Schannwell et al. (2018, TCD) 'Dynamic response of Antarctic Peninsula Ice Sheet to potential collapse of Larsen C and George VI ice shelves.' MS No: tc-2018-21

Response to final reviews (08-06-2018)

We thank the reviewers for their additional comments which we have addressed as follows. Reviewer comments are in black regular font text. Our responses are in *black italic font text*, and revisions to the manuscript are shown in *blue italic font text*.

Report #1 (Anonymous Referee #1)

Recommend manuscript should be accepted subject to minor revisions.

1 Summary statement

The revised manuscript by C. Schannwell and co-authors addresses most of the points raised by both reviewers. The new version is clear and the authors tried to present a more detailed analysis of the results and an improved discussion. There is one thing that remains unclear and not really discussed: it concerns the similarity of the results between experiments 1 and 2 for the PSU3D model but not for the BISICLES model. The BISICLES results show a much larger grounding line retreat and ice loss in experiment 2 than experiment 1 for George VI ice shelf, while one would expect a larger response for experiment as the changes are more extreme (removal of all the ice shelf).

Note that the page and line numbers refer to the version of the manuscript including the highlighted changes.

2 Major comments

There is no real discussion on the differences between experiments 1 and 2. For Larsen, results are similar for the two experiments and for both PSU3D and BISICLES models. For George VI, the results are similar between experiments 1 and 2 for PSU3D but significantly different for BISICLES. The thinning rates and grounding retreat (Fig. 6 and 8) as well as the ice loss (Fig.4 and Fig.7) show a much larger response in for experiment 2 than for experiment 1, which is unlike one would expect as experiment 1 represents a much more radical change (removal of all the ice shelf). There is not much explanation about this, and the authors should at least discuss why the behavior of PSU3D is similar why the behavior of BISICLES is different for the two experiments, and also explain why the changes are larger for experiment 2 than experiment 1 in BISCICLES.
We think this is a misunderstanding. Experiment 1 does not necessarily represent a larger perturbation than Experiment 2. Owing to the crevasse calving law and their different implementations, Experiment 2 has much more potential to vary. In our simulations, either very little ice can calve (RCP4.5 scenario) or given enough surface water the entire shelf can collapse and new floating areas (that were formerly grounded) keep on calving. So unlike Experiment 1 where collapse is only enforced once, we can have repeated/continuing collapse of the shelf in Experiment 2. To make this clearer in the manuscript, we added: The discrepancy in sea-level rise projections between Experiment 1 and Experiment 2 is a result of the different applied perturbations. In Experiment 1, the entire ice shelf is removed at the start of the simulation before a fixed calving front is employed. In contrast, Experiment 2 with the crevasse calving law has much more potential to vary. Our simulations show that either very little ice can calve (RCP4.5 scenario) or given enough surface water the entire shelf can collapse and emerging new floating areas that

were formerly grounded keep on calving. So unlike Experiment 1 where collapse is only enforced once, repeated/continuing collapse of the shelf can occur in Experiment 2 (RCP8.5 BISICLES simulation).

The reasons for the agreement between PSU3D and BISICLES for Larsen C and disagreement for George VI are outlined on page 16 in the paragraph starting at line 15 (highlighted MS) where it reads for Larsen C: We attribute the good agreement across both models for Larsen C to the fact that the area of the marine-based sectors is limited in this domain (2.1 mm contained in marine-based sectors) due to the very mountainous bedrock topography constraining potential grounding-line retreat. This is supported by all simulations across all ice-sheet models as even under a wide range of different forcings the Larsen C embayment does not contribute more than 4.2 mm by 2300. And for the differences in the George VI it reads: *As this large grounding-line retreat is only initiated in the BISICLES simulation, large differences in sea-level rise projections occur. The most likely explanation for this differing behaviour is due to the difference in the inferred basal traction coefficient fields that affects each model's response to ice-shelf removal. PSU3D predicts much higher-friction bedrock conditions in the George VI embayment than BISICLES (Figure 2). These high friction bedrock conditions result in little acceleration of the major outlet glaciers following ice-shelf breakup in Experiment 2. This in turn means that the calving law applied to only floating ice cells cannot drive the initial retreat into the marine based sectors as the outlet glaciers do not thin sufficiently to form floating ice tongues. In contrast in the RCP8.5 BISICLES simulation for George VI, speed-up in response to ice-shelf breakup leads to enhanced dynamic thinning of the main outlet glaciers. This thinning in conjunction with the calving law drives the calving front into the marine-based sectors where further retreat is initiated by a combination of the marine ice-sheet instability and the meltwater driven calving law, resulting in the simulated much higher sea-level rise projections.*

