

Reply to Editor

Overall Comments

The Methods section of your revised manuscript is much improved. It is now much clearer that you perform the spinup in both S1 and S2 with different basal melting methods, but for the projection you go forward with one method. That means that in the case of S2, the forcing is continuous, while in S1, when you switch to projections, there is a discontinuous change in the basal melt rate being applied. Nonetheless, the additional time series you included in the review response demonstrate that this discontinuity likely does not impact the results. To be clear, the change in slope of ice volume at 2015 is probably indicative of a discontinuity and could be a cause for concern. This seems to derive from the climate forcing applied, rather than the change in melting scheme, so at least it applies equally in both cases. And the fact that the control run (which I assume is with S2 melting, but no climate changes) remains stable supports your claims. Finally, your additional test with the ΔS approach shows that most differences can be attributed to the change in scheme.

I believe therefore that the manuscript is near ready for publication, however there are still several minor changes to be made. Please address the remaining comments below.

Response: We sincerely thank the editor for the valuable and professional comments, which have steadily improved and refined our manuscript and made our research more rigorous and meaningful. We also greatly appreciate the editor's positive and encouraging feedback on our revised manuscript. Following your comments, we have revised and clarified the definitions of "S1 simulation" and "S2 simulation" throughout the manuscript. Following your provided sketch, we have also readjusted the experimental flowchart in Fig. 3. To avoid confusion, we have clearly distinguished between "difference" and "misfit" in both the figures and the text. Additionally, we have added a new appendix demonstrating that no shock occurs when transitioning from the S1 initialization to S2 forcing during the projection, thereby supporting the robustness of our results. We have also revised ambiguous wording and explanations throughout the manuscript to improve clarity and readability.

Minor comments

General: in my comments below, I have tried to address some things I noticed to be difficult to follow due to the wording/logic. Please make sure to go through the manuscript one more time and look for instances where wording and explanations can be made more clear, with a focus on straightforward sentences with precise meanings.

Response: Thanks for your suggestions. Following your comments, we have redefined the S1 and S2 simulations in the revised manuscript, replaced all ambiguous terms such as “S1-based” with clearer expressions like “S1/S2 projection”, and clearly distinguished between “misfit” and “difference” in their usage. We have also revised unclear wording and explanations throughout the text.

General: In an Appendix, please include the time series shown in Figure R3, and include another panel which shows the same for the S2 simulations. Also include R4 as an additional panel and describe this perturbed test in the Appendix too. In this way, you can refer to this Appendix briefly in the text and give the reader confidence in your methodology.

Response: Thanks for your suggestions. Following your comment and the ΔS approach suggested in your previous review, we have added an appendix demonstrating the absence of a potential shock in the “S1 simulation”, and have briefly referenced this appendix in the Methods section: “*Although “S1 simulations” are initialized with the S1 method (Eq.1) but driven by the S2 parameterization (Eq.2) for projections, their results are not contaminated by a potential shock (Appendix A).*”;

Appendix A: Validation of the Potential Shock in “S1 Simulation”

In our experimental design, “S1 simulations” are initialized with basal melting prescribed from the S1 method (Eq. 1) but driven with the S2 method (Eq. 2) for projections. “S2 simulations”, in contrast, are both initialized and projected using S2 parameterization. The time series of ice volume above flotation (VAF) for the S1 simulations transitions smoothly from the historical spin-up to the subsequent projection, exhibiting a continuous evolution with no detectable discontinuity at the transition year (Fig. A1a). The S2 simulations also display an equally smooth transition (Fig. A1b), confirming that the S2 parameterization itself does not induce abrupt volume changes when initiated from its own spin-up.

