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Interactive comment on "ISMIP6 projections of ocean-forced Antarctic Ice Sheet evolution using the Community Ice Sheet Model" *by* William H. Lipscomb et al.

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1 General Impression

This is a manuscript that complements the currently reviewed ISMIP 6 inter-comparison on the Antarctic ice sheet. Beyond the contribution that was already accounted for in the paper by Seroussi et al. (Seroussi et al., 2020), the authors show new ways of spinup and present computations beyond the time-frame of ISMIP 6.

The text describes in detail the methods used and the assumptions applied within the contribution from the ice-sheet model CISM to this inter-comparison. From my

point of view, the paper to a large extent analyses the impacts of assumptions and approximations on the results, except for two open points which I will raise in the section below. The article is well written and has a clear line to follow. I got the impression of a few inaccuracies in the notation, which in my view is easily to be fixed. I also have some suggestions on how to improve or augment the figures.

Modelling techniques presented here are mainly variations of existing approaches, but the size and the context of the applications make for the scientific novelty of this paper. I also see the value in documenting the techniques leading to these results, which of course cannot be presented in such detail in an inter-comparison paper. In that sense, I see it as a useful contribution to the literature, also to demonstrate what efforts have to be taken in order to realise such an implementation.

I mainly placed a few technical suggestions to (according to my subjective impression) improve the readability of the text, in particular for readers not being that familiar with ISMIP 6. Besides this, the only more substantial criticism I would place is the in my view not complete analysis of the impact on the results of some of the assumptions made, in particular the over-simplification of calving and the impact of the (actually not clearly revealed) approximation to the Stokes equation and the resolution applied in the ice-sheet model. If these points are addressed, I would recommend this manuscript for publication in TC.

2 Main points

As mentioned above, I would see it necessary to have two in my view not completely clear issues to be elaborated:

1. The influence of the in my view oversimplified calving law on the results, in particular its impact on grounding line migration and retreat

2. The choice of the approximation to the Stokes equation and its consequences on the accuracy and sensitivity of the results

Concerning the first point, i.e. calving, you explain (page 4, line 20): *Instead, we use a no-advance calving mask, removing all ice that flows beyond the observed calving front. The calving front can retreat where there is more surface and basal melting than advective inflow, but more often the calving front remains in place.* Here you might mention the point in time of the observation of the prescribed front (I presume it somehow refers to your initial state in 1995). In the Conclusions, you mention it in just one sentence: *In terms of ice sheet physics, these simulations do not include hydrofracture or calving-front retreat, and thus are missing positive feedbacks associated with reduced buttressing of grounded ice by ice shelves (Sun et al., in review).* In my opinion, the reader would benefit from some clarification on how much impact neglecting the possibility of ice-shelf collapse (as it would be provided by ISMIP 6 forcing) on the resulting sea level has. In my view, the current approach significantly might lead to an underestimation of sea-level rise, in particular at times beyond the year 2050. One additional experiment could be to, for instance, just remove the shelf in front of Thwaites and see what short-time impact this has for this region – maybe in combination with the upper end combination of AOGM and ocean forcing. Or to apply the shelf-collapse scenario from ISMIP 6. If this is for some technical reason not possible at this stage, please mention that in the paper.

On the second point, model approximation, you present (Page 3, line 30) that the CISM code includes options for the following approximations to the Stokes equations: *SSA, L1L2, LMLa* – mainly following Hindmarsh' nomenclature (Hindmarsh, 2004). On page 4, line 1, you write: *The simulations for this paper use the depth-integrated solver, which gives a good balance between accuracy and efficiency for continental-scale simulations (Lipscomb et al., 2019)*. I consider SSA as well as L1L2 as depth integrated (the latter of course with some corrections from a vertical profile). Could you please clearly identify which of the previously listed models you deploy in your study? I am

further confused how a depth integrated model can use 5 vertical layers (as described in the paragraph before), but assume that you use this for some aspects different from depth-integrated parts, such as temperature evolution or velocity corrections. Is this vertical resolution sufficient to resolve the physics? Can you include some lines on how much you estimate the influence of the choice of the approximation and the chosen resolutions on the results of your study. For instance, would one expect to reach the same conclusions if using a LMLa model or perhaps the current model with a finer resolution?

