Review of: ISMIP6-based projections of ocean-forced Antarctic Ice Sheet evolution using the Community Ice Sheet Model

Thomas Zwinger

December 7, 2020

1 General Impression

The second revision of the manuscript now includes a wider sensitivity study of the ISMIP6 CISM setup, including also different approximations to the governing equations. In my view, the main points of my criticism from the first round have been fully addressed. The authors have taken great efforts to accomplish a wide range of simulations with variations in parameters, approximations to the Stokes equation and physical models for calving and sliding. Representation of data in graphics has significantly improved - I in particular like the choice of colour scales. I see one issue about the conclusions concerning the Amundsen sea sector stability open to be elaborated, but else recommend the publication of this manuscript.

2 Main point

I kind of pick up my first point from the last review, with a particular focus on a **possible underestimation of the contribution of SLR from Amundsen sea sector**. In your response letter you state: *Removal of the Thwaites shelf does not lead to much additional retreat in the Amundsen Sea basin, suggesting that the current buttressing effects are small.* I presume that this statement applies solely to the previously applied power-law. In the main text you added a statement: *For example,if the topographic dataset is missing pinning points near the grounding line,the ice will be biased thin, and* C_p *can be driven to high values to make up for the lack of buttressing.* I subscribe to this statement, but I think one should also include the relatively coarse interpolation of the bedrock that can easily miss out on potential pinning points. In view of this, I am not surprised that in your model a vanishing shelf in front of Thwaites is of less severe consequence as – in my opinion – it would be in real world. Apparently, when using the Coulomb law, which is less able to compensate for missing pinning points and in my view a physically more justifiable choice at the grounding line in particular with coarse resolution ([Gladstone et al. 2017]), you seem to get the tendency for an ice-sheet collapse in this region. It is my opinion that the latter version hence is the more realistic scenario and that scenarios showing stability of the Amundsen sea sector are merely a consequence of a combination of under-resolution and the choice of in my view not optimal power law as the standard for sliding. I see that you reported all these findings at different places in the text, but it is somewhat spread all over the manuscript. I would like to see some statement in the conclusions (as this the highest impact part of the paper) on the in my view not enough highlighted importance of the choice of the correct sliding law, which I think is key to get realistic SLR estimations. Else, I would like to get some clear arguments (beyond numerical stability) why the power law in your view is the right standard choice.

3 Suggested corrections, additions and typos

Please, find suggestions for corrections, changes and typos spotted in the order of their occurrence in the text. Line numbers refer to the change-tracked version of the manuscript that was attached to the reply letter:

- Page 9, line 10: *. . . C_p is initialized to 20,000 Pa $m^{-1/3}$ y^{-1}* . What motivated the significant reduction of the initial value of 50,000 Pa $\text{m}^{-1/3}$ y^{-1} in the previous version?
- Page 10, line 29: *. . . is adjusted in each of <u>the</u> 16 sectors to match the observed . . .*
- Page 11, line 1: *We compensate by increasing the target thickness for this sector by the equivalent of 1000 Gt of ice, ...* Can you please elaborate how exactly this was implemented? How do you distribute this amount of ice underneath the shelf?
- Page 26, Fig. 7: Can you improve the explanation what comes to the black line that sometimes goes through the middle of the ice shelf (upper left panel; non-local MeanAnt) and is missing on the calving front at all shelves in (lower left panel; non-local PIGL). From reading further in the text I understand that all the shelf that is not confined within black lines is gone in 2500. For convience for the readers, I suggest you put this piece of information into the caption of the figure. Same applies Fig. 8 on next page.
- Page 22, Fig. 12: Can you help me (and also the readers) interpreting the closed black loop inside the Ross ice-shelf in the lowest left panel (nonlocal MeanAnt CESM2)?
- Page 23, Fig. 22: *CISM gives accurate results in steady-state and perturbation experiments at resolutions of 2 to 4 km.* Accurate with respect to what?

- Page 23, Fig. 27: *...resulting in slightly more drift during the projection runs.* Is it possible to elaborate on the trend (lower or higher sea level) of this drift?

- page 24, line 2: *...for which the SLR contributions at 2, 4, and 8 km are 1047 mm, 1300 mm,and 1473 mm, respectively.* In this line you report the lowest SLR (= lowest ice loss) for the 2 km grid but in the beginning of the next paragraph – which seems also to be confirmed by the graphs in Fig. 13 – claim that the finest 2 km grid has the largest ice loss (quote): *Thus, refining the grid to 2 km leads to greater ice loss.* For me this is contradicting. Please let me know if I am missing something.

- page 35, Fig. 24: Subjectively, I cannot read a lot from this graph because of a with respect to the lines over-dominating texture colour scale. This single new figure is in contrast to the in my view good choice of colour scales in the other figures. Additionally, if in any way possible with reasonable effort, it would be good to get the years annotated to the different lines.

References

[Gladstone et al. 2017] Gladstone, R. M., Warner, R. C., Galton-Fenzi, B. K., Gagliardini, O., Zwinger, T., and Greve, R.: Marine ice sheet model performance depends on basal sliding physics and sub-shelf melting, The Cryosphere, 11, 319–329, https://doi.org/10.5194/tc-11-319-2017, 2017.