To further assess whether a potential shock arises from the S1 initial state to S2 forcing during the projection in S1 simulation, we have introduced a general equation for the melt rate calculation (Eqs. A1-1 and A1-2): From the S2 projection, we extracted the annual oceanic forcing (S_{2_proj} ; time, x , y) and subtracted the corresponding S2 initial state (S_{2_init} ; x , y) to obtain a perturbation ΔS (time, x , y ; Eq. A1-1). This ΔS was then added to the S1 spin-up (S_{1_init} ; x , y) to construct a synthetic forcing S_{s1_s2} (time, x , y ; Eq. A1-2) that represents the absolute ocean state under the S2 projection anomaly imposed on the S1 spin-up:

$$\Delta S = S_{2_proj} - S_{2_init}, \quad (A1-1)$$

$$S_{s1_s2} = S_{1_init} + \Delta S \quad (A1-2)$$

Using this $S_{S1,S2}$ forcing, we performed an additional projection starting from the $S1$ initial state. The resulting VAF time series (Fig. A1c) again shows no abrupt change, confirming the absence of a forcing shock. This can be attributed to two factors: i) our projections are driven by absolute oceanic temperature and salinity, not anomalies; this ensures that the basal melt rates calculated from Eq. 2 respond directly to the full ocean state rather than to a perturbation. Although abrupt changes in the input forcing could theoretically affect ice-sheet evolution, the CMIP5 and CMIP6 climate forcing datasets used in our projections do not exhibit such abrupt shifts. ii) the constant parameters values in the $S2$ parameterization are derived from in-situ observations or empirically calibrated against them, ensuring that the resulting basal melting closely matches the observations. Furthermore, in Fig. A1, the change in the slope of ice volume around 2015 likely indicates a discontinuity that appears to arise from the applied climate forcing rather than from the change in basal melting scheme, so it does not affect the difference between the two cases. In summary, no such shock is detected in the $S1$ simulation results when transitioning from the $S1$ initial state to $S2$ forcing.

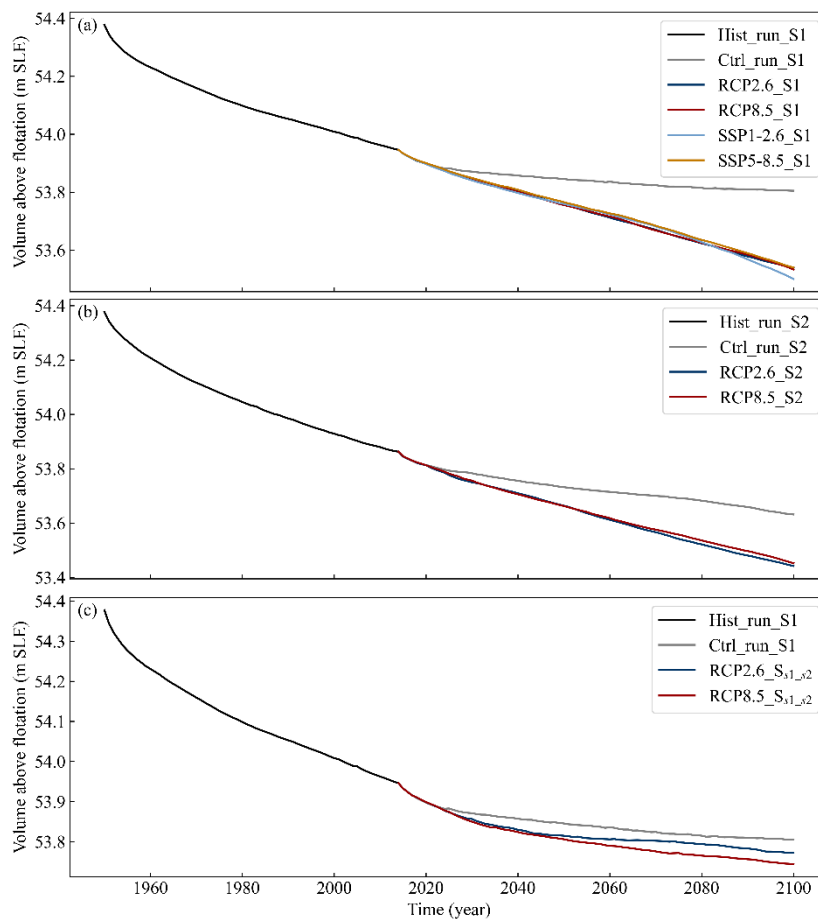


Figure A1: Ice volume above flotation (VAF) during spin-up and projection for (a) $S1$, (b) $S2$, and (c) $S_{S1,S2}$ simulations. Black lines denote the historical spin-up;

coloured lines show projections under RCP/SSP scenarios (light blue, red, dark blue, orange) and a constant-climate control run (grey).