3 Suggested corrections, additions and typos

Please, find suggestions for corrections, changes and typos in the order of their occurrence in the text:

- Page 1, Author list: Typo: Nicolos → Nicolas
- Page 2, line 28: *To study long-term ice sheet evolution, we extend the simulations to 2500, ...* Minor issue, but for better readability, please mention that this is a date (it could be iterations or any other time unit).
- Page 4, line 6, 9 and 11: Again, minor issue, but in order to stay consistent with the nomenclature in equation (1), I would use the subscript for bedrock in all the following occurrences of the basal velocity, i.e., $u \rightarrow u_b$.
- Page 4, line 13: Can you elaborate on the choice of exactly this value for C_c? Is this representing some tested optimum? Or (see next question) does it not really matter what to put there?
- Page 4, line 17: *Setting N to overburden pressure implies power-law behavior in nearly all of the ice sheet.* To my understanding, this means that power-law

sliding then most likely prevails down to the grounding line - or am I missing something? If this is the case, I would question the benefit of explaining sliding law (1) and the Coulomb sliding parameter therein in such a detail. Being aware that in lack of a detailed representation of pinning points, any sliding law that reaches zero resistance at the grounding line inherently causes issues (confirmed also by our experience with Full-Stokes models at similar resolution), I do not criticise the approach as such, but would welcome to include some information on how high the friction parameters at the grounding line actually are (or have to be in order to be stable) and how – also by keeping them constant relative to a moving grounding line – that impacts the prognostic runs.

- Page 5, line 5: *(The simulations described here use only the ocean forcing.)* I do not understand the meaning of this sentence. Does it mean, you do not apply any atmospheric, but only ocean forcing? Or does this only apply in connection to CMIP? Please, elaborate. On a side-note, in my personal opinion, I find it strange to put whole separate sentences in brackets. I would write it without. I mention this, as it occurs a few times in the text.
- Page 8, line 4: *Eq. 6 is based on the equation for a critically damped harmonic oscillator, where the first term in brackets nudges H toward Hobs, and the second term damps the nudging to prevent overshoots. (The damping is not exactly critical, however, because dC_p/dt is not exactly proportional to d^2H/dt^2.)* I am slightly confused by this statement, since from my point of view, if dC_p/dt would be somehow proportional to d^2H/dt^2, the C_p on the right-hand-side would introduce some functional relation to H and I would not immediately see the equation of a damped harmonic oscillator recovered by (6). Could you clarify or insert a reference where this is explained in detail in order to help the reader to follow that up. Minor issue: *Eq. 6* → Eq. (6).

- Page 8, line 6: *We hold C_p within a range between 10^2 and 10^5 Pa m$^{-1/3}$ y^{-1}, since smaller values can lead to numerical instability, and larger values do not significantly lower the sliding speed.* Can you explain the nature of the instabilities? I wonder if this is somehow linked to the fact that you had to fix the effective to the overburden pressure in the sliding law.
- Page 9, line 7 and Eq. (8): Based on results shown in Fig. 5, I conclude that the symbol δT stands for the nudged values of the by the ISMIP 6 protocol given δT_{sector}. Simply, the missing subscript might also lead to the conclusion that it is the difference to this reference value, i.e., the correction to the correction. I would consistently add the subscript *sector* to δT.
- Page 9, line 5: *After some experimentation, we set $m_T = 10$ m y^{-1} K^{-1} and $\tau_m = 10$ y.* Do these values link in some way to physics? If not, please explain the procedure behind the term *some experimentation*.
- Page 9, line 6: *. . . , with modest values of δT (≈ 1 K or less). Modest* with respect to what? I guess to melt-rates.
- Page 9, line 9: Typo: To me it appears that there is an orphan *A* between two sentences.
- Page 9, line 24: *Some biases can likely be attributed to errors in ocean thermal forcing (which is treated simply by the basin-scale melt parameterizations) and seafloor topography (e.g., an absence of pinning points, resulting in grounding-line retreat that can be compensated by spurious ocean cooling).* Could the errors by absence of pinning points also link to the relatively coarse resolution of the applied ice flow-model and the under-resolution of bedrock data in some regions? If so, please mention it.
- Page 9, line 30 and page 10, Fig. 2: *Thwaites Glacier is too thin and Pine Island Glacier is too thick;* For me, the contours in Fig. 2 make it virtually impossible to

