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Interactive comment on "Retreat of Thwaites Glacier, West Antarctica, over the next 100 years using various ice flow models, ice shelf melt scenarios and basal friction laws" *by* Hongju Yu et al.

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I am happy if the authors are aware of my name.

General comments

This study from Yu and his colleagues aims at simulating the future of Thwaites Glacier in West Antarctica, over the next century. They use the ISSM ice-sheet model in its full-Stokes (FS), Shallow Shelf Approximation (SSA) and Higher Order (HO) versions, applying two kinds of basal friction laws (either based on effective pressure, using the Budd law, or not, using the linear Weertman law), two different grounding line parameterizations and various sub-shelf melting depth-dependent functions. This represents 12 familys of simulations, each of which forced by 8 different melt parameterizations.

Almost all the simulations show a similar retreating pattern, which I think is consistent, that the soon future Thwaites Glacier will be much thinner and that its grounding line will be much farther inland, especially its Eastern part. The Thwaites Glacier has been the focus of quite a lot of attention during the last couple of years, but I think this study adds novelty in this field of research. The results are in line with past studies, such as Joughin et al. (2014).

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The paper reads quite well, which is a pleasure, and is mostly well organised, which is even more a pleasure. A significant number of simulations was ran and I don't think it has been easy to organise the results this way.

I have two or three main concerns about the paper, which are not to be considered as major, but to which I would like the authors to respond. This consists other simulations and a point to add to the discussion.

- As you say, your 80_1000 melt scenario is representative of a cold year melt scenario, and was calibrated to match ice/ocean coupled simulations from Seroussi et al. (2017). What I am concerned about here is the fact, which was also a conclusion from the Seroussi et al. (2017) paper, that this type of sub-shelf parameterization leads to higher ice mass loss, compared to the coupled model. Thus, I would recommend to run another set of simulations in which the melt would be halved (for instance, could be a 40_1000 scenario), or at least significantly decreased so your study would consider the fact that the ice-sheet response to this type of parameterization is overestimated.
- My second concern is the proximity of the Pine Island Glacier (PIG) nearby. In all the simulations, the West part that is retreating is touching the PIG drainage basin, and I wonder the implications related to the change in boundary conditions. The Brondex et al. (2018) paper now in TCD seems to show a prior retreat from a nearby PIG tributary, of which the floating part eventually links to the floating part of TG. I would like this point to be included somewhere in the discussion.
- Finally, a number of grounding line discretizations have been explored by your team (Seroussi et al., 2014; Seroussi and Morlighem, 2018). If I understood well you used the so-called NMP in the Seroussi and Morlighem (2018) paper, in

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which you don't apply melt to partially floating elements and the so-called SEM1 discretization in which you also apply melt to the element in which lies the grounding line, but in proportion to the floating area of this element. I would be in favor of running another set of simulations considering the SEM2 grounding line discretization (or the SEM1 if I was wrong and misunderstood the fact that you used the SEM2...), since I don't think one can discard one parameterization or another on the basis of ideal simulations only Seroussi and Morlighem (2018). I don't think this is a big deal for you to do so.

The rest of my review is a series of specific comments.

Specific comments

Page 2, l25 to l28: Here, I understand that the ice mass loss is more sensitive to the use of different friction laws, or melt treatment close to the grounding line, but only when the stress balance is approximated (HO or SSA) but not when full-Stokes is used? I don't think this is what you wanted to say, since you have an impact of friction laws onto full Stokes modelling as well. Could you rephrase or explain.

Page 2, l33: In regards to the simplicity of your melt parameterization, the use of the word "realistic" is far from being fair. Could you rephrase.

Page 3, Fig.1: For consistency and clarity of the figure, in a) could you add the other grounding lines. For b) could you do the same and also add the front of all the glaciers.

Page 3, l9: Could you add those sensitivity tests as a Supplementary figure.

Page 4, l6 to l11: Here, you should refer to Seroussi et al. (2014) and Seroussi and Morlighem (2018) and mention the discretizations name that you used as defined in those two papers. This would clarify if you used SEM1 or SEM2 grounding line discretization, which is not completely clear to me.

Page 4, l19: For clarity, could you define the effective pressure.

Page 5, l5: Could you refer to my first main comment above.

Page 6, l14: I wouldn't only blame the datasets for this change in velocities after inversion. I would say that the model is not perfect as well, and that the model parameters can induce part of those initial changes Gillet-Chaulet et al. (2012). Could you rephrase.

Page 6, l30: Here, I would like a little explanation about why is the ice stiffer at the grounding line, or softer much higher up inland (different stress regimes, this is discussed in Ma et al. (2010).

Page 6, Fig. 3: Could you draw the grounding line position in those maps.

Page 10, Fig. 5: For clarity reasons, I would be in favor of using different maximums for the vertical axis so one could distinguish the differences within each type of friction law and implementations of the ice-shelf melt (like VAFmax=40 for the two Budds and VAFmax=25 for the other Budds)

Page 11, l31: Could you add those results in a supplementary figure.

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Page 12, l10: There is a law that you didn't discussed, the so-called Schoof law that is used in Brondex et al. (2018) and Brondex et al. (2017). I would strongly recommend it to appear in the paper as it has strong physical basis.

Page 12, l18: I would like to see those ridges you talk about shown in the figures (for instance Fig. 1)

Page 13, l16 to l21: your sub-shelf melting is a major limitation of your study, not just one limitation. For instance, the difference in grounding line position between the coupled model and parameterized simulations in the study from Seroussi et al. (2017) is significant. Could you discuss and insist a bit more on that point please. Also, I would recommend to add another set of simulations with even less melt in order to compensate for the overestimation of mass loss related to this type of parameterizations (see my comment at the top).

References

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