General: In many figures, and text, the word "misfit" is used to describe the difference between two experiments. I think this can lead to confusion, since "fit" tends to indicate a target like observations. It would be good to change this wording to "difference" where possible (so instead of "Misfit thickness", "Thickness difference"). In figure labels etc, you could alternatively use the delta-symbol (" Δ ").

Response: Thanks for your suggestions. We have revised the relevant expressions to distinguish between the "misfit" of observations from simulations and the "difference" between the S1 and S2 results, and have adopted the " Δ " symbol in place of "difference" in figure labels.

Figures 2, 6, 7, 9:

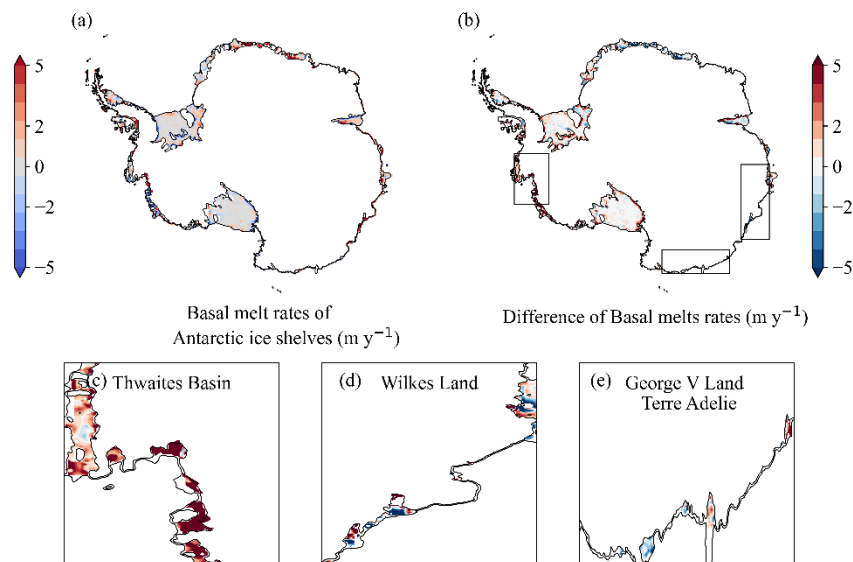


Figure 2: Comparison of sub-ice shelf melt rates between S1 and S2. (a) Sub-ice shelf melt rates derived from the TF-linear parameterization (S2). (b) Difference in basal melt rates (S1–S2). Regions of interest are highlighted by black boxes: (c) Thwaites Basin, (d) Wilkes Land, and (e) George V Land Terre Adelie.

