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

We would like to thank the reviewers for their positive evaluation of our manuscript and for pointing out where the manuscript required clarification. We have done our best to provide the required changes and are confident that we were able to meet the requests. For convenience, the reviewers' comments are provided in red and our answers are added in blue and italic font.

Reviewer #1

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.

Response:
We would like to thank the reviewer for appreciating the goal of the paper and acknowledging its difficult objective. We would like to underline that it is not the aim of the paper to provide the one and only projection of future Antarctic sea-level contribution, but to describe a scientifically valid and transparent methodology together with the corresponding result. The more important it is to us that the methodology is described clearly and we appreciate the reviewer's suggestions of where we had not succeeded in the initial submission.

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.

Response:
We are not fully certain whether we understand the reviewers comment here. The linear response function of the ice models is defined as the models' response to a delta-peak forcing. Within linear response theory this may be obtained experimentally (in the sense of a numerical simulation) from the models' response to a step function or Heavyside forcing. This was the approach that we chose, because it was available from the SeaRISE experiments. This approach is experimental in the sense that it was derived from a simple set of (numerical) experiment and comprises a number of processes within the ice sheet models.
In an earlier publication, Winkelmann & Levermann (Clim. Dyn. 2013) had derived some closed formed functional formulas for some specific sea-level contributions, but in the current study the

attempt was to capture the response in a non-idealized way. We were not able to derive a response function from first principles as in the case of oceanic thermal expansion. If future studies can obtain such function that would be highly valuable and would allow a strong improvement of our projections. Unfortunately, we were not able to present such a derivation here.

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?

Response:
We would like to thank the reviewer for highlighting the difficulty in understanding the probabilistic approach from our previous description. We have now added a conceptual figure (new Fig 1) and some further explanation in the introduction and in section 4 to illustrate the approach and reasoning behind it.

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?

Response:
We have added a discussion of the context of the results to the discussion section.

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.

  Response: *We have tried to make the abstract more legible and would like to thank the reviewer for pointing this difficulty out to us.*

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

  Response: *Done.*

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

  Response: *Done.*

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

  Response: *Done.*

- 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?

Response: *This equation derives directly from the definition of the linear response function. As explained above it might be possible to derive such a function from first principles. However, for a realistic topography as applied here, we believe that this might be difficult. A simple function was found by Price et al. (PNAS, 2011) from numerical simulations. This simple form was then capture by Winkelmann & Levermann (Clim Dyn. 2013) in a functional form of a power law, but also there no simple derivation was provided. The only simple derivation that the authors are aware of is that for one-dimensional vertical diffusion which follows the reciprocal of a square root behavior. In complete agreement with the reviewer, we would very much appreciate such a derivation. Unfortunately, it is out of the scope of this paper to derive it.*

- 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?

  Response: *Thanks for the note. We have rearranged the figures in some cases and added references in others.*

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

  Response: *This relates to the explanation of the probabilistic approach which we have extended in section 4. We used an ensemble of 600 time series of the global mean temperature for each scenario. This ensemble derives from the climate emulator MAGICC 6.0 (Meinshausen et al) and was used extensively (including the IPCC AR5) to capture the possible future range of temperature evolution under different future scenarios. We have tried to make this clearer in the description of the procedure.*

- 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?

  Response: *Since the temporal evolution is important because of the memory of the system (as captured by the response functions) the approach taken here differs from just using all different temperature values within a time series.*
  The method applied is a simple Monte-Carlo method with independent choices of the global mean temperature time series, the oceanic scaling coefficient, the melting sensitivity and the ice sheet model (represented by the respective response function). We have followed the procedure with 10,000 "experiments" and then with 50,000 and there was no difference between the results which is a strong indicator for convergence of the procedure. To our knowledge the Monte-Carlo procedure should converge for large numbers of "experiments". Multiple occurrence of the same combination of values is allowed and should not jeopardize the procedure.

