Reply to comments by Reviewer 1: The influence of present-day regional surface mass balance uncertainties on the future evolution of the Antarctic Ice Sheet (egusphere-2023-2233)

Summary of Changes

We are grateful to the reviewer for evaluating our work, and the valuable and constructive comments that help improve the manuscript. To address the major comments, we now

- include individual thermal spinups for every RCM in the present-day equilibrium simulations.
- adapt the spinup for centennial-scale projections to limit model drift under preindustrial conditions.
- perform additional pre-industrial control runs.

Below, we respond to the reviewer's individual comments in detail and describe the actions we took to address them.

Detailed response

(Original report cited in italics)

General remarks

In this study, the authors investigate how the applied present-day atmospheric climatology (specifically surface mass balance and air temperature) influences the simulated evolution of the Antarctic ice sheet. They employ outputs from four regional climate models (MAR, RACMO, COSMO, and HIRHAM), all boundary-forced by the ERA-interim climate reanalysis. To gauge the impact of the present-day climatology, the ice-sheet model PISM is used in two sets of experiments. In the first, the ice sheet evolves over 30 000 years under a constant present-day climate. In the second, Antarctic simulations spanning 1860 to 2300 are generated by adding HadGEM-ES anomalies to the respective RCM present-day climatologies. In both cases, for each RCM, an ensemble of simulations is run, covering uncertainties in model parameters such as enhancement factors, sliding parameters, and oceanic heat conductivity.

I appreciate the study's focus on quantifying uncertainties related to the atmospheric boundary conditions (and especially the surface mass balance) derived by regional climate models. I also value the concept of applying an ensemble of simulations sampling uncertainties in model structure for each RCM. However, I have concerns about the *methodology employed in the study, particularly regarding the model initialisation procedure.*

In the PD-equilibrium experiment, the authors notably assess which RCM present-day climate triggers the greatest ice-sheet deviation from present-day observations. However, these results may be biased by the fact that the thermal spin-up is performed using RACMO's surface air temperature field. In my view, a more robust approach would involve conducting the thermal spin-up individually for each RCM. Alternatively, the thermal spin-up could use ERA-interim as direct boundary conditions (similar to the approach by Li et al., 2023, where ERA5 is employed to approximate the present-day climate).

The Reviewer raises an important point here. We therefore now perform individual thermal spinups for every RCM forcing and use those to carry out the long-term equilibrium simulations. The revised manuscript will feature these new simulations. Figure R1 illustrates the updated simulation results corresponding to Figure 3 from the initial manuscript. This change in initialization procedure resulted in an overall decrease of the ensemble mean ice mass change. However, the relative changes we focus on stay rather unaffected. On the level of individual simulation, we observe changes between the old RACMO thermal- and the individual spinup. Nevertheless, one of our main findings (ISM parameter configurations in which one RCM forcing triggers a (partial) collapse of the WAIS while other do not) still holds.

