

# Review of O’Neill et al. ‘ISMIP6-based Antarctic Projections to 2100: simulations with the BISICLES ice sheet model’

## General comments

This is my second review of the manuscript by O’Neill et al., which presents new ice sheet model experiments using BISICLES and following the ISMIP6 protocol to make projections of sea level rise from the Antarctic ice sheet by 2100. I am grateful to the authors for addressing my previous comments, and those of the other reviewers. The manuscript is now substantially improved. I have a few further specific comments prior to publication, which are outlined below.

## Specific comments

**Line 9:** This sentence is very similar to the one on line 5 starting “We present simulations”, I suggest combining them.

**Line 33:** not clear what would “amplify uncertainty” here, do you mean potential for marine ice sheet instability introduces uncertainty in projections? Perhaps rephrase to improve clarity.

**Line 100:** When the ice is thinner than 1 metre is it fully removed from the model domain (rather than just being set to 1 metre and effectively ignored)? In the high-end simulations that are “collapse-off” how much of the ice shelves end up thinning to 1 m and then being calved off?

**Line 170:** Be consistent between “Ronne-Filchner” here and “Filchner-Ronne” elsewhere in the manuscript.

**Line 176:** Could you say why you see this slowdown at Pine Island? has the grounding line advanced and grounded on the ridge downstream of the current position?

**Figure 3:** suggest adding labels (initials) for Amery ice shelf and Totten Glacier to panels b and c. Also label Pine Island and Thwaites in panel f, as you refer to these several times in the main text.

**Line 202:** Is that really the reason for the slow-down in the control simulation at Pine Island, or is it rather that the melt rates are not high enough close to the grounding line, or the sub-grid parameterisation, or the grounding line had grounded on the ridge and subsequently advanced?

**Figures 5 and 6:** I did not notice this before, but why do all the simulations not start at the same grounded/floating area in 2015?

**Line 219:** “increase their VAF up to -21 mm sea level contribution” reads a bit awkwardly, if saying increases to -21 best to state what this is relative to. What was the VAF contribution in the control?

**Line 222:** Restate the range here.

**Line 223:** Remove “on the one hand” and “on the other”.

**Figure 8:** Replace the numbers with letters.

**Line 245:** Suggest changing to “compare simulations using different GCM forcings but with the same ice shelf basal melt sensitivity and emission scenario”.

**Figure 9:** Are these cumulative values for SMB integrated over the grounded part of the catchment? Add this detail to the caption.

**Line 303-306:** This sentence is too long. Split into two.

**Line 327:** Add “GCM” before model.

**Line 407:** Is the reason the sea level contribution for basal melt sensitivities is similar because a large proportion of the total ice shelf area has been removed and therefore limited regions where basal melt is still applied?

**Line 415:** The start of the limitations section needs an introductory/topic sentence.

**Line 446:** Could also add something about the models ability to replicate the trend in present-day observations.

**Conclusions:** I am grateful the authors made efforts to improve the conclusions in this version. I still think it could be shorter and more concise. I do not feel you need to go into detailed examples of results for individual climate model forcings.

**Supplement:** supplementary figures should be referred to as “Supplementary Figure 1” or “Fig. S1” so that it is clear to the reader that you are not referring to figures in the main text.