Reply to comments by the Reviewers: The influence of present-day regional surface mass balance uncertainties on the future evolution of the Antarctic Ice Sheet (egusphere-2023-2233)

We thank all Reviewers for their time and effort in reviewing this manuscript. Please find our detailed response to the comments by the reviewers below.

(Original report cited in italics)

Reply to Reviewer 1:

I appreciate the authors' efforts to implement the suggestions provided by the reviewers. I believe that this has significantly improved the quality of the manuscript. I recommend accepting the manuscript once the following minor comments are implemented:
We thank the reviewer for the effort and the given detailed remarks and comments, which substantially improved the manuscript.

- Figure 2 caption: ' Freely evolving ensemble simulations are indicated by solid lines' --> This is confusing as the thermal spinups with a fixed geometry are also represented by solid lines. Please adjust.
Thank you for pointing this out. In detail the thermal spinup is indicated by a bold solid line, while the freely evolving simulation is indicated by a thin/normal solid line. To avoid confusion, we now explicitly state this in the caption.

- l.222: refer to panels a—e
Adapted

- l.229: refer to panels f—j
Adapted

- Figure 6 caption: Specify again that the changes represented in this figure are changes in volume above flotation.
Now reads as: “Time series of the median (solid lines) total ice mass above flotation change (a-c) […]”.

- l.233: The changes in grounded and floating ice areas are no longer represented, please adjust.
We apologize for this mistake. The adjusted text now reads as: “The evolution of the total ice volume is shown in Fig. 6 […]”.

- section 3.2.2: I suggest clarifying in the text that delta_h represents the deviation from the common mean.
We now quickly remind the reader that Δh measures the thickness deviation from the common mean as well as referring to Equation 3. Line 278 now reads like: “The spatial distribution of Δh

(thickness deviation from the common mean; cmp. Eq. 3) […]"

- l.307-308: This is indeed surprising. It would be interesting to provide an explanation/hypothesis for this behaviour.
Indeed, this is extremely interesting. A potential explanation might be given by the fact that the regional SMB around Pine Island and Thwaites glacier is the smallest in COSMO (cmp. Fig. A2), which would explain why a collapse under PI-control conditions happens with COSMO as the baseline forcing in parameter configuration No. 12. The reason, why the PI-control simulation leads to a collapse while the projection does not, might be the fact that in the RCP scenarios an initial increase of local SMB for substantial parts of WAIS is observable, as illustrated in the Figure below for RCP2.6. This increase might again stabilize the WAIS, while the only moderately increasing ocean temperatures are not sufficient to destabilize Thwaites. However, we have not explicitly tested this hypothesis since it only appeared in one specific parameter configuration which in general seemed to be extremely sensitive to SMB differences across all scenarios (cmp. Fig. C6 – C9).

