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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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This work by Levermann and co-authors, using linear response theory, interpret the seaRISE modeling exercise over the Antarctic ice sheet to establish projections of Antarctic contribution to sea level rise.

I have two essential comments. First, it is now clearly established that most of the models used are not able to confidently investigate the effect of ice shelves melting perturbations onto the upstream grounded ice sheet. This casts some doubts on the approach they used. Second the methodology used seems inappropriate and needs to be clearly justified. As a consequence, I believe that the proposed projections are not reliable. I would therefore not recommend this paper for publication.

The authors propose a statistical analysis of all the models applied to Antarctica in the seaRISE benchmark in order to establish sea-level projections. However, two of the

models (AIF, UMISM) do not represent ice shelves. This is embarrassing when the experiment used is a perturbation in the sub ice shelves melt! In that cases, melt rate seems to be crudely parametrized at the ice sheet front. But it is known that the spatial distribution of melting below the ice shelf is capital [Gagliardini and other 2010], casting some strong doubts on the obtained results. Moreover, according to the brief description of the models, 4 of the 5 models have most probably inappropriate resolution of the grounding line problem (AIF, UMISM, SICOPOLIS and PISM). Spatial resolution of the mesh at the grounding line is known to be capital to have consistent results of the coastal dynamics (Vieli and Payne, 2005). There is no detail on the spatial resolution used, but it is presumably in the order of 10 km. With the numerical scheme used in these models this is about two orders of magnitude to large to obtain results that are not deeply affected by numerics (Pattyn et al., 2012). In summary, only the Penn-State-3D model seems appropriate to produce sensible projections. Therefore, the proposed study may be valuable to investigate the dispersion of the models, definitively not to "average" the result of all the models and propose reliable sea-level rise projections.

Authors use the linear response theory. I am not convinced it is appropriate and it would require to be clearly justified. Linear response theory assumes that the response to a WEAK external perturbation is proportional to the perturbation itself. Authors justify the pertinence of using such a theory by arguing that the duration of the simulation (100 years) is a relatively short period for the response of an ice-sheet. This sounds as a weak argument. I believe that their Figure 3 clearly demonstrates that the changes after 100 hundred years are absolutely huge (West Antarctica has almost collapsed according to the only reliable model (Fig. 3a)). It is presumably not a weak forcing that would drive such changes. Furthermore, hypothesizing a linear response of the sea-level contribution from ice discharge sounds in contradiction with the hypothesis of marine ice sheet instability [MISI, Schoof 2007]. In a case of an overdeepening bedrock (most of the outlet glaciers in West Antarctica are in a such topographical configuration) there is a clear threshold in the forcing. Below that threshold the retreat of the grounding line and volume change are limited, above that threshold, retreat is continuous and discharged volume is dramatically high. Volume change versus perturbation should therefore be a step function. The Penn-State-3D includes the boundary layer theory proposed by Schoof [2007], so it inherently reproduces MISI (and it takes place as the retreat is extremely large after 100 years, see Fig 3a). If authors observe a linear response, presumably only one of the step is represented in the experiment. But there is no indication where the threshold in forcing is. Any scaling of the forcing may overpass the threshold and therefore the linear interpolation would be completely out of range. Validity of using linear response theory in that particular case must be strongly discussed.

In summary, only one model appears reasonable to produce Antarctic volume projections, but the method used to process the outputs seems in contradiction with the physics implemented into that particular model. Large efforts have been engaged in the seaRISE benchmark, by a lot of groups, and there is probably a lot to learn in the comparison of the models. However, according to the title the aim of the present paper is to establish projections of ice discharge. The pertinence of using the seaRISE benchmark to compute reliable projections of the Antarctic discharge would have to be clearly demonstrate. To my opinion, it is clearly not the case in the present manuscript.

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