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Earth System Dynamics Discussions

# *Interactive comment on* "Projecting Antarctic ice discharge using response functions from SeaRISE ice-sheet models" *by* A. Levermann et al.

**Anonymous Referee #2**

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In this paper, the authors use five different Antarctic ice-sheet (AIS) models that participated in the SeaRISE intercomparison project to estimate ice-sheet response functions for four sectors of AIS. These response functions are constructed so that they can be convolved with melt rate to yield total ice sheet discharge over time. Under the assumption of (1) linear responses of subsurface marine temperatures to global mean temperature (using patterns in this case derived from the CMIP5 models), (2) linear responses of melt rates to subsurface marine temperatues, and (3) stationary ice-sheet response functions, these ice sheet response functions are combined with probabilistic global mean temperature projections to yield probabilistic projections of AIS melt over the 21st century. While each of the core assumptions introduce important limitations that, in some cases, shoud be highlighted more clearly, the approach taken is a very reasonable one, and I would recommend this paper for publication with minor revision.



The most important caveat, which I would highlight more clearly, is that the AIS models may be missing core physics that could affect projections under all scenarios. At times, the authors suggest that the assumptions of their linear projections are most likely to fail under the low-emission RCP 2.6 pathway and are more likely to hold under the high-emission RCP 8.5 pathway (e.g., 1128). However, one might note other work, such as the recent work of Rob DeConto on ice-cliff collapse and melt-enhanced calving suggesting that near-meter-scale AIS contributions are physically plausible under RCP 8.5. This physics is not included in any of the AIS models. Conversely, I believe the negative feedback on marine ice sheet melt provided by static equilibirum sea-level effect (Gomez et al., 2013, doi:10.1016/j.epsl.2013.09.042) is also not included in any of the models, with the possible exception of the Penn-State-3D model. I would accordingly suggest somewhat greater modesty in the presentation of the probabilistic projections.

Minor substantive comments

Page 1121: "The full uncertainty range of future climate change for each of the Representative Concentration Pathways (RCP, Moss et al., 2010; Meinshausen et al., 2011a) using the current simulations from the Coupled Model Intercomparison Projection, CMIP-5 (Taylor et al., 2012)" does not in fact describe what the authors do – a good thing, since the CMIP5 ensemble is an ensemble of opportunity, not a probabilistic distribution representing the full uncertainty range. In fact, the authors use MAGICC, a

Page 1131: Is Schewe et al. (2011) the right citation here? Schewe et al. (2011) appears to use MAGICC6 to emulate 19 AOGCMs, whereas here the 600 time series projected seem to use MAGICC as described in Rogejl et al. (2012, doi:10.1038/nclimate1385).

Page 1131: Is there a bias-correction applied to the CMIP5 model output before a scaling relationship is derived from them?

Page 1132: Clarify (if true) that the basal melt coefficients are drawn from a uniform distribution.

Page 1134: I assume the authors discard the misfit between their linear fits determined for the CMIP5 coupled climate models subsurface ocean temperature and global mean temperature projections. If this misfit were retained as an error term, would it significantly affect the results?

Page 1136: "The long tail towards higher sea-level contributions makes the estimate of the 90%-range of the distribution (thin horizontal lines at the top of each panel) very difficult, because it is based on few extreme combinations which might not be robust." This is surprising to me given the 50,000 Monte Carlo draws – there should be 2,500 draws outside the 90% range, which I would think would make the range fairly robust. Have the authors checked the stability of these estimates?

Page 1136: I would think the authors' approach would provide a useful method for estimating the covariance of melt between different ice sheets, providing a useful method to probabilistic projections methods such as those of Little et al. (2013, doi:10.1073/pnas.1214457110).

Page 1137: "The aim of this study is to estimate the full range of potential sea level rise caused by future ice-discharge from Antarctica." The paper does not do this, nor is it capable of doing this given the potential omission of important physics. See comment above, and page 1138, which states "estimates may not cover the full contribution from consecutive, potentially self-accelerating grounding line retreat which may be significant. "

Page 1137: "The largest uncertainty in the future sea-level contribution estimated in this study arises from the external forcing." Please clarify that the forcing uncertainty referred to here is not the range of global forcing as represented in the RCP, which has



a second-order effect, but the uncertainty resulting from translating global temperature to melt rate.

Page 1139: "This is particularly relevant for weak forcing scenarios in which an instability might be triggered but the directly forced ice loss is weak. For strong forcing scenarios like the RCP-8.5 the forcing is likely to dominate the dynamics." I don't think this point is established; see comment above.

Minor presentation comments

Page 1121, line 28: "allows to" -> "allows us to"

Page 1122, line 9: "model's" -> "models"

Page 1122, line 11: It is unclear what the antecedent clause for "which is a possible responseto enhanced ice flux and upstream thinning" is.

Page 1122, line 23: strike "of"

Page 1135, line 13: "capable to simulate" -> "capable of simulating"

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Interactive comment on Earth Syst. Dynam. Discuss., 4, 1117, 2013.