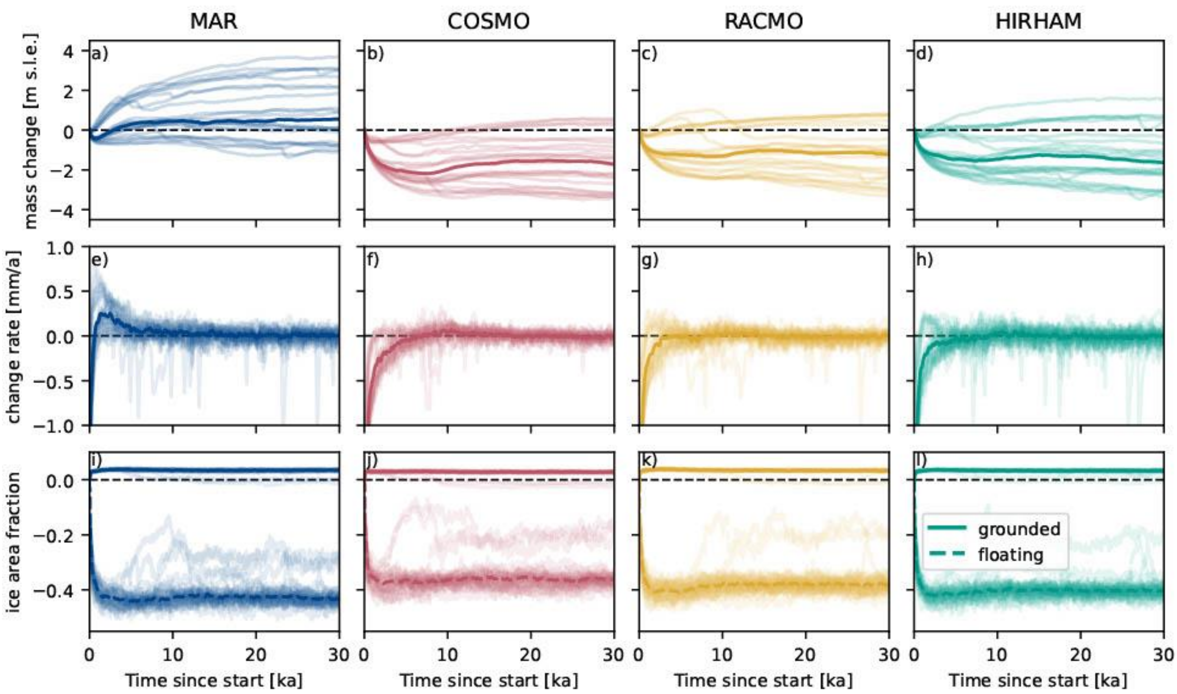

Figure R1

In their future projections experiment, the authors quantify the uncertainty arising from the choice in RCM baseline climatology and compare it with the spread observed in the ISMIP6 *ensemble. However, I feel that the sea-level projections produced in this study are significantly influenced by the initialisation procedure. Based on my interpretation of Figure 2 and section 2.2 (if incorrect, I recommend clarifying the methods section), it appears that the simulations spanning 1860-2300 initiate directly from the fixed geometry thermal spin-up. If this is indeed the case, I believe it induces significant model drift, stemming from (i) the transition in the parameter-set model parameters for each ensemble member, (ii) the shift from the RACMO climatology used in the thermal spin-up to the present-day climatology of the respective investigated RCMs, and (iii) the abrupt imposition of pre-industrial anomalies derived from HadGEM-ES, while suddenly allowing the ice-sheet geometry to evolve. Model drift can be gauged by comparing control runs in Figure D1 (though it would be better approximated by a control run with a constant pre-industrial climate): the spread among the control runs from the four RCMs is similar to that observed in the RCP projections. Therefore, my impression is that the modelled responses stem more from model drift rather than from the climate forcing itself (especially given that the HadGEM anomalies are consistent across all simulations).*

The reviewer correctly understood our methodology for our centennial ISM-projections. We also agree with the reviewer that our simulations are subject to change due to the transition in the model parameters. In contrast, the divergence of individual runs due to the difference in the underling RCM forcing is in fact the subject of our investigation. Nevertheless, we acknowledge the necessity to quantify the model-specific drift under PI conditions. Therefore, we will provide additional PI control runs for a revised version of the manuscript.

While I acknowledge that the study's aim is to quantify the influence of different forcings on future projections rather than to generate robust Antarctic sea-level projections, the results are nonetheless compared to such robust projections (i.e., the ISMIP6 ensemble). Given that current sea-level estimates prioritise minimal model drift by initialising the ice-sheet model with the starting climatology (whether pre-industrial, 1950, or present-day climatology, as seen in studies by, e.g., Seroussi et al., 2020; Reese et al., 2023; Coulon et al., 2023; Klose et al., 2023; Li et al., 2023), I find it challenging to grasp the value and interpretation of the numbers presented here.

We would like to point out, that the ISMIP6 ensemble is associated with significant uncertainties and (in case of individual model contributions) featuring very large model drift (see supplements of the respective paper, specifically table B2). Our model drift is actually within the range of comparable model contributions in ISMIP6. This being said, our study aims to quantify the impact of different RCM baseline forcings on centennial changes in Antartica, rather than producing robust sea level rise projections. However, we acknowledge the fact that our findings will be more robust if our simulations would feature relatively small model drift. To achieve this, we employ a thermal spinup using the mean of all four RCMs modified with PI anomalies, referred to as PI forcing. From there we initialize 18 individual freely evolving simulations forced by constant PI forcing for 300 years on 8km resolution. After those 300 years all our applied parameter combinations feature annual sea level contributions of less than 0.15 mm/yr (currently observed rates are ~0.3 mm/yr (Smith et al. 2020)), with close to observation grounding zone positions and overall smaller-than-PD thinning rates for the WAIS (Smith et al. 2020). In a next step, for every parameter configuration we branch off simulations with the individual RCM forcings and historical and RCP anomalies, as well as PI control runs using either one of the four RCM forcings + PI anomalies or the mean of all four RCM forcings + PI anomalies. By doing so we can ensure considerably smaller model drift than in the previous version. Below you find an updated version of Figure 2, illustrating the new setup.