Reviewer #2

In this paper, the authors use five different Antarctic ice-sheet (AIS) models that participated in the SeaRISE intercomparison project to estimate ice-sheet response functions for four sectors of AIS. These response functions are constructed so that they can be convolved with melt rate to yield total ice sheet discharge over time. Under the assumption of (1) linear responses of subsurface marine temperatures to global mean temperature (using patterns in this case derived from the CMIP5 models), (2) linear responses of melt rates to subsurface marine temperatures, and (3) stationary ice-sheet response functions, these ice sheet response functions are combined with probabilistic global mean temperature projections to yield probabilistic projections of AIS melt over the 21st century. While each of the core assumptions introduce important limitations that, in some cases, should be highlighted more clearly, the approach taken is a very reasonable one, and I would recommend this paper for publication with minor revision.

Response:
We would like to thank the reviewer for this very positive assessment. In accordance with the reviewer's three points, we have added a paragraph to the introduction to make the constraints of the approach very clear.

The most important caveat, which I would highlight more clearly, is that the AIS models may be missing core physics that could affect projections under all scenarios. At times, the authors suggest that the assumptions of their linear projections are most likely to fail under the low-emission RCP 2.6 pathway and are more likely to hold under the high-emission RCP 8.5 pathway (e.g., 1128). However, one might note other work, such as the recent work of Rob DeConto on ice-cliff collapse and melt-enhanced calving suggesting that near-meter-scale AIS contributions are physically plausible under RCP 8.5. This physics is not included in any of the AIS models. Conversely, I believe the negative feedback on marine ice sheet melt provided by static equilibirum sealevel effect (Gomez et al., 2013, doi:10.1016/j.epsl.2013.09.042) is also not included in any of the models, with the possible exception of the Penn-State-3D model. I would accordingly suggest somewhat greater modesty in the presentation of the probabilistic projections.

Response:
This is a very important point and we have added explanations in this direction to the discussion section.

Minor substantive comments

- Page 1121: "The full uncertainty range of future climate change for each of the Representative Concentration Pathways (RCP, Moss et al., 2010; Meinshausen et al., 2011a) using the current simulations from the Coupled Model Intercomparison Projection, CMIP-5 (Taylor et al., 2012)" does not in fact describe what the authors do – a good thing, since the CMIP5 ensemble is an ensemble of opportunity, not a probabilistic distribution representing the full uncertainty range. In fact, the authors use MAGICC, a
  Response: *We thank the reviewer for spotting this mistake. It has been corrected.*

- Page 1131: Is Schewe et al. (2011) the right citation here? Schewe et al. (2011) appears to use MAGICC6 to emulate 19 AOGCMs, whereas here the 600 time series projected seem to use MAGICC as described in Rogejl et al. (2012, doi:10.1038/nclimate1385).

Response: *We very much appreciate the reviewer's scrutiny. This is really helpful. In this case however, the reference was correct. The ensemble was taken from the Schewe et al paper.*

- Page 1131: Is there a bias-correction applied to the CMIP5 model output before a scaling relationship is derived from them?
  Response: *There was no bias correction applied. The scaling is between warming anomalies of the surface air temperature and the subsurface ocean temperature. This was not properly described and has now been corrected. Again thank you for spotting this imprecise statement.*

- Page 1132: Clarify (if true) that the basal melt coefficients are drawn from a uniform distribution.
  Response: *They are being drawn from a uniform distribution which is now stated.*

- Page 1134: I assume the authors discard the misfit between their linear fits determined for the CMIP5 coupled climate models subsurface ocean temperature and global mean temperature projections. If this misfit were retained as an error term, would it significantly affect the results?
  Response: *That is a very relevant question. We provide the scaling coefficients for the different regions and models in tables 2-5 together with the r2 values. In most cases the r2 values are very high indicative of the very good fit especially when a time delay is taken into account. At the same time the coefficients vary between 0.07 (disregarding the 0) and 0.67 within each sector. Thus the uncertainty arising from the difference in models is larger than the scatter within a model. We use only the larger scatter between models and consider the scatter within the models a noise that the ice will integrate over.*

