Detailed response to the reviewers´ comments of the manuscript "Projecting Antarctic ice discharge using response functions from SeaRISE ice-sheet models" by Levermann et al.

General response:

We would like to thank the reviewers for their time and very thorough evaluation of our manuscript. We have prepared a detailed response to their comments and we have made **significant changes** to the manuscript and to all figures and tables in order to improve the manuscript in accordance with their suggestions. We do appreciate the assessment by Eric Larour (reviewer #3) who states that the "(...) *study has strong implications in that it will enable better estimates of the contribution of ice sheets to the present sea level rise, which has so far been neglected in established projections (...)*". We are however confident that we can also convince reviewers #1 and #2 that the approach taken here is indeed valid to estimate the uncertainty range of future sea-level contributions from solid ice-discharge from Antarctica. To this end, we would like to address the main criticism upfront and provide more detailed responses to the specific comments below. There are a number of arguments that would suggest that this paper should be published as a **contribution to the discussion of sea-level contribution** from Antarctica. We would like to list these arguments here upfront:

1. **General purpose of the study:** The first point that might not have been made sufficiently clear in the first submission is that the paper tries to estimate the **uncertainty range** of the sea-level contribution. As was perhaps not clear from the first submission this uncertainty arises mainly from the uncertainty in the external forcing. While we do not have the necessary experiments from all the ice-sheet models to make this point quantitatively explicit, this issue is further illustrated in a new figure that we have added (figure 12). When computing the response function of the ice-sheet models from the M1-SeaRISE experiment which applies 2 m/a of additional basal ice-shelf melting, then the response functions are much more noisy and look quite different compared to the response functions obtained from the M2-experiments (20 m/a) which we use for most of the study. We argue that the response can be better estimated from the stronger M2-experiment because the signal-to-noise ratio is better. However, the point here is that while the response functions look quite different for the two experiments, the uncertainty range of the sea-level projections until the year 2100 are very similar for both choices. It is the magnitude of the continental response of the ice-sheets to enhanced basal ice-shelf melting on a decadal time scale that is relevant for the uncertainty estimate. In order to estimate the uncertainty range of the continent´s sea-level contribution until 2100,

it is thus more important (1) to capture the response of the full ice-sheet and (2) to capture the full range of uncertainty in the forcing than to precisely represent the local ice-sheet dynamics. This is the focus of this study. This issue is now extensively discussed throughout the paper, particularly in the discussion and the appendix.

2. **Hind-cast of observed sea-level contribution (1992-2011):** We have added a full subsection (5.2) including a figure and an associated discussion on the models ability to hind-cast the sea-level contribution from Antarctica as derived from observations for the period 1992-2011. Here we relate to the recent paper by Shepherd et al. (Science, 2012) and show that the range of sea-level contribution as computed with this method using the models with explicit ice-shelf dynamics is almost exactly the same range as obtained from observations (figure 7). This emphasizes again the importance of the probabilistic approach taken here. While not all climate models will provide the correct forcing at each coastal region of Antarctica for the 20th century and while not each ice-sheet model is able to respond to the forcing in a precise manner, the sea-level contribution from Antarctica is comprised of the ice-sheet's response to a spatially broad range of forcing. The probabilistic approach taken here aims at capturing the combination of different imperfect methods to obtain a range that encompasses the real solution in a statistical sense.
3. **Model choice (ice-shelf models):** We agree that the response to basal ice-shelf melting is better captured with models which explicitly represent ice-shelf dynamics. We have therefore changed all figures in such a way as to show the ranges obtained from the three models with explicit ice-self representation only. We also provide only these ranges in the abstract now. We kept the other models in the paper and provide numbers in the final table 6 in addition to the numbers for the ice-shelf models but explain explicitly in the discussion why these are less likely to properly represent reality. This is also consistent with the fact that the hind-cast including the non-ice-shelf models is not consistent with the observed range.
4. **Model choice (grounding line motion):** A criticism raised by both the first and second reviewer relates to the ability of the applied ice-sheet models to represent grounding-line motion. For this and other reasons discussed, we have decided to base most of the figures and tables on the models with explicit ice-shelf representation. We further agree with the reviewers that the models applied here PennState3D, PISM and SICOPOLIS are not able to perfectly reproduce the analytic solution of the MISMIP or the Full-Stokes solution of the MISMIP-3d intercomparison. We have added statements for each model discussing the specific performance with respect to the MISMIP experiments. While the PennState3D model performs reasonably well in MISMIP, the SICOPOLIS model has not participated because a new numerical solver was introduced to the model and it was simply not ready for the intercomparison in time. In the meantime the model´s horizontal ice-shelf velocity field has been tested positively already at low resolution against an analytic solution

as documented in the PhD thesis of Sato (2012). This is now stated in the text along with the clear statement that a validation of the grounding line motion is missing. The PISM model has also undergone a numerical improvement since the submission to the MISMIP-3D intercomparison. This changed has improved the performance with respect to the grounding line motion dramatically. Please see the figure R1 and the figure caption at the end of this response where you find in the upper panel the old performance and in the lower panel the new performance as applied in all the SeaRISE publications and also in this paper. This performance is only achieved at significantly higher resolution than applied in this study. Consequently, we have added a very critical statement at the end of the PISM-description and in the discussions, clearly stating that the model can only perform well at resolutions below 1 km.

