1) so figures are in the order they're referred to.

- p11, line 263: does **original forcing field** refer to the fixed elevation SMB anomaly of NORESM-forced MAR SMB with respect to the ERA-forced reference SMB (from section 2.3.1)? In any case, can you refer directly to Fig. 4a there to make the read easier?

- p12, line 301: isn't the parameterized SMB-elevation feedback simulation the one with a final volume of around 1.6 x 10^{18} Gt (green line) and the 2.4 one the fixed geometry one (blue line)? Also, it should be 10^{6} Gt according to the figures and not 10^{18}.

2. Figures

Most of the figures (apart from 1 and 2) are quite narrow and would benefit from taking the whole width of the page. Figures 3 and 6, in particular, have many panels and it's difficult to see the details mentioned in the text without zooming in a lot.

3. Typos and grammar

- p2, L59: standalone. You use the hyphenated stand-alone throughout the manuscript except for this one

- p3, L66 + p18, L 413: physically-based instead of physically based for consistency. Later you use hyphenated versions, e.g. temperature-based, gradient-based, similarity-based …
- p3, L67: remove one of in ‘at the surface of of the ice sheet’
- p3, L86: extra space in ‘consistent/ uniform’
- p5, L129: van den Broeke et al. instead of Brooke et al. + p20 for the reference
- p5, L132: Pollard and DeConto (Capital D and C and no space between De and Conto) instead of Pollard and Conto + p23 for the reference
- p5, L141 + p14, L318: downscaled instead of down-scaled as you use downscaled most of the time
- p6, l163: calculating the SMB forcing (missing r)
- p16, L339: remove comma between parameterization and partially
- p17, L368: there might be an extra space at the beginning of this sentence