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The Cryosphere 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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Projecting Antarctic ice discharge using response functions from SeaRISE ice-sheet models

General Appreciation

This paper describes an assessment of SLR contribution of Antarctic ice sheet models due to atmospheric/oceanic forcing. The evaluation of the scenario's is done using linear response theory based on a temporal stepwise increase in basal ice shelf melt rate up to 20 m/a. The participating Antarctic models are all different in their physical representation of processes and numerical treatment. While this is potential an interesting as well as important contribution to IPCC AR5, the paper lacks sufficient scientific scrutiny. The major problem is that the differences between ice sheet models is well beyond the difference in ice dynamics/physics, mass balance treatment and basal slid-

ing parameterization. It is heavily biased by the numerical treatment of grounding line and ice shelf dynamics, which may even incorporate a much higher bias than the one that is attributed to the cited processes. Recent theoretical advances (Schoof, 2007) and numerical model intercomparisons (MISMIP; Pattyn et al., 2012) have demonstrated what model requirements are to accurately represent grounding line migration. Most models that participate in this SeaRISE experiment have either not performed such tests are have shown that they fail to reproduce reversibility (advance/retreat) of steady-state grounding line positions under simplified conditions. In any case, finite difference models that have a too coarse grid resolution at the grounding line will never produce a reversibility, unless a parameterization is introduced in which the necessary boundary conditions at the grounding line are implemented. The latter can be on the basis of a heuristic rule (See for instance Pollard and Deconto, 2012). Furthermore, longitudinal stresses should be evaluated at both sides of the grounding line, which automatically invalidates SIA models, as shown by Schoof (2007).

Another issue is that the response of marine ice sheets to melting under the ice shelf are largely depending on WHERE precisely this melting is applied. Models with ice shelves apply it under the ice shelf and models without a floating shelf at the grounding line. This implies that two different perturbations are used that cannot be compared. A detailed analysis of the effect on melting under ice shelves is given by Gagliardini et al. (2010) as well as in Dupont and Alley (20XX). Either every model applies melting at the grounding line, either models without ice shelves should be removed from the analysis, to make comparison possible.

A distinction should be made between the uncertainty stemming from ocean and climate models and the one from ice sheet models. The fisrt type of models have an uncertainty pertaining to parameterization of unknown or poorly understood processes, such as representation of clouds in atmospheric models and representation of subshelf cavdities in ocean models. Similar uncertainties exist in ice sheet models, regarding basal sliding, grounding line migration and ice shelf buttressing. This issues are not solved. However, we currently possess the tools to identify what type of models could eventually qualify in representing these processes. We do know how, under simplified conditions, grounding lines should behave. We do know that ice shelves cannot be removed if we want to study ice-ocean interaction. Ice sheet models of poor spatial resolution, according to shallow ice approximation or an approximation that does not guarantee stress transfer across the grounding line disqualify.

I would suggest that models that are invalid for these obvious reasons are removed from the analysis and that only models that can demonstrate that their grounding line result is not biased by numerical issues, even if this would mean that potentially only one model produces results. The analysis could be repeated and the uncertainty would not be due anymore to obvious numerical reasons.

I made more detailed comments below.

Comments

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Line 20: Models that do not incorporate ice shelves do have a completely different sensitivity than models that do. Recent evidence has shown that ice shelves do matter in the response to the ocean in transmitting the loss of buttressing signal to the inland ice sheet. So, if ice shelves are not included in participating models, they will bias the results of the analysis considerably, thereby introducing a large error since *in se* they cannot deal with such dynamics.

Line 25: The time delay also contributes significantly to the response time and should therefore be analyzed in detail.

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Line 7-9: The study from Bamber et al does not show THE potential of WAIS to contribute to SLR. This is an *ad hoc* cartography of the grounding line position according to the stable/unstable slope idea (in absence of buttressing) and an ice sheet to relax

to this position. The potential for WAIS is probably a larger number, as for instance shown by disintegration of the ice sheet in Pollard and DeConto (2009).

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Line 20: The applied melt rates of up to 20 m/a demand some more explanation. These are rather large values. Even though such values (and even higher) are observed at certain ice shelves for given periods, this is not scalable to the whole of the Antarctic ice sheet. Secondly, the large melt rates may also alter the cavity shape, potentially leading to changing melt rates (either redcution or enhancement). This should be briefly explained in the first place.