We would like to add the following general note in order to put the role of the grounding line motion into perspective with respect to **this particular study here**. Currently, there is a strong scientific focus on the motion of the grounding line under external forcing within the cryospheric community. We would like to emphasize a number of issues that might not have become sufficiently clear in the paper. According to the 19 comprehensive climate models applied, the atmospheric warming arrives at the entrance of the ice-shelf cavities with a time delay of several decades. We apply a broad interval of coefficients to translate this time-delayed temperature increase into basal melt rates. The ice-sheet response (to this basal ice-shelf melting) with an increase of the ice flux across the grounding line which is the main sea-level signal that is detected in this study. In some places this increased flux leads to a thinning upstream of the grounding line which, if sufficiently strong, leads to a retreat of the grounding line. The three models applied are capable of modelling grounding line retreat and they show grounding line retreat in response to the climatic forcing applied. The main signal that is computed in this study, however, arises from the enhanced ice flux across the grounding line and not from the consecutive potentially self-accelerating motion of the grounding line. While it needs to be made very clear in the text that this might not be the full contribution and that grounding line motion might take a significant part, we would like to publish the ice-flux response to the large range of climatic forcing obtained from the climate models. It needs to be taken into account that we are estimating the sea-level contribution for the next 100 years in response to a <u>time-delayed</u> warming signal. For this specific constellation we believe the estimate obtained from the ice-flux with a possibly under-estimated grounding-line motion provides interesting information about the sea-level contribution from Antarctica - even if the models' shortcoming in grounding line dynamics indicates that the ice flux changes are not modelled perfectly.

5. **Linear response theory:** Our previous response (no. 4) also relates to the second major criticism of the first reviewer. The reviewer hypothesizes that the sea-level

contribution from Antarctica during the 21st century will be dominated to a large extent by the marine ice sheet instability. As a consequence an irreversible grounding line motion will be triggered which will be independent of the forcing and thus the linear response method is not valid. We agree that if the ice loss due to an instability is faster than due to the external forcing than this additional ice loss will not be captured by the linear response theory. This is particularly relevant for weak forcing scenarios in which an instability might be triggered but the ice loss that is directly due to the forcing might be weak. As explained above, we provide an estimate of the enhanced ice-flux due to a time-delayed oceanic warming signal for the next century. We have added a detailed discussion in the paper on this issue.

6. **Transparency of results:** It is important to note that we present only one method to estimate the uncertainty range of the sea-level contribution from Antarctica. Our method focuses particularly on the uncertainty in the external forcing. While the method is not perfect, we would like to second the opinion of reviewer #3 that this work is an advance towards an assessment. In our opinion, the paper should be rejected only when false. We would like to argue however that we present a well-established method applied to well-documented models in a transparent way. As a consequence, any reader with sufficient background can judge the results by his- or herself. While the reviewers might not fully agree with the interpretation of the results as a good estimate of the uncertainty range of Antarctica´s future sea-level contribution, the methods are transparently described for every reader to judge. We thus would like the reviewers to support publication of this manuscript after the necessary discussions of the short-comings are included.

Some general technical comments to avoid confusion:

Please note that we have altered the uncertainty ranges in order to be consistent with the likely range of the IPCC. Different to the first submission we now provide the 66% percentile around the median as opposed to the 33-66% range which we accidently provided earlier. Also a numerical deficiency of the PISM model was corrected throughout the SeaRISE publications. The performance of the PISM model in the MISMIP experiments has improved significantly due to this change. As a consequence results for the PISM model are now generally between those of SICOPOLIS and PennState3D. The projection ranges have changed slightly, but not significantly, due to this improvement. They have changed however due to the different percentile that is now presented. Due to an interest by the IPCC chapter, we also provide the 90% percentile.

For the editor´s and reviewers´ convenience:

We provide the following files:

- Response letter (RevisionsLevermannEtAlTCD2013.pdf)
- New manuscript (SlrSeaRiseRfunction.pdf)
- New manuscript with changes compared to the old manuscript in bold-font (SlrSeaRiseRfunction_corrections.pdf)

We have indicated at each response whether we agreed with the suggestions of the reviewer and have changed the manuscript accordingly but putting "**Done**" in front of our response. In rare and minor cases where we have not obliged we put "**Not done**".