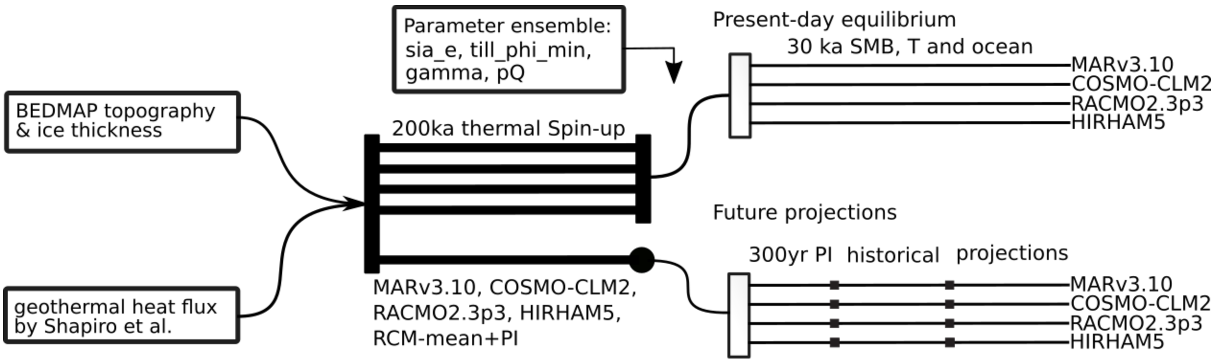

Figure R2

If my understanding is accurate and the aforementioned points are applicable, I believe that the model initialisation procedure should be reconsidered, ensuring that the simulations start from an ice-sheet configuration in equilibrium with the initial pre-industrial boundary conditions (see, for example, the initialisation procedures in Li et al., 2023, Reese et al., 2023, Klose et al., 2023). It is worth noting, however, that even if such a strategy is applied to the present study's investigation of the four RCM present-day climatologies + GCM anomalies, it may be that the spread in different projections would result more from geometry differences arising during initialisation (and therefore potentially considered as 'initial state uncertainty') rather than from variations in the different RCM climatologies, to which the ice-sheet initial state is equilibrated. This is because identical temperature and SMB anomalies are added to these respective RCM present-day climatologies. Instead, the authors may consider investigating the spread due to different RCMs projections forced at their boundaries by identical GCM projections. Alternatively, they could apply an approach similar to that of Li et al. 2023, Klose et al., 2023, or Coulon et al., 2023, where climate models air temperatures and precipitation rates (in the case of the latter two, anomalies are added to RCM present-day climatologies) are corrected for elevation changes and used as input to a positive degree day scheme which then calculates surface melt and runoff amounts.

The overall aim of this study is to investigate the interplay between different RCM forcings and the Antarctic ice sheet system to quantify the impact of the choice of RCM baseline forcing.

In the following we want to quickly motivate our methodology. Generally, one could describe the simulated ice sheet state at time t by S(S_0, F_0, F_ano, θ, t). Here, S depends on several driving components which are the initial state S_0, the baseline forcing F_0, time dependent forcing anomalies F_ano (please note that F_ano carries the entire temporal information form from time 0 to t) and the parameter configuration θ. Now one could decompose S into terms which are only affected by one driving component (in the later referred to by $\mathfrak{S}$, single), terms which are affected by a mixing of two driving components (in the later referred to by $\mathfrak{D}$, double), terms which depends on a mixing of three driving components (in the later referred to by $\mathfrak{T}$, triple), and a term dependent on a mixing of four driving components (in the later referred to by $\mathfrak{Q}$, quadruple). Mathematically, one could achieve such separation by performing a Taylor expansion and then rearranging the individual terms into the desired shape. For simplicity we briefly demonstrate this in a simplified example with only two variables x and y.

Let's define f(x,y) as a differentiable function. Then we could derive the n-th order Taylor approximation around the point (a,b) as shown below.

$$\begin{aligned}
f(x,y) &\approx \sum_{i=0}^{n}\sum_{j=0}^{n-i} \frac{1}{i!j!}\frac{\partial^{(i+j)}}{\partial x^i \partial y^j} f(a,b)(x-a)^i(y-b)^j \\
&= f(a,b) + \sum_{i=1}^{n} \frac{1}{i!}\frac{\partial^i}{\partial x^i} f(a,b)(x-a)^i + \sum_{i=1}^{n} \frac{1}{i!}\frac{\partial^i}{\partial y^i} f(a,b)(y-b)^i \\
&+ \sum_{i=1}^{n}\sum_{j=1}^{n-i} \frac{1}{i!j!}\frac{\partial^{(i+j)}}{\partial x^i \partial y^j} f(a,b)(x-a)^i(y-b)^j \\
&= f(a,b) + \mathfrak{S}_1(x) + \mathfrak{S}_2(y) + \mathfrak{D}_1(x,y)
\end{aligned}$$

One can now rearrange the terms and identify $\mathfrak{S}_1$ and $\mathfrak{S}_2$, which only depend on x and y respectively and $\mathfrak{D}_1$, which only depends on mixing terms of x and y.
The same formalism could now be applied to decompose S(S_0, F_0, F_ano, θ, t) into individual parts.

$$\begin{aligned}
\mathbf{S}(S_0, F_0, F_{ano}, \theta, t) = I &+ \mathfrak{S}_1(S_0,t) + \mathfrak{S}_2(F_0,t) + \mathfrak{S}_3(F_{ano},t) + \mathfrak{S}_4(\theta,t) \\
&+ \mathfrak{D}_1(S_0,F_0,t) + \mathfrak{D}_2(S_0,F_{ano},t) + \mathfrak{D}_3(S_0,\theta,t) \\
&+ \mathfrak{D}_4(F_0,F_{ano},t) + \mathfrak{D}_5(F_0,\theta,t) + \mathfrak{D}_6(F_{ano},\theta,t) \\
&+ \mathfrak{T}_1(S_0,F_0,F_{ano},t) + \mathfrak{T}_2(S_0,F_0,\theta,t) + \mathfrak{T}_3(S_0,F_{ano},\theta,t) + \mathfrak{T}_4(F_0,F_{ano},\theta,t) \\
&+ \mathfrak{Q}_1(S_0,F_0,F_{ano},\theta,t)
\end{aligned}$$

Note that *I* denote the ice sheet state around which the Taylor expansion is performed (equivalent to f(a,b)) and vanishes for differences of simulations. As a quick additional explanation, the term $\mathfrak{S}_1$(F_0,t), only carries the ice sheet response due to the change of the baseline forcing, while $\mathfrak{D}_4$(F_0, F_ano, t) only carries the response which occurs due to the interaction of baseline forcing and the forcing anomalies.

