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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 2 for reviewing the manuscript and the constructive commentary. We respond to each point below, with the referee comments in plain text *and the responses emphasised*

This paper studies the evolution of the West Antarctic Ice Sheet (WAIS) using the ice sheet model, BISICLES, which is driven by atmospheric and ocean model predictions. It is well known that numerical simulation of the grounding line migration needs a high level of horizontal resolution. The authors in this paper applied a high resolution regional ice sheet model to each sector of the WAIS and evaluated detailed grounding line migration processes and sea level equivalent ice volume changes in the region. Furthermore, the topic is within the scope of this journal. The result of prognostic runs and estimated sea level changes are also carefully written. Therefore, I recommend this paper for publication provided certain corrections and minor revisions are applied. Though quantification of ice volume sensitivities on mesh resolution is certainly an important result of this study, its presence in the body of the paper detracts from the main thrust. The estimation of sea level values in the coming centuries using the WAIS atmospheric and ocean models is paramount here. With this in mind, I recommend that the section on ice volume sensitivities on mesh resolution be included into the appendix or supplementary information.

We agree that the paper is primarily about the response of the ice sheet to future forcing and have moved the section on mesh resolution to the appendix and summarized its results in the main text.

I offered these comments based on the current structure of the manuscript. However. I also welcome it to be modified the structure based on the suggestion of another reviewer. If there is no significant change in scientific results, there would be no trouble for publication.

We have moved a large amount technical detail from the main text to the appendix, in line with the advice of referee #1 and above.

P1899. L9 - 17. Because ice velocity has a certain range, it is better to report the percentage of the mismatch, not only the magnitude of difference.

These numbers are now in appendix B (page 28 in the annotated revised manuscript) As suggested, we reported the ratio of the mean absolute values of the speed mismatch to the mean observed speed in the text. Since reporting the relative and absolute differences in the text results in a long list of numbers, we made a table (table 2) which shows the mean absolute values of the speed mismatch and the mean observed speed.

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P1901. L14 - P1902. L8. Change of stiffening factor also modifies ice velocity, which is adjusted by the inverse method described in 2.4.1. It would thus be possible to move this part into or after that subsection. The rest of this subsection (1902. L9-25) concerns adjustment of surface mass balance. It would be more appropriate to title this subsection as such.

Most of the detail of the inverse problem was moved into the appendix. The modification to the stiffening coefficients is included with section B1 of appendix B (which includes the material from the original subsection 2.4.1. and is titled "Basal friction and stiffening coefficients") section B2 is titled "Relaxation, initial accumulation and initial melt rate".

P1903. L23. The authors describe numerical simulation settings carefully in the Method section, however, there is no description on some basic settings, such as the time step of the prognostic simulations, and the vertical coordinate resolutions used to calculate vertical shear stresses. Because the inclusion of the effect of vertical shear is an advantage of this model (Cornford et al., (2013); Fravier et al., (2014)), it is recommended that the authors add this information in an annex.

We agree that these data need to be included, but their description is brief enough to add the main text. We noted that the ice sheet is sub-divided into 10 layers in the description of the vertically varying viscosity (equations 5 and 6, page 5 in the annotated, revised manuscript). The timestep varies depending on the velocity and mesh resolution, so we added a brief description of that process to section 2.3 (adaptive mesh refinement)

P1909. L13 - 16. Several different topography datasets are used in this adaptive mesh model. Although Sun et al. (2014) shows that a lower frequency scale topography is more important than a high frequency scale topography, does the result partly depend on topography dataset resolution? ALBMAP (5 km resolution) is used in RISFRIS and MBL experiments. The other custom topography map based on Bedmap2 (1 km resolution) is used in ASE experiments. The latter can be more sensitive than the former. For example, one kilometer is one fifth of ALBMAP and just the same size of Bedmap2.

We have added a note on this to the text. As noted, the model is less senstive to random finer scale perturbations, though specific fine scale features might still be important. Bedmap2 has a nominally finer resolution than ALBMAP but is still derived from flightlines separated by ∼5 km outside of the ASE (though with more extensive coverage in some areas). We have begun to work with Bedmap2 for the remainder of Antarctica, but have not carried out the same experiments, so while we see the same qualitative dynamics, we cannot comment with more authority than we have in the revised manuscript.

Figure 3. Although it is clear, it is preferable to write what Figure (a) or (b) represent.

Agreed, and done. Figure 3 of the original submission is now part of appendix B, it is numbered figure 13 in the annotated, revised manuscript

Figures 15 and 16. It is difficult to distinguish each line even in the on-line document, particularly in the region of the first one hundred years. If possible, use a non-linear scale, a log-scale, or split up the figures.

It is certainly difficult to separate the lines in fig 15 and 16 (now 5 and 6) especially in the first 100 years. We tried a number of non linear scales, but they have one or two problems. The first is that it becomes harder to spot the broad acceleration of mass loss after 2100. The second is the the appearance of large positive values of (say) -log(-delta VAF) when delta VAF is small, which causes tiny fluctuations at the start of the simulations to dominate the graphs. Instead, we have modified the symbols used, marking all A1B results with filled symbols and E1 with wire-frames, and HadCm3 results with squares and ECHAM5 results with triangles. That helps to pick out the data after 2100. Before 2100, the results are so similar that is still difficult to pick them apart, so we added magnified versions covering the period 1980-2100

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to the supplement. The supplement also contains a .csv file containing the data from these plots, and we have made mention of it (and the magnified figures) in the revised manuscript.

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