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Interactive comment on "Dynamic response of Antarctic Peninsula Ice Sheet to collapse of Larsen C and George VI ice shelves" *by* Clemens Schannwell et al.

Anonymous Referee #1

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1 Summary statement

This manuscript presents simulations from different ice sheet models showing the impact of a potential collapse of Larsen C and George VI ice shelves on the tributary glaciers feeding them. They investigate the case of a sudden and gradual collapse, and assess the impact of different model parameters (grid resolution, sliding law, ...) on the results. They show that changes in the Larsen C ice shelf have limited impact on its tributary glaciers, as this ice shelf provides a limited amount a buttressing. A collapse of George VI ice shelf on the other hand would have a much larger impact, as it provides more buttressing to its tributary glaciers and these glaciers are resting

on bedrock with retrograde slopes inland, making them prone to the marine ice sheet instability.

The results presented in this manuscript are novel and interesting, showing the very different response of glaciers in two basins, in terms of grounding line retreat and contribution to sea level change. It is great to see that this study is based on three different models, however one of them is exactly state of the art, and present results largely different to the other two, so it would be great to discuss this point and conclude on the possibility (or not) to use such simple models to investigate dynamic changes of Antarctic glaciers. Furthermore, there is not much discussion in this manuscript, just a description of the results, so it would be good to see a more substantial discussion added, including the impact of the different choices make in the model such as the sliding law used, the model resolution, and the agreement between models or between scenarios. The paper is well written and clear, except for the two tables and their captions, which are quite confusing. Below are some more detailed comments.

2 Major comments

I think it would be great to add "potential" in the title ("... to a potential collapse ..."), to highlight that this is just a possibility, or a future event. I think this is important given the recent collapse of the Larsen C iceberg, as it might confuse some people to talk about the collapse of Larsen C.

I found it confusing that the experiments are described one after the other as the text goes (new friction laws, different resolutions, ...). It would help to list all the experiments done in section 2 (maybe in 2.5 mention the additional experiments), or add a table with the list of experiments, so that the reader knows ahead of time what to expect.

In section 2.4, it is stated that the models should start with an initial state as close as possible from a steady-state. I disagree with this statement; the goal of the initialization is to get as close as possible to the conditions at a given time, including the thinning rate observed at this time. Removing this thinning/thickening rate can lead to an underestimation/overestimation of the changes simulated, especially as this kind of signal would probably take decades to fade out. Also, how large is the flux correction applied to the models and how does it impact the simulations and the conclusions of this paper.

p.10 l.2: I have a different interpretation of the Pattyn et al. (2013) paper. If steady-state grounding line positions are well captured with an internal flux condition, the paper states that "the short-time transient behavior is then incorrect" (abstract of Pattyn et al. (2013)). So such models might be less dependent to grid resolution but it does not mean that they are accurate.

Fig.7 shows that for some basins and variables, there is a good agreement between the PSU3D and BISICLES models for the different scenarios, while in other cases, there is a bigger difference between the two models that between the different scenarios. This should be better discussed, especially to highlight the reasons of these differences as well as the different cases. Section 3 describes these results, but there should be some discussion summarizing these findings.

Overall, there is no real discussion, just a description of the results. A proper discussion should include the current limitations of the models and future possible improvements, the impact of the different models compared to other parameters, such as the sliding law employed, the scenario chosen, or the bedrock used, with references to previous studies.

3 Specific comments

p.1 l.1: "past several": be more precise

p.1 l.13: "northerly limit": it would be great to explain this limit in a few words

Fig.1: "meters above sea level" is a bit confusing as all elevations are negative, maybe simply saying "in meters" would be enough. Also mention that the colorbar is truncated at 0, and maybe add the highest elevation in this area. The black polygons are not clear and can be confused with the grounding line position, consider using a different color or thick lines.

p.2 l.7: mention that happens on downward sloping bedrock elevation inland (not just on all marine based sectors)

p.2 l.9: remove "state-of-the-art" as I am not sure that the BAS-APISM model can be considered to be a state-of-the-art model ("simulates ice flow by solving the simplest permissible force basal approximation" p.3 l.7)

p.2 l.10: same as the title: add that you are talking about a potential collapse

p.3 l.10: "SIA is not valid at the grounding line", the problem here is rather that SIA is not valid on floating ice shelves and fast flowing ice streams.