- Page 1136: "The long tail towards higher sea-level contributions makes the estimate of the 90%-range of the distribution (thin horizontal lines at the top of each panel) very difficult, because it is based on few extreme combinations which might not be robust." This is surprising to me given the 50,000 Monte Carlo draws – there should be 2,500 draws outside the 90% range, which I would think would make the range fairly robust. Have the authors checked the stability of these estimates?
  Response: *We agree with the reviewer. The tails are robust within our statistical analysis we have eliminated the corresponding sentence.*

- Page 1136: I would think the authors' approach would provide a useful method for estimating the covariance of melt between different ice sheets, providing a useful method to probabilistic projections methods such as those of Little et al. (2013, doi:10.1073/pnas.1214457110).
  Response: *Thank you for this suggestion. We have gotten in contact with Christopher Little and Michael Oppenheimer on this matter.*

- Page 1137: "The aim of this study is to estimate the full range of potential sea level rise caused by future ice-discharge from Antarctica." The paper does not do this, nor is it capable of doing this given the potential omission of important physics. See comment above, and page 1138, which states "estimates may not cover the full contribution from consecutive, potentially self-accelerating grounding line retreat which may be significant."
  Response: *We agree with the reviewer and have changed the sentence to read:*

"The aim of this study is to estimate the range of potential sea level rise caused by future ice-discharge from Antarctica that can be induced by ocean warming within the 21st century within the constraints of the models and the methodological approach." Furthermore we have added a discussion of the missing physics to the discussion section (also upon request of reviewer #3).

- Page 1137: "The largest uncertainty in the future sea-level contribution estimated in this study arises from the external forcing." Please clarify that the forcing uncertainty referred to here is not the range of global forcing as represented in the RCP, which has a second-order effect, but the uncertainty resulting from translating global temperature to melt rate.
  Response: *We agree and have changed the sentence to:*
  "However, the largest uncertainty in the future sea-level contribution estimated in this study arises from the uncertainty in the external forcing and here in particular from the uncertainty in the physical climate system, not in the socio-economic pathways."

- Page 1139: "This is particularly relevant for weak forcing scenarios in which an instability might be triggered but the directly forced ice loss is weak. For strong forcing scenarios like the RCP-8.5 the forcing is likely to dominate the dynamics." I don't think this point is established; see comment above.
  Response: *We agree with the reviewer that this is not established. Since we would however at least mention this possibility, we have reformulated the sentence as a hypothesis as follows:*
  "It is hypothesized that this is particularly relevant for weak forcing scenarios in which an instability might be triggered but the directly forced ice loss is weak. It might be less relevant for strong forcing scenarios like the RCP-8.5 when the forcing might dominate the dynamics."

Minor presentation comments

- Page 1121, line 28: "allows to" -> "allows us to"
  Response: *Done*

- Page 1122, line 9: "model's" -> "models"
  Response: *Done*

- Page 1122, line 11: It is unclear what the antecedent clause for "which is a possible response to enhanced ice flux and upstream thinning" is.
  Response: *This statement was not very helpful and was eliminated.*

- Page 1122, line 23: strike "of"
  Response: *Done.*

- Page 1135, line 13: "capable to simulate" -> "capable of simulating"
  Response: *Done.*

Reviewer #3

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 sub-shelf parameterization 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.

Response: *We agree with this assessment and have eliminated the notion of a scenario dependence from the abstract and conclusion section.*

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.

Response: *We had thought that we had already made clear that there are strong limitations to the ocean as well as to the climate models. We have now added explanations in this direction to the discussion and to section 2 which describes the models.*

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.

Response: *We agree with the reviewer that this was not properly formulated. We have reformulated the abstract, introduction and conclusion accordingly which led to a less strong statements while underlining the importance of the uncertainty in the boundary conditions.*

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?

