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Interactive comment on "Sensitivity of the Weddell Sea sector ice streams to sub-shelf melting and surface accumulation" *by* A. P. Wright et al.

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Summary

A well-motivated introductory section includes an introduction to the study area (the portion of the West Antarctic Ice Sheet draining into and including the Filchner-Ronne ice shelf) and arguments for why model-based explorations into the future evolution of this region are needed (outlet glaciers and ice streams feeding the FR ice shelf reside on retrograde bed slopes, suggesting their susceptibility to the marine-ice sheet instability; recent studies suggest that ocean warming in this region may lead to increased rates of submarine shelf melting). A brief description of the model and experimental setup is then given, followed by discussion of the model initialization procedure. A series of forward model experiments is then described, including a baseline reference experiment. During the coarse of the experimental description and discussion section of the paper, the authors report on a wide range of dynamical behaviors to be expected from ice streams draining into the Filchner-Ronne ice shelf, following reasonable changes in submarine melting beneath the ice shelf and at ice stream grounding lines, and as a result of reasonable changes in surface accumulation. The authors conclude by reiterating the main findings of their work, which are namely that specific ice streams in the region are likely to be stable, unstable, or exhibit a threshold behavior with respect to increased submarine melting (or changes in accumulation rate). Importantly, the work clearly identifies the ice streams in this particular region of the WAIS that deserve close future monitoring with respect to concerns about grounding line retreat, dynamic mass loss, and sea level rise.

Overall, I found this paper to be well organized, clearly written, and enjoyable to read. The science addressed is clearly within the scope of TC and the application of an appropriate model for addressing the questions of dynamic mass loss and g.l. retreat in this region are (to my knowledge) novel. The authors reach substantial conclusions by identifying 1) the range of future dynamical behaviors to be expected and 2) the ice streams / outlet glaciers that require close further monitoring in this region. I expect that the results from this paper will have an impact on both future modeling and fieldwork-based studies in this region.

While I do not think the paper requires any major revisions, I have numerous suggestions for minor revisions that I think would help to make the paper more clear and easier to read. Below, I've done my best to divide these into more broad topics (minor concerns) vs. specific editorial suggestions.

In all cases below, I use "x,y:", with "x" referring to the page number and "y" referring to the line number(s) in question. For multiple items on the same page, "(a-b): . . ." refers to additional line numbers (a through b) being discussed.

Minor Concerns

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Interactive Comment

5477, 22-26: "... under warmer past climates" Is there clear evidence there were past retreats? Is there evidence that these past retreats coincided with warmer climates? If so, perhaps some refs. should be added to support these claims (or make it clear which of the refs. do support these claims). It sounds plausible, but could be supported better here.

5479, 21: Is it accurate to refer to BISICLES as a "3d" model? While the full 3d velocity field can be recovered (when accounting for an SIA-like vertical shearing component to the flow), in my understanding, it is most commonly applied in a 2d mode (SSA "star"), where the vertical shearing term only affects the depth-ave viscosity "seen" by the SSA solution. More importantly, is it being applied in a fully 3d mode here, or something closer to 2d? I see that the "SSA star" issue is discussed in a subsequent paragraph, so it is probably sufficient to simply link this discussion more clearly in the text. (25-26): suggest using "block structured adaptive mesh refinement" to describe the AMR "method". Also, it is not just the g.l. that dictates the refinement is it? Doesn't general dynamic complexity also come into consideration, e.g. regions of large strain rates get AMR as well?

5480, 15-17: Providing the values for C and m in the sliding law doesn't really help the reader if they can't see the form of the sliding law. Perhaps just show us the equation inline here? Also, if m is just set to 1, we probably don't even need to know about it.

5481,3-9: It isn't clear to me why the inversion procedure breaks down when the ice sheet surface topography data is too detailed (relative to the bed topography). The ice flow model, a constraint in the inversion procedure, includes membrane stresses and thus should be serving to smooth these out. While I don't think this can / needs to be addressed here, it seems like something worth further attention (e.g., could it be "fixed" through regularization?).

Section 2.4: (5482, 24-28) Clarify – is the 'relaxation' done to bring the init. cond. closer to being in equilib. with the SMB forcing? To reduce otherwise large and spurious values of the flux divergence field? Does "... two continuous fields " means "spatially continuous"? (5483,2-5) It took me multiple readings to start to understand what is being done here, but I still don't follow it all. I think I follow that the basal melt rate field required to maintain a SS in the ice shelf is diagnosed, and then divided into a g.l. and "ambient" component. This is then somehow tied to the location of the g.l., so that as the g.l. moves over time these fields move with it? I don't follow how this allows for a basal melt rate field that covers the entire shelf at all subsequent times (e.g., as the g.l. retreats, there would seem to be grid cells near the calving front that no longer have a melt/freeze rate associated with them). Maybe more importantly, it isn't clear to me why this basal mass balance (for the ice shelves) and the "compensatory" SMB on grounded ice are not adequate to maintain equilibrium for the reference experiment. It seems like they were designed for that purpose. At any rate, I think this section could be explained a little more clearly, even at the expense of adding additional text / figures. This initialization problem is of significant interest to a lot of other modeling groups right now, so even if it has not lead to ideal results here, a clear description of the problems and lessons learned in this study would be useful to other readers.