Another relatively minor point relates to the tables that were in the first version of this manuscript and disappeared into a supplementary material. As mentioned by both reviewers, the tables were not clear and needed some clarification, which does not mean to hide them. It would be best to put them in the Appendix with the supplemental figures, and make sure they are easy to understand.

We have clarified the tables as suggested by both reviewers and had moved the table to the supplementary as suggested by the other reviewer because it disrupted the reading flow. We have now moved the table to the Appendix of the main manuscript and further clarified the table caption.

3 Specific comments

p.1 l.4: "in future" → "in the future"

Text changed to ..in the future.

p.1 l.11: "relative importance to sea level of the large ..." → " relative importance of the large ... of collapse on sea level rise."

We have rewritten this sentence according to the reviewer's suggestion but find that it no longer makes sense nor provides grammatically correct English. As such, we prefer to keep the sentence as before. The key proposition is the first part of the sentence, that we measure the 'varying and relative importance to sea level' of potential collapses.

p.5 l.2: "A is the depth averaged rheological exponent" → "A is the depth averaged rheological coefficient”

Changed to read .. depth-averaged rheological coefficient.

p.5 l.12: “the thickness” → “the ice thickness”

Changed to read .. ice thickness.

p.7 l.1: I still think this is confusing. You want the model to represent “reality”, or the version we know of reality provided by observations. So the models “should” only be as close as possible to that. I understand the argument of being close to equilibrium to simplify the comparison between models and assess the impact of the ice shelf collapse, but the first sentence is really misleading and not accurate, and should be at least rephrased.

We have changed this sentence to read: Following initialisation, the sheet-shelf models should aim to be as close to the steady-state initial conditions provided by observations, as long as the ice sheet itself is in steady state, such that dh/dt = 0.

p.10 Tab.1: Add the default sliding on BISICLES Experiment 1 (third line of the table). Also add “zero melt” or something along this line for Experiment 2 (lines 8 and 9).

Default sliding added to Table 1 line 3, zero melt added to lines 8 and 9.

p.12 Figure 4 caption: How is the uncertainty in BAS-APISM computed?
We added: Uncertainties are quantified by a Monte-Carlo simulation (see Schannwell et al. 2016).

p.12 Figure 4 caption: “Upper panels show” and “Lower panels show”

Text amended to read show, in both cases.

p.16 l.23: “scenario” → “reason/explanation”

Text amended to read explanation

p.16 l.25: “sticky” → “high friction”

Text changed to higher-friction and high friction here and nearby.

p.18 l.14-20: This paragraph is not really convincing, the arguments should be more detailed.

This paragraph is rewritten to now read: For Experiment 2, total maximum grounding-line retreat rates are similar to those from Experiment 1 when simulated by PSU3D. In the BISICLES simulation retreat rates increase from 6.4 km in Experiment 1 to 21.3 km in Experiment 2. This is in agreement with computed sea-level rise projections (Figure 7). Significant speed-up is absent in the years following ice-shelf removal across all basins due to the more gradual loss of buttressing in Experiment 2 (compared to the complete ice-shelf removal in Experiment 1). This results in a less dramatic dynamic response than in Experiment 1, with the exception of several basins of George VI Ice Shelf where retreat rates can lead to large mass losses. The gradual loss of buttressing simulated by Experiment 2 leads to grounding-line retreat and mass loss response occurring >15 years after ice-shelf removal.

p.29 Figure A4 caption: “distribtution” → “distribution”

Text changed to read distribution.

p.31 Figure A6 caption: should be “Upper panels show” and “Lower panels show”

Text amended to read show in both instances.

Report #2 (Anonymous Referee #2)

Recommend manuscript should be accepted as is.

We look forward to hearing back positively.

Regards,

Clemens Schannwell and Nick Barrand, on behalf of the author team.