IPCC-Deadline

While **this should not influence the outcome of the review process**, we would like to point out that the deadline for manuscript acceptance of the IPCC is the **15th March** 2013. At the same time we would like to apologize that the resubmission too longer than expected and will do our best from our side to meet this deadline.

Reviewer #1:

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 too 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 such a 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.

Response:

We are sorry to have provoked such strong negative response. Since the criticism is quite fundamental and the first point relates to a criticism by the second reviewer, we have addressed the reviewer´s comment in the general response at the beginning of this letter. We disagree with the reviewer's assessment on both points. We hope that after improving our manuscript in the way state above and with our explanation given the reviewer will agree to the publication of the paper.

Reviewer #2:

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 sliding 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 first 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 cavities 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.

Response (Done):

We would like to thank the reviewer for these suggestions and comments. We have changed the manuscript significantly and now show predominantly the results of the models with explicit ice-shelf representation. We have provided a detailed response to this issue and the other points raised in the general response in the beginning of this letter.

Comments

Page 3449
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.

Response (Done):

We agree and have changed all of the corresponding figures to show only the models with explicit shelf-representation. We keep the other models in some figure in order to illustrate the difference.

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

Response (Done):

We provide all results with and without time delay in order to bracket the real sea-level contribution from both sides. In principle, the climate models are not necessarily capable of reproducing the time delay properly as was shown for example by Hellmer et al. 2012 and Yin et al. 2012. Here we represent only the large-scale processes such as coarse resolution advection within the ocean. This is biased towards longer time delay. We thus show results with and without this time delay in order to bracket the behaviour of the real ocean. We agree that the time-delay of the forcing and the consecutive response of the ice sheet is an important topic. Such analysis would, however, significantly expand the scope of the paper and while it would be of interested for the ice dynamics it would not directly add to the estimate of the uncertainty range of the sea-level contribution. We would prefer not do the analysis within the framework of this paper.

Page 3450
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).

Response (Done):

We agree and have changed the sentence to: "While the part of the ice-sheet directly susceptible to ocean water on Greenland is limited, marine ice sheets in West Antarctica alone have the potential to elevate sea level globally by several meters (Bamber et al., 2010)."

Page 3451

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 reduction or enhancement). This should be briefly explained in the first place.

Response (Done):

We have done the following in order to meet the reviewer's very valid criticism: We have (1) added a discussion of the strength of the melt-rate compared to observations, (2) redone the analysis with the 2m/a response function and obtain similar results which we also discuss in the paper now, and (3) we have added a discussion on the fact that the geometry change of the ice-shelf cavity does constitute a feedback in the system which is accounted for, but potentially not in a correct fashion. We consider this effect to be of second order, but it is very important to mention it and we would like to thank the reviewer for pointing this out.

Page 3452

Although 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.

Response (Done):

We have added a description of the models ability to model the grounding line motion to each of the model description and to the final discussion.

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).

Response (Not done):

The models are well documented and we prefer to keep the exact description of the models to these papers. Otherwise we are losing focus in the current paper. We hope that the reviewer can live with this.

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.

Response (Done):

Agreed. We have reduced the presentation of the models without shelves to a minimum as describe above. Please see general points in the beginning.

Page 3453:

PISM: 'The grounding line is not subject to boundary conditions'. The experiments 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)

Response (Done):

Please see our general comments on the role of the motion of the grounding line. We have added discussions on this matter to the model description and the discussion. In addition to this we would like to add that the PISM model uses a superposition of the SIA and SSA which was shown by Hindmarsh and Schoof (2012, appendix) to be a valid though obviously not perfect representation of the transition zone and that a numerical deficiency has been removed in PISM which improves the performance in the MISMIP experiments significantly, though only for high resolutions (see general response above and figure below). Furthermore we have added text in section 3 to clarify that numerical implementation is an important cause of the differences between models.

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).*

Response (Done):

We agree with the reviewer and have added a sentence in the model description of UMISM. We now provide the main results with the set of models which explicitly model ice-shelf motion which excludes UMISM.

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.

Response (Done):

We apologize for bad wording here. What we mean by dynamics is the response of the model to basal melt without the surface mass balance changes. We reworded to be more careful and have now added explicitly the role of numerical representations.

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).

Response (Done):

We agree and have changed the text and the representation accordingly. Please see the general response for more detail.

Page 3463:

Line 23-25. I am not surprised 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.

Response (Done):

We identified a numerical problem in PISM which was resolved and has changed the response in such a way that the model now shows sea-level contributions between SICOPOLIS and Penn-State-3d. In any case, please not the general comment.