5483,19-22: This is a bit confusing as written. By "either" side of the g.l., I think you mean "both" sides (upstream and downstream)? And why 8 cells? When you refine at the g.l. you do so for more than just the cells that are on both sides of the g.l.? Does the 8 refer to before or after you divide by one half? If this is really important, perhaps a figure would help the description?

5485, Section 3.2: As above, not clear to me why the reference experiment fails to maintain a steady state. Wasn't it designed to do so?

5486,5-7: "The reference run ... viewed as a neutral staring point ...". It sounds like "neutral" might not be the right choice of words here. From the longer description of the reference state, given in 3.2, it doesn't sound like the reference run produces a steady state, which is what I think of when I read "neutral".

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5491, 15-18: Is it worth mentioning the apparent similarities here between these ice streams and the "classical" Siple Coast ice streams flowing into the Ross?

5492, 26: "indicate" -> "suggest"? "are CURRENTLY? situated very close to . . ."

Figures

The necessary detail from the figures was very difficult to discern in the print version of the manuscript. The online versions of the figures were better, but still required excessive zooming in to be able to see the necessary details discussed in the text and captions. For example, for Figs. 8 and 10, I had to zoom in to >300% to really see what was going on.

Fig. 2: Might be worthwhile reminding the reader which color (red or blue) is more sticky / slippery.

Fig. 4, caption: "Results of the 200 yr EXPERIMENT ON GROUNDLING LINE CONVERGENCE . . ."

Figs. 5, 6, 7, 8, 10: for right column of line plots, showing ice loss vs. time, suggest adding a right-hand side vertical axis, that gives the volume loss in equivalent of cumulative SLR.

Acknowledgements

While probably not strictly necessary, I know that the developers of the BISICLES model on the DOE side would greatly benefit from any acknowledgement of DOE's role in developing the code. Co-author Cornford probably has access to something appropriate from previous papers (e.g., a recent Nat. Clim. Change paper contained such an acknowledgement).

Editorial Suggestions

p.5476 (Abstract) Lines 5-8: suggest adding something like ". . . and the ability to accurately model marine ice sheet dynamics . . ." (under the description of "significant advancements”). Line 10: note that the “perturbations” are not arbitrary? Label them as “reasonable” or “realistic”?

p.5476 (Intro) Line 20: move Mercer ref to end of sentence? Line 23: It reads like “smooth” is needed for “retrograde” slopes, but I don’t think this is the case – you can still have retrograde without being smooth. Anyway, the definition of “smooth” seems arbitrary to me.

5477, 6: Add some observational &/or modeling refs. to section on loss of grounded ice following loss of buttressing (e.g. Scambos et al. folling Larsen B collapse?). (27): A few more refs. for the modeling of ice shelf buttressing, like Goldberg et al. (and other refs therein?)

5478, 2: Payne et al. ref ... add a few more, e.g. recent paper by Joughin et al. (GRL,37,2010), recent work by ISSM group at JPL? (5-6): “highest salinity . . .” Isn’t this usually referred to as “High-Salinity Shelf Water (HSSW)”? (14-15): “. . .increase by an order of magnitude” are there refs. to support this statement?

5479, 5: “constrained” -> “narrower”. (7): “enhanced ocean warming” -> “changes in ocean warming” (not all anticipated future changes are going to lead to ocean warming). (13): “predicted” -> “expected” or “anticipated”? I don’t think we’re good enough yet to call these predictions.

5480, 7: If “n” is not ever referred to again here, then omit it? That is, you can just say “power law exponent set to 3”. (7-8): How different is the Pattyn formulation for A(T) from something more standard like that given in Paterson? Give the most fundamental reference here if similar.

5481, 12: “up to 2000 yr” is used here and in other parts of the text. This is confusing. I assume it means 2000 yrs into the “future”, assuming that time 0 is supposed to be something like today?

5482, 10: “containing” -> “quantifying”. (19): Is the optimization converged after 16 iterations? (19-22): A bit of a run on. Break up into two sentences?

1584, 3: suggest "... =4km) to 5 (maximum size of 4 km, minimum size of 125 m)." (23-24): "grounding line shape" -> "grounding line position".

5487, 21: suggest "Very little melting occurs on the Antarctica ice sheets even at sea level ..."

5488, 3.5: suggest section title, "Grounding line melting versus ambient shelf melting"

5491,3: "steep walls" -> "steep rock walls"? (7): "...which appear to place them CURRENTLY near to a critical threshold ... and unstable RETREAT."

5492, 1: "inverse bed slope" -> "reverse bed slope"? (7-9): " Trough width has previously ... Even under conditions ... stabilizing effect during retreat." Shorten this? Could go with "While trough width has previously been show to XYZ ... (reference), it is unlikely to ... ABC ... here."

5493,2: "once retreat begins FOR THESE ICE STREAMS ...". (13-14): "...affect the responses of ice streams to EXTERNAL FORCING."

Interactive comment on The Cryosphere Discuss., 7, 5475, 2013.