To study the impact of the choice of the RCM forcing, one wants now to isolate the terms depending on the baseline forcing from the rest. This can be achieved by looking at the difference between two simulations which only differ in F_0. It is important to mention that this requires the same initial state S_0 and parameter configuration θ, since otherwise terms containing S_0 or θ (but not the baseline forcing) would not vanish in the difference. In other words, if we were to compare simulations starting from different initial states, they would observe different driving stresses, which would lead to different outcomes. One could label this initial state uncertainty. For our study we want to avoid having different initial states for different RCMs, because that would also mean we would have translated some of the RCM difference into the initial state as well.

Nevertheless, for projection like simulation, one often assumes equilibrium at the pre-industrial initial state. This means all terms not containing the time dependent F_{ano} are time independent and could be summarized to the equilibrium initial state. The impact of the choice of RCM baseline forcing would then be described by terms containing both the RCM baseline (F_0) and the time dependent anomalies (F_{ano}). Those terms could be estimated by comparing different "projections" after subtracting them by their control run. We have adopted this approach in the revised version of the manuscript.

In summary, I propose two key recommendations: (i) improve the initialisation procedure for the PD-equilibrium experiment, and (ii) reconsider the approach and methodology employed in the future projections experiment. These suggestions aim to positively contribute to refining the study's methodology for a more robust outcome. I align with the authors on the significance of elucidating and quantifying uncertainties in Antarctic projections related to surface mass balance, particularly those arising from regional climate models. Therefore, I believe that the study holds significant value for the scientific community and would be well suited for the scope of The Cryosphere. However, some major issues need to be addressed to make it a valuable contribution. Also, it is important to acknowledge that adequately addressing these recommendations would require rerunning the entire set of experiments, impacting not only the results but also reshaping the manuscript and its core findings.

We thank the reviewer for the extensive review and fruitful comments and will briefly summarize our proposed actions here:

I) We improve the initialization setup for PD-equilibrium experiments, by performing individual thermal initializations for every RCM forcing.
II) We improve our projections by performing a 300-year PI-spinup simulation forced by the mean of all RCMs together with PI anomalies, to absorb the initialization shock and achieve ice sheet states with very small model drift.
III) We additionally improve the interpretability by assessing the isolated impact of the choice of RCM in interaction with GCM anomalies

Specific points

1. *Abstract, l. 10: It is not clear here what is meant by 'underlying ice sheet model parameterization'. Please clarify for better understanding.*

We refer here to the applied parametrization as representations of physical processes as well as the used parameters in those parametrizations. We will clarify this.

2. *Abstract, l.8-9: 'Uncertainties in future sea-level predictions of 8.7 (7.3-9.5) cm …' --> I find this sentence confusing, as uncertainties are mentioned, but it looks like the sea-level prediction and their uncertainties are presented. I think that it would be helpful to clarify what the numbers between brackets represent.*

We will clarify that the values represent min and max of the observed difference.

3. *Introduction, l. 24: Include a reference to Goelzer et al. 2020 ISMIP6 projections when comparing GrIS and AIS sea-level projections by 2100.*

In our study we only look at the AIS response to different RCM-forcing thus we include the Seroussi et al. 2020 ISMIP6 community paper.

4. *Introduction, l.28: I'd suggest adding more references to the concept of calibration reducing uncertainties in sea-level projections, such as, e.g., Edwards et al., 2019, Coulon et al., 2023, Nias et al., 2019, Lowry et al., 2021.*

Will be added.

5. *Introduction, l.29: please check these numbers.*

We did and they are correct., Compare to Seroussi et al. (2020) under 4.4: "Runs with HadGEM2-ES lead to significant sea level rise, with a mean ice mass loss of 96 mm SLE (standard deviation: 72 mm SLE)" and "Runs performed with CCSM4 show the largest ice mass gain, with a mean gain of 37 mm SLE (standard deviation: 34 mm SLE)"

6. *Introduction, l.31-32: I'd suggest adding a reference to Coulon et al. 2023 here, as they investigate uncertainties in ice-ocean and ice-atmosphere interactions.*

Will be added.

7. *Introduction, l.46: I'd suggest specifying that there is no specific reason to exclusively use one model given that other RCMs such as MAR are also designed to simulate polar regions by accounting for these processes.*

We will clarify this.

8. *Introduction, l.48: Seroussi et al. showed the influence of the choice of the GCM used to derive the forcing on Antarctic projections and not on its equilibrium state. Also, I don't think that they isolated the specific influence on the SMB, as the oceanic forcings also vary for each GCM.*

They did not isolate that. However, they showed that projections heavily depend on the applied GCM. We will clarify that. This Includes differences in ocean conditions as well.

9. *Methods, l. 65: refer to Mottram et al. 2021?*

Will be added.

10. *Figure 1: It is not clear to me from the caption what exactly is represented in the second line (figures f—j). I'd suggest clarifying this in the caption and maybe also in the figure itself.*

It shows the difference between ERA Interim and the RCMs. We will clarify this.