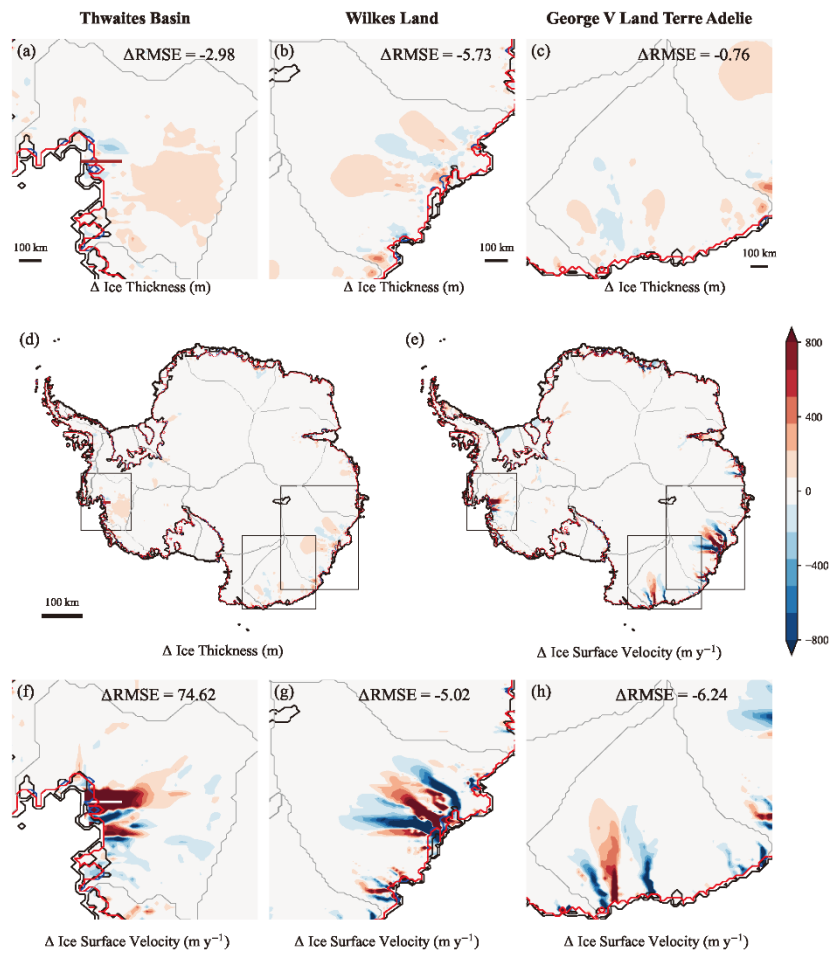


Figure 6: Comparison of spin-up ice thickness and surface velocity differences between S1 and S2. (a–c) Ice thickness differences (S1-S2) in the TB, WL, and GVL, respectively. (f–h) Ice surface velocity differences (S1-S2) in the TB, WL, and GVL, respectively. The difference in root mean square error between S1 and S2 ($\Delta\text{RMSE} = \text{RMSE}_{S1} - \text{RMSE}_{S2}$), compared to observations. (d) and (e) present the deviations in ice thickness and surface velocity between two experiments, with three black boxes highlighting regions showing the most significant discrepancies. Grounding lines: S2 (blue), S1 (red), and observed data (black) from BedMachine v.3 (Morlighem et al., 2019). The profile line locations corresponding to Fig. 8 are in Thwaites Glacier: (a), (d) brown; (f) white.