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Altough the details of the models are given in another paper, it is important to summarize the important elements of the participating models, at least those factors that relate to grounding line response. Spatial resolution and especially spatial resolution at the grounding line is a key parameter in understanding grounding line migration (retreat). It should be given for all models.

Line 8: I am not sure whether this model qualifies as a higher order model. It is evidently different from SIA (Zero-order model) and includes longitudinal stress gradients in the effective viscosity term, but from the paper i guess that they are not included in the force balance. For instance, the basal shear stress is only given by driving stress. Correct me if i am wrong. For a nomenclature on full Stokes approximations, one can refer to Hindmarsh (2006).

Line 15-16: The model has not an ice shelf, so cannot qualify to incorporate buttressing; In principle, having ice sheet models without shelves would be a good metric to compare them to models that do have ice shelves in order to interpret the spread as the effect of buttressing, as is probably the intention of this paper. However, as shown by Gagliardini et al (GRL, 2010), it is very important where exactly this melting occurs.

The reaction is completely different if it is at the grounding line or underneath the ice shelf. So comparing AIF to Penn-State 3D, for instance, is not a comparison as not the same forcing is applied and the differences could be more due to forcing than to model differences, e.g., buttressing.

Page 3453:

PISM: 'The grounding line is not subject to boundary conditions'. The experiemnts shown in Winkelmann et al (2011) clearly demonstrate that at the spatial resolution used here, the reversibility of the grounding under simplified conditions (MISMIP experiments; Pattyn et al, 2012) is not guaranteed. This essentially means that the retreat of the grounding line is different than, say, the Penn-State 3D model, because the grounding line is not resolved. While advance could be simulated reasonably well, retreat is not. So, differences will be due to numerical issues that are identified and can be solved, and not due to differences in the physics or treatment of boundary conditions in the different participating models. (see also a remark further down)

Page 3454:

UMISM does not incorporate horizontal advection, if i am correct (I did not check the papers by Fastook and if i am wrong, please disregard this remark). This model will therefore over-estimate the heat budget of ice streams where horizontal advection plays an important role in cooling down the bed and (partially) compensating for frictional heating and dissipation due to sliding. This should be taken into account in the analysis, as this may have an impact on the results as such. Since subglacial water plays a dominant role in this model, the lack of cooling may overestimate the sliding produced. Furthermore, the lack of ice shelves also makes the model not comparable for an applied forcing, because the forcing is applied at the grounding line through a thinning function. This point should be better explained, because it is essential to understand how the forcing is applied and should not be looked up in Bindschadler et al. (2012).

Page 3457:

Line 11-13: The spatial distribution of ice loss after 100 year does NOT illustrate the different dynamics of ice sheet models ALONE. It may well reflect to a large extent (which is possible to evaluate by doing MISMIP-type experiments) numerical issues between the model beyond the physics, ice dynamics mass balance and basal sliding parameterization. This is a serious issue.

Line 25: I disagree; this is not capturing the uncertainty range, because uncertainty can easily be altered by avoiding to have model response due to numerical problems that are identifiable. If you would like to capture the full range of uncertainty, you could also include a basic isothermal 2d plane SIA model on a 50km grid (runs very fast) and add it to the range. I use such a model in the classroom. We know it is wrong, but it is not so wrong with respect to ocean contact dynamics than other models that participate in the test. I would therefore continue the analysis solely with those models that capture at least grounding line mechanics with ice shelves. (buttressing).

Page 3463:

Line 23-25. I am not suprised that the weakest response comes from PISM, compared to a stronger response from Penn State 3d. The former has issues with grounding line retreat due to the coarse spatial resolution (which could be resolved by locally increasing the resolution); the latter has proven the reversibility of grounding line migration under simplified conditions. The differences in response can probably be largely attributed to this difference.

Page 3464; top:

UMISM shows strong melting along the whole coast and via the thinning function to translate this melting at the edge also shows a strong response. This is also an identified problem which we know is unrealistic and can be avoided by removing the model from the analysis.

Interactive comment on The Cryosphere Discuss., 6, 3447, 2012.