11. *Methods, l. 105: refer to Figure 2 here.*

Will be added.

12. *Methods, l. 112: an 8-km resolution was mentioned above, please clarify.*

The applied forcing is on 8 km resolution, while we perform our PD-equilibrium ice sheet simulations on 16 km resolution. Projections are carried out on 8 km resolution.

13. *Methods, l. 117: when is this evaluated? At the end of the 30-ky run? Please clarify, as the calibration step remains a little bit unclear so far. Also, in Table 2, an ensemble of 54 simulations is presented, which ones are the 14 selected ones? Maybe highlight them in bold in the table? It could also be interesting to visualise the obtained equilibrium ice-sheet geometry for each, maybe in the supplementary material.*

For our parameter ensemble we choose parameter combinations which after 15 ka of constant RACMO present-day forcing produced ice sheet geometries and velocities sensibly close to present-day observation. To minimize overfitting to the RACMO model we kept the ensemble spread relatively broad as seen in Figure 3. However, there might still be a RACMO bias in our ensemble. In contrast one could perform a parameter selection individually with each of the four RCM forcings and then combine all parameters. Nevertheless, this would be computationally very costly .

Table 2 only sates the individual values for the changed parameters, not all the combinations. We will add another table to the Supplements explicitly denoting every used combination.

14. *Methods, l. 138: I do not understand how the computation is rendered more cost-effective. Please clarify.*

The formulation used by us is unclear here, the computation is not rendered more cost efficient but there is just less computation to be done using our proposed methodology. We will specify this.

15. *Methods, l. 143: was an 8-km resolution also used for the thermal spin-up?*

The thermal spinup was also performed on a 16 km ice sheet model resolution as geometry is fixed so resolution does not really matter here.

16. *Methods, l. 143: I am confused by the abrupt shift from the fixed geometry thermal spin-up under present-day RACMO climatology to the RCM + 1860 anomaly climate for the historical spin-up. Why not start from an equilibrated state, i.e., as in the PD-equilibrium experiment, but for the 1860 climate, as is performed in e.g., Reese et al., 2023, Li et al., 2023, or Klose et al., 2023? Could the authors comment on this, and*

ideally show the model drift when applying constant 1860 climate for the ensemble of simulations?

As described above we will start from a PI-thermal spinup and additionally show PI model drift in the updated methods.

17. *Methods, l. 145: Does HadGEM2-ES has projections outputs available until 2300 under RCP2.6, 4.5 and 8.5? If not, how are the projections extended to 2300?*

Projected HadGEM2-ES until 2300 was provided.

18. *Methods, l. 151: Could the authors comment on why the list of ensemble parameters in Table 2 differs from the 'PD-equilibrium' experiment and what guided this choice? Also, I understand that configurations without long-term stability are no longer excluded, it would be good to clarify which ones are the ones selected by the calibration procedure.*

We perform PD-equilibrium simulations on 16km while we perform projections on 8km resolution. Therefore, other parameters were selected. Nevertheless. The applied parameters for the projection like simulations will change such that model drift under constant PI forcing is minimized as requested by the Reviewers. All applied parameter combinations will be stated in the supplements.

19. *Methods, l. 151-152: What is meant by 'model spin up' here? Is the thermal spin up, or the short historical run? Please clarify.*

Thermal spinup+historical run.

20. *Methods, l. 153: What initial ice-sheet configuration is referred to here? If my understanding is correct, the ice-sheet initial state obtained from the thermal spin-up was produced with fixed ice-sheet geometry. Deviations with respect to ice thickness should therefore be zero.*

After the thermal spinup, ice thickness deviations are zero. The sentence is a little misleading, since we wanted to state that we expect the model to have relatively large ice thickness deviations between the equilibrium state and observations, since no extensive initialization (e.g. inversion) was performed. We will clarify this.

21. *Methods, l. 155: 'have often been used in the past' --> I'd suggest adding some references to support this. I would also suggest clarifying what exactly is meant by 'simple spin-up' routines, is it the thermal spin-up?*

We will provide additional references.

22. *Figure 2, caption: 'First the model is initialized from present-day ice sheet observations. Then a 200-ka thermal spin up is performed.' à My understanding was that the initialisation was the thermal spin up itself. Here, it is implied that the spin-up is performed after a first initialisation procedure. Please clarify.*

With initialization here we mean that we regrid from the observational grid to our 16km PISM grid. From there on perform the thermal spinup.

23. *Figure 2: The figure says 'BEDMAP topography' while Bedmachine is mentioned in the manuscript, please correct. Also, I would suggest specifically writing on the figure that the thermal spin up is performed with a fixed ice-sheet geometry.*

We will clarify this.

24. *Figure 3: I'd suggest clarifying in the figure caption that these are the timeseries under constant present-day conditions, i.e., the PD-equilibrium experiment. In addition, please clarify what change rate is meant in figures (e-h). Also, I suppose from the figures that (i-l) represent the change in ice fraction area? Finally, please clarify how the total ice mass change is translated in m s.l.e? Is only the ice above floatation accounted for here?*

(e-h) are the change rate of above floatation ice mass. (i-l) shows the ice area extend relative to PD observations. Only the ice above flotation is account for here. We will clarify this in the caption.