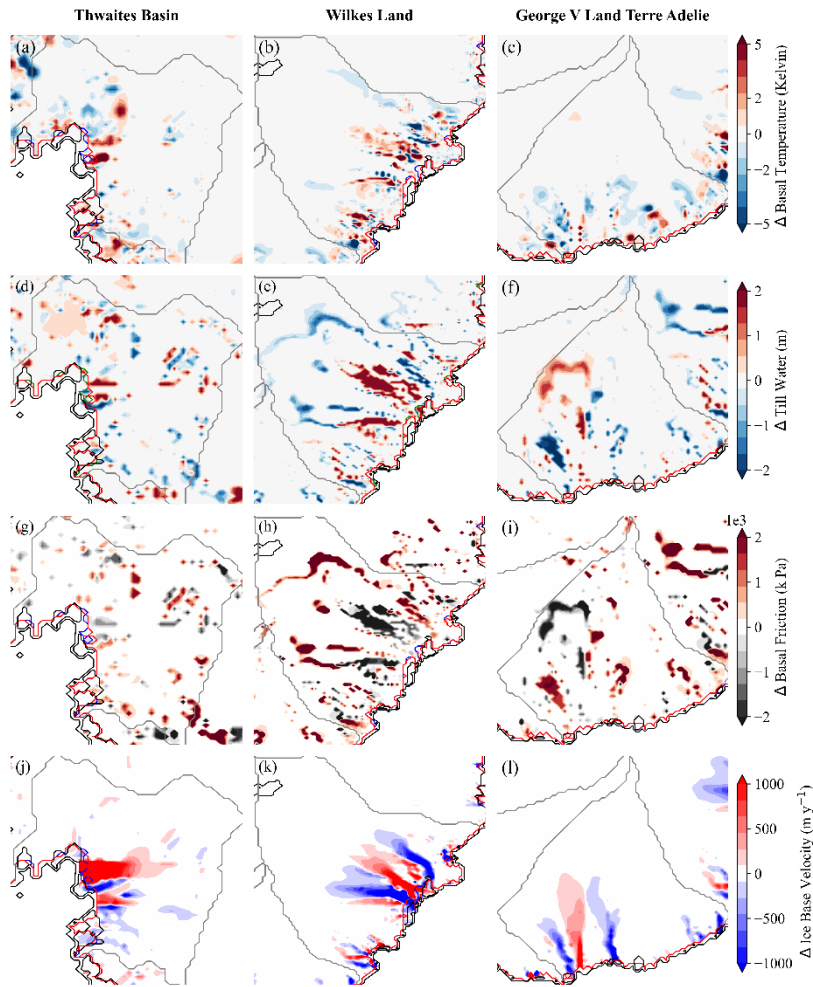


Figure 7: Spatial distribution of differences (S1-S2) in basal ice temperature field (Kelvin) (a-c), basal till water content (m) (d-f), basal friction (k Pa) (g-i), and basal velocity ($m y^{-1}$) (j-l) in the three basins.

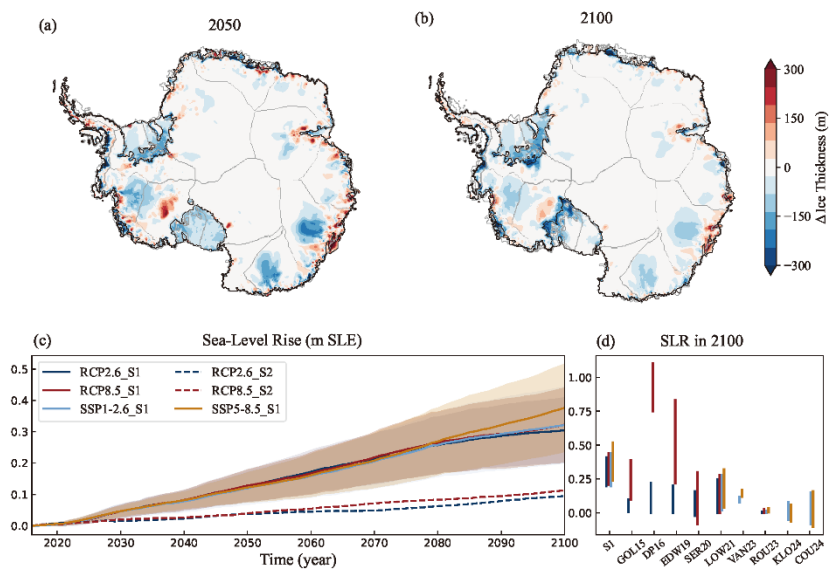


Figure 9: Ice sheet thickness differences and projected contribution of the AIS to sea-level rise (m SLE). Spatial differences in the projected ice thickness between the S1 and S2 multi-scenario ensemble means for 2050 (a) and 2100 (b). The values are derived by averaging the differences (S1–S2) under the RCP 2.6 (RCP 2.6_S1, RCP 2.6_S2) and RCP 8.5 (RCP 8.5_S1, RCP 8.5_S2) scenarios in (c). (c) Predicted sea-level rise for “SGO scenario” to “SGF scenario” simulations under four scenarios (color shading) and mean values (color lines).

Text: “Figure 4: The left column shows ice thickness and ice surface velocity results from S1, alongside their misfits from observation.”.

General: I think you should remove the term "S1-based" and "S2-based", as "based" is still ambiguous as to how the model is forced in the future. It will be simpler and cleaner to just define "S1 simulations" as those that are initialized with S1-forcing but driven with the S2-forcing for projections, while "S2 simulations" are those that are initialized with S2-forcing and also driven with S2-forcing. With that definition clear, then you can discuss e.g. an S1 simulation after the spinup, or during the projection, but conceptually, it is one continuous simulation.

