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Interactive comment on "Century-scale simulations of the response of the West Antarctic Ice Sheet to a warming climate" *by* S. L. Cornford et al.

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We thank referee #1 for reviewing the manuscript and the valuable advice given. We respond to each point in turn below, with the referee comments in plain text *and the responses emphasised*

Using the BISICLES adaptive mesh ice sheet model, the authors produce simulations of the evolution of the West Antarctic ice sheet for the next centuries. Perturbations of surface accumulation and sub ice shelf melting are provided through anomalies given by atmosphere and ocean models driven by the E1 and A1B scenario. The authors show that dynamics response of fast flowing ice stream is mainly dominated by the

choice in the initial conditions and values of sub-ice shelf melt and point the importance of using a sub-kilometric mesh resolution to avoid underestimation of ice sheet contribution to sea-level rise (SLR). This is a very nice piece of work and the manuscript extensively describes the methods the authors used. However, to my opinion, given the important amount of details given, the complex notations, numerous figures of insufficient quality, and a rather descriptive discussion, it makes the manuscript hard to follow and this weakens the message the authors may want to offer to the community. I believe that in the current state, only ice dynamics specialists can read and understand the significant results behind that work, which is I think a pity as presented results may interest a broader community. After some restructuration, I also think that this paper would have the potential to be highlighted by the EGU. To my understanding/opinion the most important results are

(i) Contribution to SLR will remain largely dominated by the Amundsen Sea sector, as destabilization of other sectors seems unlikely with our current knowledge of coming ocean forcing. Large uncertainties remains, particularly due to the behavior of the Thwaites Glacier. (ii) In their experiments, the contribution to SLR induced by change in surface mass balance is the same order of magnitude when compared to dynamics but SMB change does significantly not impact ice dynamics with the considered time scale. (iii) Mesh resolution is an issue (this is well known) and some regions are more sen- sitive than other to the resolution (this is pretty intuitive, but to my knowledge this has never been discussed so far).

To my opinion, this manuscript would deserve substantial reorganization to better highlight the main results, and more emphasizes on point (i) (and then abstract should focus more on that point). I tried to formulate few suggestions below, which I hope will help the authors to improve their manuscript.

This is indeed a long paper, and it does make good sense to make it accessible to a wider audience, who will certainly be more interested in (i) and perhaps (ii), and less interested in (iii). We do think that (iii) is important, not only to ice-sheet dynamics

specialists, but more widely – a general reader needs to judge the accuracy of the study, and we think that a convergence study should be a routine part of this kind of paper, as it is both simpler and more widely applicable than, say, performance in idealized test problems – but we agree that the detail presented in the main text could be replaced with a simpler statement of import, with the detail available for those more interested in it. The abstract was modified accordingly.

- This is a pretty long manuscript. I would suggest summarizing the description of the methods to what is absolutely required to understand the results and discussion and move all the technical details into an annex. I agree that such a preference in the presentation is subjective but to my opinion this would greatly help non-specialist readers to follow the main results and any ice sheet modeler can refer to the annex to have details and reproduce the experiments if they whish. But in the current form a general reader cannot skip the methods and understand the results, and most probably would not understand the methods if they make the effort to read it.

We agree that non-specialist readers should be able to read the paper without becoming mired in technical detail. We have followed the referee's recommendation, and moved the detailed material on mesh resolution and initialization to appendix A and B respectively, and summarized them in the main text.

In the methods, the authors first describe the combine anomaly experiments, and then the melt rate ones. This makes sense as they want to crank up the perturba- tions and evaluate how far could go the SLR contribution. However, in their results- discussion section, order of presentation is the other way around. I guess they choose that option to first show how much their results are sensitive to mesh resolution (and this makes sense to do that experiment on the forcing presenting the largest retreat). I think that the discussion on mesh resolution is rather technical and should be moved to the methods in the annex. Then the authors could start their discussion with the com- bine anomaly experiments. I think this would help to clarify the flow of the discussion and would more focus the discussion on climatic results rather than mixing them with tech-

nical aspects (which are of importance but to my opinion should only be mentioned in the main text).

We moved the mesh resolution results to an appendix, and now present the combined anomaly results followed by the melt-rate anomaly results.

In general the quality of the figures could be improved. Few suggestions. Some Figure(s) could be moved the Annex (at least Fig. 2, probably Fig. 10 and 11). Figure 13 and 14 could be merged, or better follow the template with all the basins as in Figures 7, 8, 15 and 16. The choice of color scale in Figure 4 forbids discriminating easily the negative and positive larger values (-10 and +10 are pretty close). In Figure 7 color scale highlight the 2 different atmospheric models when the discussion is focusing E1 and A1B scenario, which are uneasy to discriminate. In Figures 7, 8, 15 and 16, using filled symbols may help to read the figures and follow the discussion.

Figs. 2,3,4,5,10,11 have been moved to the appendix.

Fig 13 and 14 were merged to become the new Fig 9 – which was a good idea – now it is easier to compare the Ronne-Filchner and Ross trends.

Figure 4 (now fig 14 and in the appendix) has a new colorscale (fading to dark blue rather than the purple shade) so hopefully the -10 and +10 values are easier to tell apart.

In Figure 7,8,15,16 (now 3,4,5,6) we changed the symbols to follow a more systematic scheme. All A1B results are marked with solid symbols and E1 results with wireframe symbols, while HadCm3 results are marked with squares and ECHAM5 results with triangles. We think this makes it easier to follow the A1B vs E1 discussion around fig 3, and also means that the reader does not need to refer to the legend so frequently. It remains difficult to pick the trends in the 21st century apart in figs 5 and 6, as they are so similar, so we have added a pair of figures to the supplement which show just the 21st century on a larger scale.

Interactive comment on The Cryosphere Discuss., 9, 1887, 2015.