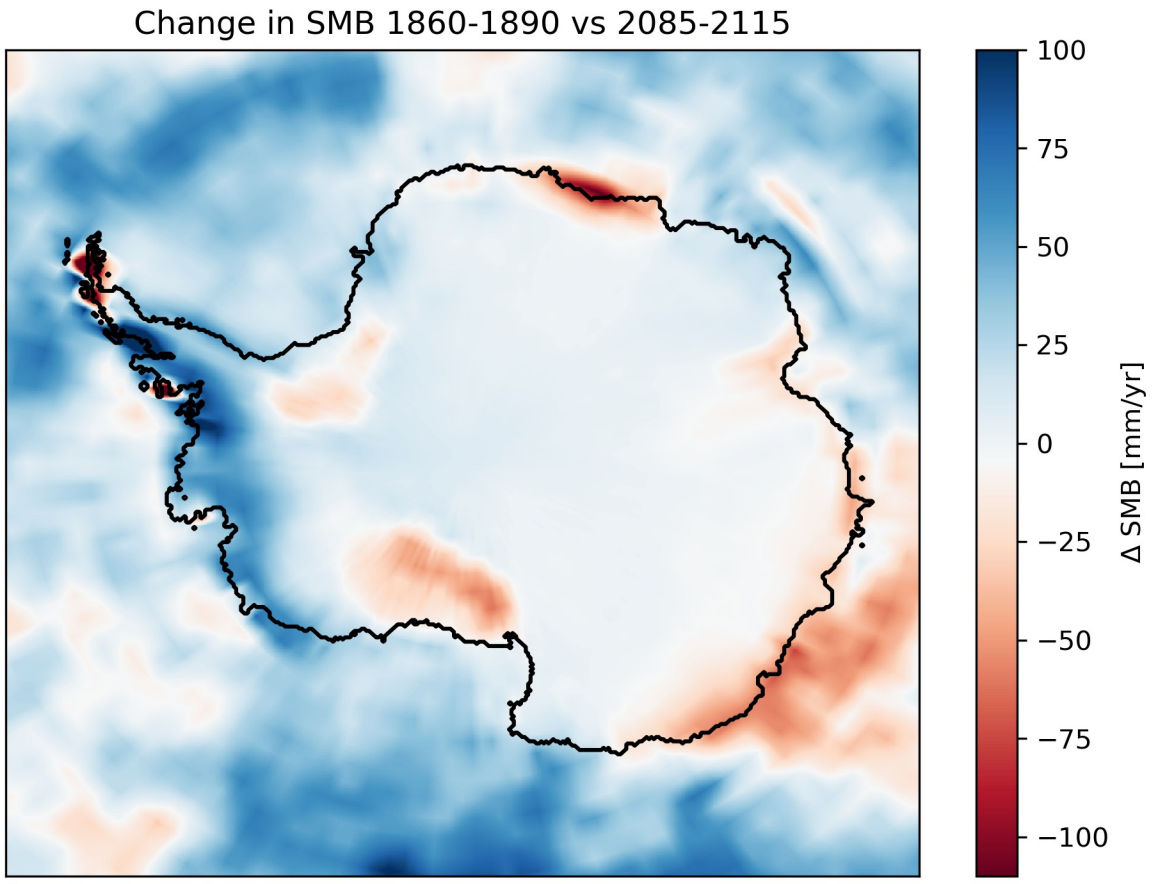

Figure 1: Difference between 30-year averaged SMB in the RCP2.6 scenario between the period from 1860-1890 and 2085-2115.

- l.355: I suggest replacing 'large differences' by 'differences'.
Adapted.

- l.370: 'COSMOS' --> 'COSMO'
Corrected.

- l.370-371: I still don't see why a retreat of the grounding line would necessarily imply reduced buttressing. I suggest removing this sentence altogether.
We agree with the reviewer, that there might not be a direct causality between a grounding line retreat and a reduction of ice shelf buttressing. What we refer to here is the fact that ungrounding will lead to reduced/zero basal friction and therefore acceleration of ice flow. We removed buttressing from the sentence which now reads: "[...] which leads to accelerated ice flow."

- l.421: I suggest replacing 'large' by 'potentially large'.
Adapted.

Reply to Reviewer 2:

The authors study the influence of the initial SMB-baseline to force the ice sheet model PISM. Compared to their first version, I found the manuscript greatly improved. The authors gave satisfactory answers to all the reviewers' comments and included them into their manuscript. I only have a technical suggestion and recommend the publication of the manuscript in TC.
We are grateful to Christoph Kittel for his insightful remarks and valuable comments, which have significantly enhanced the quality of the manuscript.

About the method scheme, if one only reads the scheme, one could think that the SMB for projections is also a future SMB simulated by the RCM while it is still the present SMB. It is well explained in the method, it could perhaps be also highlighted in the scheme.

To make this even clearer in the scheme itself, we adapted the Figure such that its clear the projections are performed by using the baseline RCM forcings together with HadGem2-ES anomalies.

I'd also mention it again in section 3.2 (especially P12 - L232-234) as keeping the present SMB should have a strong impact on projections (SMB doesn't increase until ~ 2100 before decreasing).

We now also remind the reader that our projections are done by adding GCM anomalies to the respective baseline RCMs . The section now reads as: "To investigate the effect of differences in the underlying RCM baseline data in climate scenarios we simulated the historical period from

1860 to 2005, followed by the RCP2.6, RCP4.5, and RCP8.5 scenario until 2300 by additionally applying GCM anomalies to the baseline RCM forcing on a 8 km grid resolution."

Similarly, P20 335, the authors mention that the RCM base-line selection could become even more important for long projections. I'd even go further in the implications as mentioned by other studies (eg., Coulon et al., 2024), the SMB itself should become more important compared to other processes such as basal melt. I think that the combination of RCM-baseline uncertainty and higher SMB importance would even increase the effect of the RCM-baseline uncertainty.

General, we agree that the importance of the RCM baseline uncertainty will increase with increasing impact of the SMB. Especially, for studies of the long-term (e.g., until the year 3000) commitment of Antarctic sea level contributions, this should be considered. One caveat would be that this also depends on the magnitude of basal melting which is crucial for ice sheet stability. Thus, the relative importance of SMB and basal melt would have to be considered on a case-by-case basis. For the period until 2300 a reference like the ISMIP6 ensemble will be instructive, such that one could better estimate the influence of the baseline SMB compared to other processed (e.g. basal melt).

I'd like to thank the authors for all their work.

Best regards,
C. Kittel

PS: I think there is a mistake in your answer to R1:

Integrated from 2015 to 2100, our PI-control simulations show a mean drift of 50939 Gt (MAR) -1098 Gt (RACMO) -8536 Gt (HIRHAM) and –46946 Gt (MAR)
I guess the last model should be COSMO.

This is indeed right. Apologies for this mistake and thank you for pointing this out. Correctly it should be *50939 Gt (MAR) -1098 Gt (RACMO) -8536 Gt (HIRHAM) and –46946 Gt (COSMO).*

Reply to Reviewer 3:
We want to thank the Reviewer for his work and his comments to improve this manuscript.

Minor changes:

Figure 6.
Remove black legend line "RCP"
Removed.

Figure 8:

Please mention what runs are shown. e.g. all regional model and refer to the table that mentions the ensemble number 12

The boxes in Figure 8 indicate the values within the 25th to 75th percentile, while the diamonds indicate all values outside of that interval. We now state this in the caption and additionally refer to Table C1.

Finally, we again want to thank Christoph Kittel, the two Anonymous Referees as well as Xavier Fettweis, for their thoughtful comments, which substantially helped to improve this manuscript.

Best regards,

Christian Wirths

(On behalf of all authors)