

Interactive comment on “Exploration of Antarctic Ice Sheet 100-year contribution to sea level rise and associated model uncertainties using the ISSM framework” by Nicole-Jeanne Schlegel et al.

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Received and published: 9 July 2018

This paper presents a thorough ensemble exploration of the sensitivity of a state-of-the art Antarctic ice flow model (ISSM) to uncertainty in near future perturbations. To my mind, it addresses the shortcomings of most previous attempts by (1) considering the whole continent (2) doing so at (I think) adequate mesh resolution (3) including a well thought out and well described experimental design. It ought to be a benchmark against which the community measure future efforts for some time. Given that, I don't really have any substantial criticisms of this paper, just a few minor comments to make

Abstract, L11 : it seems odd to say that 'grounding line retreat... is *driven by errors* in

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bedrock retreat' . Rephrase?

Abstract L13 : 'endmember' -> 'the most extreme'? . endmember seems a little 'jargon' for the abstract.

p3, L16 '1km resolution at the coast' - where is the coast - the calving front? Or the grounding line?. On this note, is the region over which the GL moves generally covered by ~1km mesh spacing? Could we have a figure?

P4, L6 : L1L2 costs 10X SSA - this is a surprise (my own L1L2 is about 1.2X costlier than SSA), can you say why? There are two things in L1L2 that might slow you down (a) a more costly viscosity calculation (b) shorter timesteps due to the diffusion-like component of the velocity, but these a modest for me. Or is there something about your SSA that makes it very fast (e.g, how many Newton iterations are carried out per time-step) . Possibly this is described in the cited papers, but a quick summary here would be helpful.

P5, L11. Do you mean that the presented results are 'simulation minus control' or something else?

P5,L14 : You would see the vast majority of the change if the ice shelf thinned to 1m, even if the front remained the same. I think here you are adding the result of immediate collapse, rather than saying much about gradual future calving front retreat.

P7,L30 'plus an epsilon of 0.01%' → 'plus 0.01%' ? I think know what you mean by an epsilon, but it seems redundant, here (0.01% is a small change compared to -40%).

P9, L7. If the melt rates are too great in some places and too small elsewhere, why is it reasonable? Not saying it isn't, just that you should either say why, or not say it at all.

P10: First paragraphs of result. A bit of rewriting is needed here. The second paragraph in particular is describing methods, rather than (say) outlining the results/structure of the results section.

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P13: first paragraph. I think here you are saying that as the number of partitions grow, you are seeing some result other than just the number of neutral outcomes grow quickly. OK, your mean is decaying, not just the spread, but I can't see from what is written what (substantive) evidence you have for that position.

P13:L30 – looking at the figure, I thinking you are concluding that the bimodal shape comes from the melt rate UQ because only that parameter gives a bimodal distribution when considered in isolation. Should you say that outright in the text?

P17, L27: Durand, Gladstone, and others (including Seroussi, no?) have all indicated a resolution very much finer than 8km is required around the grounding line (I find ~1km, and sometimes better, is needed for Antarctica, even with SEP treatments). But I thought (from p3, L16) that you might be at least close to this?

P19, L10: Is control mode drift necessarily undesirable? The model parameters are optimized to given the present day velocity / geometry, so ought to agree with the present day observed thinning rate, which is not zero – there is committed change to think about. Given the general nonlinearity of, say Thwaites glacier retreat, I think is is better to have the correct drift, than none.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-105>, 2018.

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