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Interactive comment on "Experimental protocol for sealevel projections from ISMIP6 standalone ice sheet models" *by* Sophie Nowicki et al.

Anonymous Referee #2

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General comments

This paper documents the experimental setup for standalone ice sheet model intercomparison experiments using a variety of climate model forcings from CMIP6. This is a tremendous undertaking and these kinds of experiments are vital in assessing the sources of uncertainty in projections of sea-level rise over the coming century. While I recommend the paper for publication with a few revisions, I have some concerns about the design of the experiments and what they aim to test. At the same time, I realize that this project has to meet the conflicting demands of running a thorough experiment that tests a wide range of parameters, and creating a protocol that ice sheet modelers can follow.

Specific comments

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The experiment aims to test the sensitivity of sea-level projects to many variables: which ice sheet model is used, which future anthropogenic forcing scenario (RCP2.6 vs 8.5) is used, which atmosphere-ocean general circulation model is used, and how the outputs of each GCM are downscaled from their native resolution to the relatively finer resolution of ice sheet models. The authors themselves state that the AOGCM forcing needs to be supplied to ice sheet models in a "uniform, standardized manner" (pg. 2 lines 29-30). While this protocol will serve as a valuable guide for future sea-level projection experiments, I think there are two respects in which the uniformity could be improved.

The experimental design uses six AOGCMs for Greenland and and six for Antarctica, but only two models are common to each (CSIRO-Mk3.6 and HadGEM2-ES). Several others are clearly related, for example, MIROC5 and MIROC-ESM-CHEM, but there's nothing in the text explaining the differences. The authors cite Barthel et al. which is currently in review but some summary of the differences would be worthwhile. It's not my place to review the Barthel et al. paper, but including GCMs that meet the criteria for inclusion for only one ice sheet but not the other is a departure from the authors' stated goal of uniformity. An argument could be made here that less is more. There's a similar problem with the climate forcing scenarios – RCP2.6 with some models but not others, no intermediate climate forcing.

The biggest issue I have is with the climate model downsampling. The Greenland runs use the regional climate model MAR, while Appendix C seems to say that the Antarctic climate model output was directly interpolated onto the ice sheet model grid . The authors state that using a RCM for Antarctica was prohibitively expensive. I certainly won't argue that point but several of the coauthors of this paper have run MAR for Antarctica (Agosta et al. 2019, Estimation of the Antarctic surface mass balance using the regional climate model MAR) and doing so for this study would be a big improvement. In principle the authors could test whether the downscaling or the choice of climate model had more of an effect by also interpolating the GCM output directly

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for Greenland and comparing the results. But this might not be very informative for Antarctica as the two continents have different topographic relief. The experiment is consistent in using the same parameterizations to extrapolate the oceanic variables for both ice sheets, and it would be great to see the same methodology applied to the atmosphere too.

Technical corrections

Page 5, line 5: criteria

Page 6, line 6: scalar

Page 7, line 2: Time-dependent data assimilation does a much better job about transients, see e.g. Goldberg et al. 2015 or Gillet-Chaulet 2019.

Figures 2-5: These figures are difficult to parse visually. Some way of showing the difference between the RCP2.6 and RCP8.5 scenarios would be especially helpful, either by using dashed lines for one scenario in the same plot or, better yet, putting the two on different plots entirely.

Page 18: Several paragraphs repeat information that's already in Slater et al. This paper is long as it is and a shorter summary of this would cut down on length some. Likewise the discussion of the results from Jourdain et al.

Page 21, line 8: the second period. . . lasts

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