25. *Results, l.168: Could this be influenced by the fact that the thermal spin up was performed with RACMO only? The trend would hence not be influenced by the RCM itself, but rather by the difference between the RCM surface temperature field and RACMO's one. Why not performing a thermal spin up for each RCM to exclude this possibility?*

See response above.

26. *Results, l.176: I'd refer to Figure 1 here.*

Will be added.

27. *Figure B3: Why not simply combine figures 3 and B3?*

Figure B3 was produced in reply to minor revisions by the Editor. For readability of Figure 3 we still tend to keep this as additional information in the supplements.

28. *Results, l.183-184: What is meant by 'mainly driven by ice-sheet model parameterisation' here? I think that this requires more clarification.*

Due to the model intrinsic parametrization, especially the applied heuristics calculating the till friction angle, results in anomalies to present-day observations, seen in many studies (Martin et al. 2011, Albrecht et al. 2020, Sutter et al. 2023). We will specify this in the manuscript.

29. *Results, l.188: '(effect of ice sheet model spin up and parameter choices)' --> Again, I think that this requires a bit more explanation.*

As stated above we mean here the effect the applied parameterizations and chosen parameters, as well as the chosen initial state (e.g. ice- geometry and temperature), has.

In our decomposition introduced above that would be mainly D(theta, t) and AD(S_0, theta, t). Nevertheless, we will clarify this in the manuscript together with the Point mentioned above.

30. *Results, l.190-193: Alternatively, a control run under constant present-day climate conditions used for the thermal spin up could be deduced from each simulation from the ensemble, allowing to isolate changes in the AIS due to the evolving climate for each configuration.*

We don't understand what the reviewer intends to say here referring to l.190-193.

31. *Results, l.195: It could be interesting/helpful to the reader to highlight, on one or several figures, some of the regions/locations that you refer to in the text.*

Since there are already a lot of details in the Figure, we try to avoid overloading the Figure with additional information.

32. *Figure B4: It is not clear to me what exactly is represented in Figure B4.*

Figure B4 (a) shows the mean state of the ice sheet after 30 ka when forced with the mean out of the four RCM forcings. (b) shows the difference between the ice sheet thickness depicted in (a) compared with the mean of the simulations forced with the RCMs individually.

33. *Results, l.205: Why were these specific simulations selected? Where do they lie compared to the rest of the ensemble?*

They were chosen, because they showed a collapse under one RCM forcing but not under another. Since we now intend to not discard simulations on being long term stable under RACMO forcing, all simulations will also be depicted in Figure 3.

34. *Figure 5: Writing the parameter values in each of the subfigures is confusing as it gives the impression that each parameter value is associated with the panel itself, I would suggest removing it. In addition, please clarify in the caption what experiment is represented in the figure.*

We will remove the parameters from the figure since they are named in the caption.

35. *Results, l.212: please clarify what is meant by 'similar' here.*

Collapse with one forcing but no collapse with another. For clarity, we will remove the restriction on 100 kyrs stability with RACMO forcing, such that we won't have to separate cases anymore.

36. *Results, l.216-219: in which figures can we see this? Please clarify. It would also probably be easier to indicate the parameter-set subset on the figure directly.*

Figure C1. Will be merged with Figure 5.

37. *Results, l.213-219: as a few of these simulations do not seem to have reached a steady state nor a quasi-steady-state yet, one could wonder whether running these simulations for more than 30kyr would lead to a WAIS collapse in all of the configurations, implying that the committed ice-sheet state is mainly driven by the parameter set itself, while the RCM climatology modulates the timing of the potential collapse? This is only a guess, but it could be interesting to discuss this somewhere?*

We will add this to the discussion.

38. *Results, l.223-224: Figure D1 seems like an important figure which, I believe, has its place (along with its discussion) in the main manuscript.*

Since the focus of our study is not the projection itself but the impact of the choice of RCM on the projection uncertainty, we choose to leave it in the Appendix while updating Figure 6 with additional control runs.

39. *Results, l.242-243: SMB over the ice shelves has no direct contribution to sea-level rise, but it does indirectly influence the ice-shelves stability and hence buttressing effect on the ice-sheet flow. Maybe it is worth briefly commenting on this?*

We will mention that the SMB over ice shelves has an effect on grounded ice-flow.

40. *Figures 7, D2-D3: I see no purple line on these figures. Also, the grey line does not seem to be the observed present-day grounding-line position. Are these the median grounding line positions? Also, are these the ice-sheet configurations by the end of the simulations, i.e., 2300? Please clarify.*

The purple line was removed for readability in, we will adjust the caption. The black line indicates the simulated grounding line position.

41. *Figures 7, D2-D5: I find the use of the difference to the common mean hard to read and interpret. Alternatively, a control run under constant pre-industrial climate conditions could be deduced from each simulation, allowing to isolate changes in the AIS due to the evolving climate for each configuration (something similar is performed in Li et al.'s Exp. CMIP6_RAW_1850-2100).*

We can add additional plots showing the difference to a control run. However, the aim of Figure D2-D5 is to show the difference between the RCM forcings. Comparing with the control run would also show the differences due to the GCM anomalies (compare with discussion above).