Response: Thanks for your suggestions. Based on your comments in L148-152 and the sketch provided in Fig. 3, we first defined the "S1 simulation" and "S2 simulation" processes. We then removed the terms "S1-based" and "S2-based" and replaced them with the simpler, more direct expressions "S1 simulation" or "S1 projection" throughout the revised manuscript.

Equation 4: I do not think it helps to rewrite Eq. 2 here. I think you could just write "S_proj = S_2" for the equation, followed by something like "where the input temperature T_s is obtained from the projected ocean forcing". It may also be fine to leave out this equation, as long as you are quite explicit that in both S1 and S2 simulations, the projection phase is driven by the S2 method described by Eq. 2.

Response: Thanks for your suggestions. To improve clarity and avoid redundancy, we have removed Eq. 4. Following your advice, we have revised the text to explicitly state: *“The basal melting scheme was parameterized using the same linear thermodynamic framework (S2) for the ice-shelf-ocean boundary layer as that employed in LOW21—specifically, the approach defined in Eq. 2, which is driven by the annual-mean spatial distribution and time series of absolute salinity and temperature fields derived from the RCP/SSP scenarios (Jourdain et al., 2020; Nowicki et al., 2020).”.*

L148-152: Please revise these sentences, as they are rather unclear. Perhaps you can make this a new paragraph that summarizes as clearly as possible the characteristics of the set of simulations you will be presenting. Something like:

> In summary, with this approach, we produced two sets of simulations that essentially only differ in their initial state. "S1 simulations" were forced by basal melting during the spinup obtained from Eq. 1, and "S2 simulations" were forced by basal melting during the spinup obtained from Eq. 2. For the projections, both S1 and S2 simulations were forced in the same way, by basal melting obtained from Eq. 2, with temperatures from climate model projections. For the S1 simulations, projections were performed for both RCP and SSP scenarios, while the S2 simulations were only run for the RCP scenarios (since they were obtained from LOW21).

Response: Thanks for your suggestions. Following your suggestion, we have summarized the description of the initialization and projection for the S1 and S2 simulations into a single paragraph, where we clarify the similarities and differences between the two phases: *"In summary, with this approach, we produced two sets of simulations that differ essentially only in their ice-sheet initial state (Fig. 3). During spin-up, under constant ocean conditions, "S1 simulations" were forced by the basal melting derived from Eq. 1, and "S2 simulations" were forced by basal melting obtained from Eq. 2. For the projections, both S1 and S2 simulations were forced using the same method—basal melting calculated from Eq. 2, driven by salinity and temperature from climate scenarios. For S1 simulations, projections were performed for both RCP and SSP scenarios, while S2 simulations, which were obtained from LOW21, were conducted only for the RCP scenarios."*

Figure 2, Figure 2 caption: please make it clear whether the misfit you show is S1-S2 or S2-S1. I assume it is S1-S2, since this is what you used for Fig. 6 and elsewhere.

Response: Thanks for your suggestions. We have refined the figure caption and text to improve clarity:

"Figure 2: (b) Difference in basal melt rates (S1–S2).";

"Figure 6: (a–c) Ice thickness differences (S1 – S2) in the TB, WL, and GVL, respectively. (f–h) Ice surface velocity differences (S1–S2) in the TB, WL, and GVL, respectively.";

"Figure 7: Spatial distribution of differences (S1-S2) in basal ice temperature field (Kelvin) (a-c), basal till water content (m) (d-f), basal friction (k Pa) (g-i), and basal velocity (m y-1) (j-l) in the three basins.";

"Figure 9: The values are derived by averaging the differences (S1–S2) under the RCP 2.6 (RCP 2.6_S1, RCP 2.6_S2) and RCP 8.5 (RCP 8.5_S1, RCP 8.5_S2) scenarios in (c).";

"L200: This higher basal melting weakens the ice-shelf buttressing effect and

accelerates the grounded ice flow, with a corresponding 74 m y^{-1} RMSE difference from S2 (S1-S2, Fig. 6f).”