Response: *We strongly disagree with the reviewer's implication that we through out the models that do not show the results we wanted. First of all, there are good reasons for not using models without ice shelves when the only forcing occurs through ice shelves. The introduction of basal ice-shelf melting into the models without ice shelves thus allowed for some arbitrariness. The AIF model solved this by applying melting at the current grounding line positions without* *accounting for possible changes in geometry. The UMISM model applied basal melting everywhere along the coast line, independent of the existence of an ice shelf. As a consequence the sea-level contribution of UMISM was significantly higher than that of the other models. Secondly, it is not clear what the reviewer means by "the results we want". The results from the AIF model lies within the interval that is spanned by the three shelf-models while the UMISM model shows a much stronger response than the others for the reason explained above. We have made it clear that UMISM is melting along the entire coast in the model description. It is immediately clear from the Figure 3 and explained in the discussion. We have tried to make this reasoning clearer in Section 4 and in the conclusion.*

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

Response: *We agree and have eliminated the sentence.*
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.

Response: *We have reordered the figures and eliminated the references to the figures 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.

Response: *Figure 5 shows the oceanic temperatures off-shore of the different drainage basins and Figure 7 the sea-level contribution from these basins. We cannot see how the respective figure captions could directly benefit from each other. We are grateful for any further instructions.*

Insets should be readable (not the case for Figure 5), not lead to confusion (figure 1 and 13)

Response: *We are sorry for the small numbers in the inlays of Fig. 5. This was a mistake which happened during the compilation and has now been fixed. With respect to Figures 2 and 13 (Figure 1 does not have an inlay), the inlays show the same functions as the main figure, but for a longer time period. We think this is understandable given the explanation in the figure captions. We would like to keep these figures if no strong objections are raised by the reviewers or the editor.*

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?

Response: *All models have started from equilibrium which is explained in detail in the corresponding SeaRISE publications but should be detailed here too. It has now been added to the model description section 2. As the reviewer know the models used here are rather coarse and thereby can run for several hundred thousand years with constant boundary conditions. The last century prior to the beginning of the experiments has no connection to the 20th century and can thereby not be compared to literature values. At present we have a rather specific description of each ice model in a rather standardized fashion. Each of these subsections is* *labeled according to the respective model. We would like to keep it this way and would rather not add another table with the model description because that would show very similar information in a very similar fashion as the current description and at the same time increase the volume of the paper.*

You constantly discuss pdfs whereas you mean frequency distributions.

Response: *We believe that to a wide range of readers of ESD the concept of pdfs are more easily understood than frequency distribution which would then have to be interpreted as a pdf. Since there is no stochastic theory anywhere in the paper, we do not believe that there is a danger of the reader misinterpreting our graphs to anything other than what they are. We would therefore like to keep the term pdf, if at all possible.*

- Page 1121 line 9 will likely to be replaced by are here assumed to
  Response: *Done*

- 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.
  Response: *We have added a statement on what is done in this paper: "Here we use linear response theory to project ice-discharge for varying basal melt scenarios."*

- Page 1122 line 23 remove of
  Response: *Done.*

- Page 1122 line 24 change only with a bed to whenever the bed is
  Response: *Done.*

- Page 1129 line 19 add importance after second order
  Response: *Done.*

- Page 1130 line 27 inform the reader immediately that derivation of the scaling coefficients follows.
  Response: *Done.*

- 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
  Response: *We have added: "*within the constraints of the applied methodology"

- Page 1137 line 6, address IPCC properly –
  Response: *Done.*

- 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.
  Response: *Feldmann et al. J Glac. 2014, have shown, that the grounding line motion in one of the coarse resolution models (PISM) qualitatively very similar to the Full-Stokes model Elmer, but that the grounding line motion is generally faster than that of the Full-Stokes model. We are thus not convinced that me can make a statement like suggested by the reviewer and would prefer to omit it.*