42. *Results, l.267-269: I think that this makes sense, given that the parameters included in the ensemble do not have a strong impact in this region, which is instead strongly influenced by the SMB.*

We will remove the word "surprising" in the text.

43. *Results, l.276: What about the control (i.e., constant present-day as of 2005) simulations? It could be interesting to show these as well to have a better grasp of the influence of this signal.*

We will add a panel showing the control.

44. *Figures D4-D5: It should be clarified in the figures' captions that these represent the ensemble member 10 only. Also, what do the different coloured lines represent in these figures? Overall, it would be good to clarify figure captions throughout the manuscript.*

Thank you for pointing that out. We will clarify the caption. For a revised version we'll improve the figure captions.

45. *Results, l.281-282: I don't think that I would say that the RCM baseline will 'significantly affect the onset and pacing of a marine ice sheet instability'. First, I don't believe that Figure 8, given that it is a snapshot at year 2300, allows us to draw conclusions about the pacing itself. In addition, except for the 5 and 95 percentiles, the grounding line positions are overall relatively similar. I think that it is more correct to say that the choice of the RCM baseline modulates the grounding line retreat. Also, I don't think that it makes sense to refer to a marine ice sheet instability mechanism here. We do not know whether a self-reenforcing retreat has been triggered. I would simply refer to a grounding-line retreat.*

We will remove the statement on "pacing" of the grounding line retreat as well as the usage of "marine ice sheet instability" here as we didn't assess whether our model results show a self-reenforcing retreat.

46. *Figure 8: It is not clear to me how the percentiles of grounding-line positions are calculated. Could the authors specify it in the caption?*

We calculated grounded ice mask density, which states for every gridbox i,j the percentage of simulations which have grounded ice. From there contour lines were drawn for the individual percentiles. We will add a precise description on how those percentiles were calculated to the caption.

47. *Results, l.287: Here it is referred to ensemble member n°10 while before the ensemble members were referenced using letters (AY, etc.), maybe consider using either letters or numbers for both for consistency?*

We will clarify this.

48. *Results, l.288-289:'similar to the already observed patterns in the present-day equilibrium runs' à I am not sure which figure I should refer to for the comparison, I am guessing Figure 4, but it would be good to specify.*

Indeed Figure 4, we will mention this in the text.

49. *Discussion, l.304-305: I think that this formulation is clearer than the one used in the abstract, maybe use an equivalent sentence as this one for the abstract as well? Also, I think that it is important to specify that these are the maximum differences between two RCM configurations.*

We will update the abstract guided by the sentence in the discussion.

50. *Discussion, l.306-308: I find that how both (different) numbers are compared is confusing, as, e.g., 8.7(7.3 – 9.5) represents a spread in sea-level contribution, while 9.6 +- 7.2 represents the sea-level contribution itself. I'd suggest presenting the spread of the ISMIP6 ensemble instead. The authors may also consider calculating an equivalent indicator as the 'mean maximum sea level contribution difference' on the ISMIP6 ensemble for a more robust comparison.*

We will compare with the IMSPI6 spread directly. However, since the ISMIP6 data contains simulations from different ice sheet models with different parameters used, a calculation of a 'mean maximum sea level contribution difference' as described in our Methods is not possible.

51. *Discussion, l.311: My impression is that the uncertainty presented here is instead mainly driven by the initialisation procedure. I think that this requires a more thorough discussion and presentation of control (i.e., constant pre-industrial climate) simulations.*

As discussed above we will provide extensive control runs in a revised version.

52. *Discussion, l.352: 'may be simulated'. It is in fact only for specific RCM and parameter set that divergences appear. Your median grounding line positions are in fact relatively similar.*

We are unsure to what exactly the reviewer refers in l.352.

53. *Discussion, l.329: what is meant by 'unforced' grounding-line retreat here?*

We mean not forced by GCM projection anomalies. We will clarify this in the text.

54. *Discussion, l.341&345: Grounding-line retreat does not necessarily imply reduced buttressing and hence acceleration in ice flow…*

We will correct this and only mention potential reduced buttressing.

55. *Discussion, l.355: I don't understand what is meant by 'the ice sheet gradually responds to the SMB forcing', please clarify.*

Indeed, this formulation is misleading. We mean a response in line with the SMB forcing e.g. ice thickness increases for SMB increase and vice versa. We will change the text accordingly.

56. *Discussion, l.358-359: I don't understand this. The evolution of the ice flow can be investigated with the evolution of the ice velocities through time.*

We now have checked the velocity fields. The ice divides are shifting in all runs in response to the applied forcing. However, a more in-depth analysis would be necessary to assess if this is the main driver of the observed behavior

57. *Discussion, l. 360: I find this title a little confusing. I would suggest reformulating it.*

We will simplify to: Parameter sensitivity

58. *Conclusion, l.377-378: I don't think that the differences in thickness and grounding line positions that are presented here may be considered as 'considerable' (see, especially, Figure B1)*

It is true that RCM induced differences in thickness and grounding line positions are not always considerable when compared with difference to present-day observation. However, there are cases where this is the case (see Figure 5, C1). Additionally, RCM induced differences can reach values up to several hundred meters in some regions, which is in absolute and relative terms quite considerable.