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.*

Response (Done):

We agree and have removed the model from most of the analysis (see general response in the beginning).

Reviewer #3:

General remarks:
the manuscript relies on the SeaRISE experiments to derive sea level rise projections, along with uncertainty estimates, for the next 100 years. As such, this study has strong implications in that it will enable better estimates of the contribution of ice sheets to the present sea level rise, which has so far been neglected in established projections such as the ones found in the IPCC AR4 report. I recommend publication of this manuscript, after some modifications to the manuscript.
I detail my remarks below, but the main comment I have about the manuscript is the fact that the critical point, Eq(1), is not explained well enough for a non-expert in SLR projections to understand. At p9: l13:20, the authors explain how they will use basal melt-rate as forcing and assess sea-level contribution from ice discharge. I believe this should be justified much more thoroughly. There is also ample room for confusion between basal melt, and basal sub-shelf melting. It would be nice to clearly define both terms, and find different ways of relating to them in the manuscript. In particular, I believe it necessary to explain the Bindschadler et al, 2012 sensitivity experiments a little bit more. It took me a while to understand that there was a distinction being made in the text between basal melt (which refers to the SeaRISe experiments), and basal sub-shelf melt rates, which are required to transmit climate forcing between global ocean temperature rise and increased ice discharge.
Maybe the last paragraph of page 11 should not be there, as it tends to introduce the confusion between basal melt rate and basal ice shelf melting. Another issue I have is the fact that increased basal melt rates are supposed to capture a signal in the atmospheric forcing (from increased lubrication, ala "Zwally" effect). There is no discussion relating this decrease in basal melt rate and the addition of another forcing through the basal ice shelf melting, which could be related. Increases in basal ice shelf melting will definitely impact ice flow dynamics, which in itself will impact basal sliding. Are there any feedbacks that would make using these two forcings redundant? or not self-consistent?
In the same line of thinking, it is not clear to me why the manuscript focuses so much on basal ice shelf melting, when at the beginning everything hinges on the response functions from the SeaRISE experiments which are based on a lowering of the basal melt rate. Clearly stating why this is being done at the onset of the manuscript would go a long way in clarifying some of the confusion I had.
Response (Done):

1. We have included a more detailed explanation of the response function method related to equation (1) in section 3.
2. In order to avoid the confusion between basal melt and basal ice-shelf melt we have always used basal ice-shelf melting and explain in the introduction that and why we focus on this change in the boundary conditions.
3. In the discussion section we have added a discussion on other ice-loss mechanisms under global warming and emphasize that here a specific change in boundary conditions is covered. The paper by Bindschadler et al. (2013) has now been accepted

Abstract: abstract is clear, concise, and to the point. I would avoid referencing tables, but I’m not sure about the policy of TC on this.
Response (Not done): We would like to keep it because the table is the main result of the paper and we would like a reader to be able to find this already while reading the abstract. If the reviewer or editor does not insist on removing it, we would like to keep it.

Detailed remarks:
p1. l8: please cite Mitrovika et al, 2009.
Response: (Done) We have cited the Mitrovika et al. (2009) together with Bamber et al and additionally in a new sentence in the introduction:
“These contributions might however be significant for the next century which would influence global mean (van den Broeke et al., 2011) as well as regional sea level changes (Mitrovica et al., 2009).”

p6. l9: use ”the” finite difference method
p6. l13: Weertman”’s” sliding law
p6. l14: ”the” ice sheet margin ...
p6. l14: rounding line − > grounding line
p6. l15: floating condition: may I suggest hydrostatic equilibrium?
p6. l15: lacks ice shelves
p6: overall, the AIF description reads more like a sequence of bullet points. Please put more links between the sentences.
Response: (Done) In response to the reviewer´s general request the description of the AIF model has been changed.

Figures:
the figures are clear and self-explanatory.
Response: Thank you.

Figure R1: Performance of the PISM model within the MISMIP-3D experiments. The upper panel shows the performance as submitted to Pattyn et al. 2013. The lower panel the performance used in this publication as well as in all SeaRISE publications. The profile after initialization is given in black (Stnd), the profile after enhanced sliding in red (P75S) and the profile after successive reduction of the enhanced sliding back to the initial value in cyan (P75R). The full-Stokes solution by the Elmer model (dashed vertical lines show the grounding line) shows an advance of the grounding line with enhanced sliding and a retreat to its initial position after the sliding was reduced to normal. While in the old PISM-version showed an advance of the grounding line even after the initial sliding was put back into place,the new version shows an almost perfect retreat of the grounding line to its initial position. While the absolut position of the grounding line is different in PISM compared to the full-Stokes solution, the magnitude of the change of the grounding line position is very similar.