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

Anonymous Referee #1

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General comments:

This paper represents a comprehensive effort to digest the information provided by a wide variety of disparate ice-sheet/ocean model runs that address Antarctica's potential contribution to sea level rise. The goal of the analysis is ambitious: to come up with a "consensus" notion of how warming ocean will affect sea level via the basal melting of ice shelves and subsequent impacts on the flow of inland ice across the grounding line.

The paper, especially where it describes the methodology employed, is a bit difficult to understand. This may stem from my lack of familiarity with the various models, however.

I was somewhat expecting the analysis to derive an empirical response function R(tau) from the models that was more simple than the derivative form in equation (4). By using the derivative form in all its tedious detail, the only thing that the response function provides is a way to "normalize" the ice sheet model experiments to a common single set of forcing conditions. This is valid and OK, but it probably means that some future study will come out with a more or less simplified (possibly empirically determined?) response function that may end up being more understandable and believable.

I was not sure I understood the "probabilistic approach" in section 4, and for what reason this approach is chosen over, say, a hand full of simple "scenario simulations". Why was the probabilistic approach needed? What problem did it overcome?

Finally, it would be interesting if the discussion or conclusion could set the context for the results. In other words, how do the results presented from the analysis here differ from what has appeared in other studies of the AR5? If there is disagreement, is this disagreement based on some specific identifiable element of the model treatments?

Specific comments:

- I find the abstract to be very long, and this may present problems for some readers. It would be better (possibly) to reorganize the abstract to summarize (a) the method, (b) the evaluation of reliability (this would be looking at the current response) and (c) the projections. With too much detail in the abstract, the messages become muted.

Page 1122 line 9 change "model's" to "models"

Line 23 remove the word "of" after "lacks of ice shelves"

Page 1128: line 2 change "capable to capture" to "capable of capturing"

Equation 4 implies that the response function R is a function of time that has to be evaluated at each time by differentiating S(t) with respect to time. Is it possible that R(tau) could be a much more simple or more universal function that can be derived by some other means than differentiating S(t) at each time t?

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Figure ordering: I notice that figures are referred to in a non-sequential manner in the text (by my count, Fig. 1 is cited first, then Fig. 5, then Fig. 13...) Is this OK for the style of ESDD?

Page 1130, line 25. I'm not sure what is meant by the "600 global-mean-temperature time series".

Page 1131. For the probabilistic approach, how does this approach differ from simply sweeping through all possible values of the global mean temperature time series and all possible values of the coefficient to translate ocean warming into sub ice melting? Also, if the process is random, it must mean that of the 50,000 experiments evaluated, some were evaluated more than once. Is experimental multiplicity recorded and evaluated?

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

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