59. *Figure D1: How come that Figure D1 shows only one curve for the control runs? What parameter values are used for these control runs? For consistency, control simulations should be performed for each parameter configuration.*

We have performed control simulations for every parameter configuration. For simplicity Figure D1 only shows the ensemble mean of all control runs per RCM forcing. We will state this clearly in the caption.

60. *Appendix D, l.399: 'minor ice loss' --> I am not sure that the 1860-2005 ice loss (of several dm) can be considered 'minor'. It is the same order of magnitude as the sea-level contribution between 2100 and present under RCP8.5, as shown in Figure 6. Also, as mentioned above, I suggest moving this entire section to the main manuscript.*

Indeed, the ice loss is not minor if compared with ice-loss until the year 2100. Most of this ice loss can probably be attributed to the initialization shock when the ice starts to evolve freely. With our new simulation setup proposed above we try to absorb this shock beforehand and reach a considerable smaller drift. However, we would like to point out here, that there are many publications where the SLE difference to PD in the respective spinup/initialisation/control experiments amounts to several meters. Compared to this several dm can be considered small.

Overall,

61. *the methodology, particularly outlined in Section 2.2, is unclear. The inconsistent use of terms such as 'spin up' and 'initialisation' makes it challenging to comprehend the precise procedures, even with the aid of Figure 2, especially for the 'Future projections' experiment (section 2.2.2). Similarly, the calibration procedure, and how it varies between experiments (resulting in different parameter values) remains unclear. To enhance clarity, the study would benefit from a clear list of experiments, similar to Table 1 in Li et al. (2023), where climate forcing, initial conditions, and objectives are explicitly stated.*

Thank you for pointing this out. We will add a table describing all simulations performed. Further we will ensure consistent use of the terms"initialization" and "spinup". As described above we will also revise our methods according to the reviewer comments, therefore Section 2.2.2 is expected to change as well.

62. *the figure captions should be enhanced for consistency, providing clear information on the represented experiments, years, and the significance of various elements (e.g., grounding-line position). Improved consistency and clarity in figure captions would enhance the overall understanding of the figures and contribute to a more straightforward interpretation of the study's findings.*

As stated before we will improve the Figure captions to allow for better accessibility.

63. *the discussion lacks consideration and comparison with related works (other than ISMIP6).*

The comments by the reviewer have brought up several important publications which will be included in the discussion of a revised manuscript.

Minor comments/Typos

64. *Abstract, l.1: remove coma after 'impacts'.*
65. *Abstract, l.7: 'constant forcing quasi-equilibrium state' --> I find this formulation confusing, try to rephrase?*
66. *Abstract, l.8: 'uncertainties of' --> uncertainties in?*
67. *Abstract, last sentence: remove coma after 'importance'.*
68. *Introduction, l. 17: add come after 'Until the end of this century'*
69. *Introduction, l. 17: 'see level rise'*
70. *Introduction, l. 25: 'century's' --> centuries*
71. *Introduction, l. 32: 'The latter, estimates' --> 'Uncertainties in estimates of'?*
72. *Introduction, l.38-41: I'd suggest splitting this sentence in two.*
73. *Introduction, l.50: I'd suggest splitting this sentence in two: 'We address the following questions:…'*
74. *Methods, l. 70: 'drainage basis'*
75. *Methods, l. 70: remove come after 'All four models'*
76. *Methods, l. 83: 'togeher'*
77. *Methods, l. 86: 'Antarctic Ice sheet' --> 'Antarctic Ice Sheet' for consistency. I believe that this is the case at other places in the text, please check.*
78. *Methods, l. 90: 'shelf's'*
79. *Methods, l. 100: to improve the readability of this sentence, consider using 'two model set ups: (i) …, and (ii) …'.*
80. *Methods, l. 102: 'scenario' --> 'scenarios'.*
81. *Methods, l. 102: 'BedMachine'.*
82. *Methods, l. 104: remove come after (2004).*
83. *Methods, l. 112: 'on 16 km resolution' à 'at 16 km resolution'.*
84. *Methods, l. 113: 'RCM-'*
85. *Methods, l. 113: 'we employ' --> 'we run/produce'?*
86. *Methods, l. 118: 'An additional constrained'*
87. *Results, l. 165 and l.172: 'initialization shock' --> 'initial shock'?*
88. *Results, l. 229: 'maxmimum'*
89. *Results, l. 244: 'SMB The accumulated…'*

90. *Figure 8, caption: ‘siumaltions’*
91. *Results, l. 285-286: remove comes after ‘both’ and ‘forcing sets’*
92. *Results, l. 286: ‘chosen ice sheet model parameter choice’.*
93. *Discussion, l.300: ‘onto’ --> ‘on’?*
94. *Discussion, l.322: ‘forcing data’ --> ‘baseline climatology’?*
95. *Discussion l.341&345: ‘butsstressing’*
96. *Discussion l.345: ‘In these simulation’*

Since we expect out Manuscript to change quite significantly, we will implement those comments unless the text passage hasn't been changed.

In conclusion, we would like to thank the Reviewer for his extensive and detailed comments. We are convinced that our proposed changes will significantly improve the manuscript.

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