spot thickness differences at the grounding lines or in smaller shelves. Perhaps, as you mention the Amundsen sea sector here in the text, some zoom-in Figure for that particular regions (just like Fig. 4 provides for speed) would be good. Neither is it possible to spot a clear outline of the Antarctic ice sheet. My suggestion to improve this, would be to have a neutral (e.g. white or blue) colour outside the ice-sheet/shelf area and only plot the grounding line. I, personally, have difficulties with the low contrast between the different shades of yellow and green in this figure. To me there seem to be jumps in ice thickness difference across many places at grounding lines (both with negative and positive signs). For instance, very prominently in the left panel for Amery. I would conclude – assuming that any evolved geometry is at least C^0 continuous across the grounding line – that also the difference has to be continuous. If you please could explain where this discrepancy arises from or else point me to my misconception of this figure.

- Page 11, Fig. 3: In this figure the colour scale actually works for me. I conclude that in Fig. 2 it is not about the colours themselves but the representation. Nevertheless, the black lines in Fig. 3 are not really visible, but in my view on that scale (different in Fig. 4) anyhow obsolete concerning the clear velocity jump between land-based ice and shelves.
- Page 11, Fig. 4: In large parts over Thwaites, the black grounding lines are really hard to spot over the dark green texture – perhaps a different colour (some grey?) would help. I mention that, as this is important since you refer to discrepancies of grounding line positions in this region in the text.
- Page 12, Fig 5: On my screen the underlying Antarctic ice-sheet shape is extremely difficult to spot. Please, enhance this. To me it comes clear that you are representing the nudged total values of δT_{sector} (see earlier comment on Eq. (8)). Would it add value to the graph to include the calibrated values ISMIP 6 values for comparison?

- Page 14, Fig. 6: Since there are finite values of the ocean thermal forcing underneath land-ice, can you please explain their meanings?
- Page 16, Fig. 8: Please, within the caption of this figure, explain the red lines in the pictures, in particular the enclosed one within the Ross ice-shelf for *HadGem2/nonlocal MeanAnt* scenario. I guess they mark shelf areas, and I also guess they represent data for least/most sensitive melt scenario with the same red colour. If so, please distinguish them with lines of different colours or different pattern. Also, please, indicate the meaning of the black line – I guess it is the initial grounding line position.
- Page 17, line 3: Typo: orphan dot
- Page 18, line 1: *Ice sheet retreat in these simulations is modest for the next several decades.* In my view, this statement links to the calving simplification mentioned in the main point. You might address the following question: Could the initial slow response be linked to the fact that you are not able to fully capture effects of potential ice-shelf collapse? To some extent you mention this a few lines below (Page 18, line 17):*In terms of ice sheet physics, these simulations do not include hydrofracture or calving-front retreat, and thus are missing positive feedbacks associated with reduced buttressing of grounded ice by ice shelves (Sun et al., in review).* For me the connection with the timings of SLR appears to be important and is missing.
- Page 18, line 21: *At 4-km grid resolution, processes such as grounding-line retreat may be under-resolved.* I think it is fair to say that at 4 km resolution grounding line mechanics <u>is</u> underresolved, in particular if combined with the choice of sliding law in this study ().
- Page 18, line 25: *These uncertainties suggest several lines of research to further improve ice-sheet and sea-level projections:* What I am missing in this list here

is the aspect of changing the accuracy of the approximation applied in the ISM - in particular as I understood that CISM would have the possibility to solve LMLa (Blatter-Pattyn) equations. Would results improve if deploying higher order ISM approximations? Are they feasible given the time-intensive spinup process?

References

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