Figure 3: This figure helps somewhat for the method, but it is still not precise and could be clearer. In line with my general comment above, I would remove any mention of "S1-based" or "S2-based". Note also that I think "Atmospheric conditions" would be better as "Climate conditions", especially since the arrow feeds into the ocean boxes. Perhaps you could also try to make this figure as one continuous line with "S1 simulations" in the top row and "S2 simulations" in the bottom row. See a sketch attached for a possible idea for improvements.

Response: Thanks for your suggestions. Based on the revision comments you provided and incorporating the sketch attached, we have modified and adjusted the content in Figure 3, removed the sub-figures, merged the spin-up and projection into one single plot, and accordingly revised the caption. The revised Figure 3 and its caption are presented below:

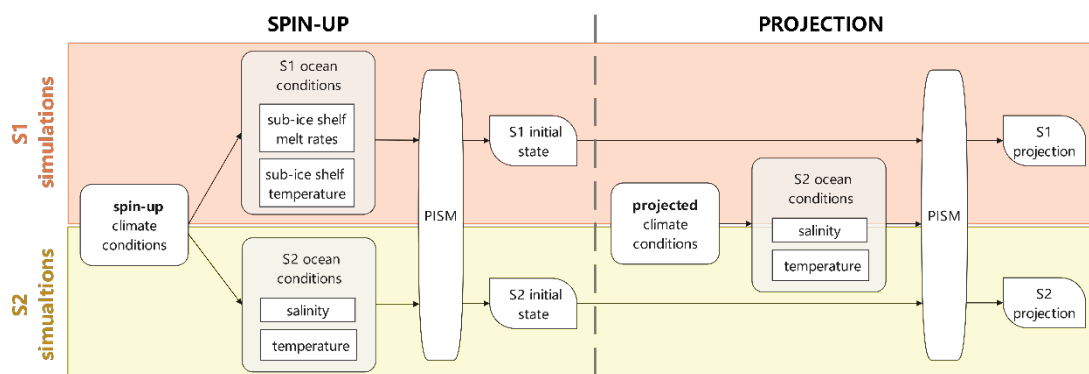


Figure 3: Overview of model spin-up and projection processes. The schematic summarizes the model setup during spin-up and projection. The ocean initialization employs two distinct schemes: using observed basal melt rates along with ice-shelf basal temperature (S1 simulations, orange box), and using Southern Ocean temperature and salinity (S2 simulations, yellow box; reproducing LOW21). Projections initialized from these two ice-sheet states and forced with an identical basal melting method (Eq. 2) as well as future oceanic conditions from 2015 to 2100.

Figure 9, caption: please make it more clear what is shown in panels (a) and (b). I assume it is somehow S1-S2 fields of ice thickness, but how do you obtain "multi-scenario (RCP 2.6 and RCP 8.5) ensemble means"? Like it is averaged over all scenarios? Is the difference pattern so similar in all scenarios?

Response: Thanks for your suggestions.

1. In Figure 9, panels (a) and (b) respectively display the spatial difference in projected ice thickness, calculated as S1 minus S2.

2. This analysis specifically utilizes the two sets of S2 projection results (corresponding to the dashed lines in (c)) supplied by Prof. Lowry from the CMIP5 IPSL-CM5A-MR RCP2.6 and RCP8.5 scenarios.
3. The calculation of “multi-scenario ensemble means” involved three steps: first, computing the difference between the S1 and S2 projections under RCP2.6; second, computing the corresponding difference under RCP8.5; and finally, averaging these two difference results. This value allows us to focus on the mean pattern of ice thickness difference in space.
4. To improve clarity, we have revised the caption to: “*Spatial differences in the projected ice thickness between the S1 and S2 multi-scenario ensemble means for 2050 (a) and 2100 (b). The values are derived by averaging the differences (S1–S2) under the RCP 2.6 (RCP 2.6_S1, RCP 2.6_S2) and RCP 8.5 (RCP 8.5_S1, RCP 8.5_S2) scenarios in (c).*”.