Earth Syst. Dynam. Discuss., 4, C662–C664, 2014
www.earth-syst-dynam-discuss.net/4/C662/2014/

Earth System Dynamics Discussions
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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 #3

Received and published: 14 January 2014

Review ESSDD, 4 1117-1168 Projecting Antarctic ice discharge using response functions from SeaRISE ice-sheet models by A. Levermann et al.

The manuscript attempts to generalize the Antarctic ice discharge for large-scale ice flow models with a strong emphasize to 3 models, which include shelf dynamics and a focus on the next century. They combine uncertainties in forcing, model set up, ice shelf melting to arrive at probability density functions.

It is a useful attempt to summarize results from existing models, but interpretation of the results is somewhat superficial.

I am not convinced by the conclusion that the ice discharge is scenario dependent. I think this is a construct from the set up of the results. If you base your subshelf pa-

rameterization on a linear function of the change of the ocean temperature change at grid points surrounding Antarctica which are linearly correlated to the global mean temperature change from the CMIP models one must end up with a scenario dependent results. This is hence not a result of the experiments but a mathematical construct. Which CMIP models can be used in the current set up of ice shelf model parameterizations as used in the present manuscript to explain the current observations at the few individual drainage basins in West-Antarctica which are currently loosing mass? Do those models indicate dTo to be significantly related to dTglobal? There is no consistency in the variations of delta_T among the different models and regions, does this not indicate that a delayed correlation between dTglobal and dTocean is questionable? A few lines on the fact that ocean variability is not captured adequately would be helpful in the interpretation of the results.

I am also not convinced by the conclusion that the main uncertainty is introduced by the external forcing whereas at the same time you admit that the models are not adequately treating the grounding line dynamics. Does this not implicitly imply that the shortcomings in the physics/numerical details dominate the overall uncertainty.

The justification for only analyzing the shelf models is somewhat inconsistent as you admit that they are not adequate on page 1137/1138. It seems arbitrary to throw out the models, which don't show the results you want. If for instance response functions of the three shelf models were identical you would have an argument, but now you don't seem to have an argument to throw them out. Why not reason the Penn state model is the best and I base my frequency distributions on that model?

The last sentence of the abstract can be left out or needs further explanation why you think this is the case.

The use of the figures is rather chaotically. Please use the standard order for numbering as they appear in the text and don't refer to a figure in the abstract. Discuss the general features of different figures in the caption where they first appear and not in

Figure 7 whereas Figure 5 has the same set up. Insets should be readable (not the case for Figure 5), not lead to confusion (figure 1 and 13) The paper would benefit from an additional table summarizing the main characteristics of the five ice sheet models used including grid resolution. I would like to see information on the initialization of the models. Are all models initially in equilibrium? How is this achieved? As the focus is on the short-term response this is an important issue. In this context it would be useful to analyze the contribution over the last century, which is available from the models. Is it in agreement with the literature values on this?

You constantly discuss pdfs whereas you mean frequency distributions.

-Page 1121 line 9 will likely to be replaced by are here assumed to -Page 1122 line 1 confusing reference to work by Bindschadler. It suggests that they did what you are also going to do summarizing results. I rather prefer to have a short statement what follows in this paper. -Page 1122 line 23 remove of -Page 1122 line 24 change only with a bed to whenever the bed is -Page 1129 line 19 add importance after second-order -Page 1130 line 27 inform the reader immediately that derivation of the scaling coefficients follows. -Page 1136 line 11 span the full range of responses. How do you know? You mean within the current set up, tune down the statement -Page 1137 line 6, address IPCC properly -Page 1138 line 21 this conclusion depends on the fact that the grounding line migration in your models is probably too slow, please not that.

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