Response to RC2

Author responses are in blue.

In this study, the authors address a simple yet important question: What is the sensitivity of ice loss to constant, uniform calving front retreat rates in the Amundsen Sea Embayment? Calving is a major source of uncertainty in ice-sheet model projections due to the lack of robust and consistent calving laws. However, this question had not been addressed yet. By filling this gap and helping to assess the influence of calving on the loss of ice shelf buttressing capacity and, consequently, on ice sheet ice loss, this study makes a highly relevant and valuable contribution to the community.

One of the strengths of the study is the 'simplicity' of its experimental design. By prescribing constant retreat rates using the ice sheet model Ua, the authors are able to identify key features of the ice sheet response, such as specific buttressing thresholds, and demonstrate how the magnitude of the retreat modulates mass loss. The paper is very well written, concise, and easy to follow. The authors have a clear objective, which is clearly stated from the beginning, and address it in a consistent and coherent way.

Thank you for your review, and your positive comments on the study. I'm glad that the way we have approached this is appreciated, and that it will be seen as a valuable contribution.

My only concern relates to one of the study's key messages. By comparing simulations forced by ISMIP6-2300 Tier 1 ocean/atmosphere coupled model outputs with the range obtained by varying the prescribed retreat rate, the authors conclude that accurately representing calving front retreat may be as important as accurately representing sub-shelf melt rates. However, if I understand correctly, the simulations are forced using both atmospheric and oceanic outputs following the ISMIP6-2300 protocol. Therefore, the spread in simulations forced by different ESMs cannot be attributed only to ocean forcing, as it also reflects changes in surface mass balance. Typically, by 2300 under high emission scenarios, some ESMs project significant increases in both snow accumulation and surface runoff. Moreover, by accounting for a range of ESM forcing, it seems to me that the authors are addressing the variability in climate forcings themselves, rather than isolating the influence of the way ocean-induced melt is accounted for in ice-sheet models. Either the message should be reformulated to something like 'the variability in the range of ice sheet response using different retreat rates is similar to the spread resulting from a range of ESM forcings', or the methodology applied to compare the impact of retreat rate and ocean-induced variability should be adjusted. This could be done, for example, by comparing to the spread in ice sheet response obtained using different sub-shelf melt parameterisations, or varying the gamma0 value within a given parameterisation (such as the local quadratic one used here).

This is a very good point. Thank you for raising the issue. The simulations are indeed forced by both ocean and atmospheric changes from the ESMs, and in our analysis of results we have erroneously attributed all of the variability to ocean forcing. In the revised manuscript, we will be careful to point out at the first opportunity that the spread is due to both ocean and SMB forcing, and alter the wording of all following statements to refer to "climate forcing" instead of just "ocean forcing". It is also true that we are not addressing the issue of how ocean-induced melt is introduced to models, but looking at a range of different forcings which are introduced in the same way. We did not mean to give the impression that we were aiming to do the former, and will rephrase where necessary to make our intentions and the appropriate interpretation of our results clearer.

Once this point is addressed, along with the minor comments listed below, I believe this study will make a very valuable contribution to the community.

Specific comments

l.10-12: As explained above, I find the comparison between variability attributed to retreat rates and the one attributed to ocean-induced melt misleading.

Agreed. This sentence is rewritten as "We compare the variability in our results using different retreat rates to that when using ocean melt and surface mass balance forcing derived from different earth system models for ISMIP6, as these forcings are major factors in determining the future evolution of the Antarctic ice sheet."

l.76-77: It would be helpful to provide more details on this adjustment of the Bedmachine dataset, perhaps in the supplementary materials.

Yes, this will be included in an appendix in the revised version and referred to here.

l.110-115: Consider adding a table summarising the experiments, for clarity.

We have added a table into the revised manuscript.

Figure 1: Since Figures 1a–b and 1c–i convey different messages, you might consider splitting them into two separate figures: one introducing the study area and another focused on the results.

This is a fair point. We will rearrange the figures to avoid any confusion arising from multiple messages being in the same one.

Figure 2: You could try making the figure more self-explanatory, for example, by highlighting in panels (b) and (d) the regions zoomed in by panels (a) and (c).

A good idea. We will do this.

l.165: You refer to the ESM forcings as ‘ocean forcing’, but I believe that you also include atmospheric forcing through anomalies in SMB following the ISMIP6 protocol? If so, the term ‘ocean forcing’ may be misleading, as changes in SMB also have quite a significant influence on the ice sheet response.

This is being addressed as per previous comments.

l.181-182: Do you mean here that the ~70 mm range for RR0 in Figure 3a is larger than the ranges shown by the green and red shaded areas in Figure 3b? If so, it would be helpful to specify that explicitly for clarity.

This has been clarified with an additional sentence with a reference to the figure.

l.198-201: Could this be explained by increases in SMB post-2100 in UKESM as compared to UKESMrep?

A quick look at the mass balance shows a large difference in basal melt between the two post-2100, and greater variability in SMB though not a large difference in magnitude. We’ve added a clarification that of course the UKESMrep forcing does not undergo any large changes after 2100, as opposed to other models.

l.209: replace ‘sensitivity’ by ‘SLR-RR-sensitivity’ for clarity.

Done

l.220-221: I’m not sure where to visualise that the threshold is reached even for the smallest prescribed retreat rate. Clarification or reference to a figure would be useful.

We’ve clarified to specify that we’re talking about the RR0.1 simulations. We decided for simplicity to focus mainly on RR0.5 and RR1 in our figures and discussion, so this specific simulation is not on a figure in the paper. We will make the model outputs available for anybody interested in looking at them in more detail.

l.255-258: In line with my previous comments, I think that this statement should be revised to something like ‘greater than the range of forcing from the ISMIP6-2300 Tier 1 experiments’. It does not seem to me that the current experiments allow for direct comparison between the influence of calving front retreat variability and variability in sub-shelf melt representation in ice sheet models. Instead, you account for variability in climate forcings, including ocean and atmospheric forcings.

l.286-296: Same as above.

This has been altered in both cases.