p.3 l.16: "in assumed" → "is assumed"

p.3 l.34: Add sentences in the three model descriptions about the grid resolution (and grid resolution at the grounding line) employed in these three models.

p.4 Eq.1: What basal conditions (friction) is used for the BAS-APISM model?

p.5 l.20: What is R exactly and how does it relate to the temperature in a few words?

p.5 l.24-25: How is this done (in a sentence or two)? Some technical explanations are missing.

p.6 l.18: ALBMAP is quite old, why not use the new BEDMAP2 or Huss and Farinotti, (2014) data for all the models?

p.6 l.23: As mentioned above, do you really want the simulations to start from a steady-state? Or from the current thinning/thickening rate? Why not correct this by adding the rate of thickness change instead of assuming that it is 0? And by the way, I don't agree that "After initialization, the sheet-shelf models should be in equilibrium". The models should represent the actual ice sheet state at the time captured by the initialization, so if the ice sheets where thinning, the initialization should capture and reproduce this initial thinning.

p.6 l.28: Adding this flux correction is fine, but you should show how large it is, and how large it is compared to the actual surface mass balance. Also, how different are the results if you don't include it? What are the impacts on the simulations?

p.7 Fig.2: Why not show the BAS-APISM model here? I have a hard time understanding what the basal boundary condition of this model is. Also should be "Black lines denote ..."

p.8 l.3: What resolutions are used? The list of experiments with their characteristics should be better detailed in the text.

p.9 l.12: Over what period does this change happens?

p.9 l.14-19: The initial conditions (ice velocity, thickness, elevation, rigidity, ...) also have an impact on the evolution of the glacier, as well as the numerical parameters (grid resolution, ...).

p.9 l.14-19: What about the BAS-APISM model?

p.10 Fig.4: Should be: "Upper panels (a,b) show ...", same for "Lower panels ..."

p.10 l.2: As mentioned above, the Pattyn et al. (2013) paper says that "the short-time transient behavior is then incorrect" for grounding line evolution captured with internal flux conditions.

p.11 Fig.5: Should be: "Black lines denote ...". Same for caption in Fig.6.

p.15 l.3: "A consequence of this is ..." → "A consequence is ..."

p.15 l.23: "most of grounding-line retreat" → "most of the grounding-line retreat"

Tables 1 and 2: the tables and their captions are quite confusing. Especially as all numbers reflect different time periods, and some variables are not standard (e.g., dGt/dt for mass change rate). Also why not use the same order as Fig.6 (BAS-APISM left, ...).

Table 2: Fig.A7 shows larger grounding line retreat for many glaciers (GeoIII, GeoIV, ...) with the Coulomb friction law, which does not seemed to be reflected in this table. But as I just mentioned above, I am quite confused by this table. I would also expect this increased grounding line retreat to transfer in more mass change for the Coulomb case. It would be simpler to have both BISICLES cases next to each other.

p.18: As mentioned previously, there is not much discussion, just a description of the results.

p.19 l.6: "vulnerability of ice-shelf ..." → "vulnerability to ice-shelf .."

Fig.A1: Y-axis label should be "Temp. bias" not "Temp.". Caption should detail bias which two quantities.

Fig.A2: Same as Fig.A1

Fig. A6: Caption should be "Upper panels (a,b) show ...". Same for "Lower panels ..."

Fig. A7: Simulations with Coulomb friction show a larger retreat, which is not captured in Table 2.

4 References

Huss, M., and D. Farinotti, A high-resolution bedrock map for the Antarctic Peninsula, Cryosphere, 8(4), doi:10.5194/tc-8-1261-2014, 2014.

Pattyn, F., et al., Grounding-line migration in plan-view marine ice-sheet models: results of the ice2sea MISMIP3d intercomparison, J. Glaciol., 59 (215), doi: 10.3189/2013JoG12J129, 2013.

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