Comment on 'Exploration of Antarctic Ice Sheet 100-year contribution to sea level rise and associated model uncertainties using the ISSM framework'

presented on 1st of June 2018 by Schlegel et al.

In this manuscript, an uncertainty assessment of the 100-year contribution to sea-level rise from the Antarctic Ice Sheet is presented. Future sea level rise is a highly important topic of great interest for the scientific community and of large societal importance. The study by Schlegel et al. uses a statistical uncertainty quantification method that has been applied previously for regional setups (e.g., Larour et al., 2012a,b; Larour and Schlegel, 2016; Schlegel et al., 2013, 2015) and is now applied to an Antarctic-wide setup. The method is based on a number of forward model runs, which are performed with a resolution of ≤ 8 km in ice streams and ice shelves, solving the 'SSA' using the ISSM ice-sheet model.

The authors test for the effects of uncertainties related to surface accumulation, sub-shelf melting, ice viscosity as well as basal friction on mass loss from the ice sheet. Schlegel et al. specify 'perturbation bounds' (uniform or individually for each drainage basin) for each of these variables. Using Latin Hypercube Sampling, a statistically representative number of perturbations for each variable and each region ('partition') of the model domain is determined. The perturbations are applied as step-forcings, uniformly over the corresponding partition. Based on these experiments, a statistical distribution of sea-level relevant ice volume loss after 100 years of model time is derived and analysed.

In additional experiments, Schlegel et al. test for the role of the bed geometry measurements. Their findings highlight that the details of bed geometry are decisive for grounding line movement that determines mass loss. In further experiments, they also test for the effect of an instantaneous ice-shelf collapse (however ice shelves form in regions that unground during grounding line retreat).

The manuscript is well written and provides much detail on the model experiments and the results. The methodology presented is innovative and provides valuable insights into uncertainties of Antarctic mass loss. I have only a few major comments on the manuscript:

Major comments:

- **Sub-shelf melt rates** With your methodology, you find that sub-shelf melt rates are dominating the mass loss uncertainty. It would hence be great to have more information on the melt rate field you use, e.g., a figure showing a spatial map of the reference sub-shelf melt rates. How large is the spread of melt rates - within ice-shelves and in-between ice shelves? Are there regions with accretion? How do the melt rates compare to observations? How large are the melt rates near the grounding lines, which are then also applied in newly ungrounded regions? And how large are the maximum CDW values that you use to constrain the IBs for melting in the individual partitions?

- **UQ and partitions**
  Please add more details on the UQ method, its underlying assumptions, benefits and limitations, the way it is applied here and on the choice of partitions made.
  In previous applications of the uncertainty quantification (UQ) technique, a high number of partitions has been used ($\geq$ 500 for regional setups in Larour et al., 2012a,b; Larour and Schlegel, 2016; Schlegel et al., 2013, 2015). Since uniform perturbations are applied within each partition, increasing the number of partitions yields convergence of the resulting uncertainty distribution as long as the errors/uncertainties sampled within the individual partitions are independent, as far as I understand. But the uncertainties under future warming scenarios, e.g., of surface accumulation changes, are not spatially uncorrelated, and this is why you find a large dependency of you results on partitioning (Sect. 4.1). How does the application of the UQ in this study hence differ from the previous, regional applications (where a large number of partitions was used)? Please discuss this in the manuscript.
  Since the IB experiments based on partitions derived from drainage basins are expected to yield more realistic estimates, it would be highly interesting to understand the effect of partitioning in these experiments: if you do not split drainage basins in high surface elevation regions and low surface elevation regions, how does this affect your results (e.g. in the IBMeltOnly and IBAccumOnly experiments)? And how are the results affected by combining the partitions relating to the same ice shelf or region (e.g., 7 and 8 for FRIS, 10 and 11 for Ross or 1, 2 and 6 for the Antarctic Peninsula)?

Minor comments

- page 1, line 13: Maybe omit ‘instantaneous’ here, since you do not test for ice-shelf collapse during the model run and your statement could be read to say ‘it is not possible to achieve $>$ 1.2 meters with a collapse during the run’.

- page 1, line 14 and line 17, page 2, line 25,...: Using the term 'scenario' could be misleading. I did understand this first in the sense that you are making projections of Antarctica's future sea-level contribution, while you are, in fact, assessing uncertainties by using idealized, step-forcing experiments.
- page 1, line 15: Maybe combine the sentences to make clear that this estimate is linked to the UB experiment?
- page 2, line 6: Maybe add that 30cm was the upper bound in (Ritz et al., 2015) and that $>$ 1m was found for RCP8.5 by (DeConto and Pollard, 2016).
- page 3, line 18: What do you mean by 'coast' - the grounding line or the ice front? How large is the resolution around the grounding line (see also page 17, line 21). How is the resolution in areas where the grounding line retreats during the model run? Do you refine your mesh during grounding line retreat?
- page 3, line 5: What model parameters do you use, e.g., related to Glen's flow law or the Budd-type friction law?
- page 3, line 29: Why do you invert for ice viscosity in the floating areas instead of applying the same procedure as for the grounded areas (assuming thermal steady state)? Does this introduce jumps in ice viscosity across the grounding line?
- page 4, line 21: Are the exchange parameters optimized per shelf or per region or continent-wide?
- page 4, line 32: Please clarify the 'large melt multipliers in the ice sheet interior'.
- page 5, line 5: Are there regions where the grounding line advances? How do you specify basal friction in these areas?
- page 5, line 19: This is a very interesting and important assessment. I wonder if (just for curiosity, if you did not do this, I do not ask you do to so): did you also compare to the original Bedmap2?
- Section 2.4: Please give more details on the underlying assumptions, capabilities and limitations of the UQ method.
- Section 2.4: In this section you relate to 'errors' in the model boundary conditions (e.g., lines 21, 24, 28). This might be confusing the reader since you quantify the effect of uncertainties in the variables rather than errors.
- page 5, line 23: Reading your manuscript, I did understand that you use Latin-Hypercube sampling and not Monte-Carlo sampling (also stated in the abstract)?

- page 5, line 27: You state that you assume a uniform distribution of uncertainties to sample the tested variables (sub-shelf melt rates, surface accumulation, ice viscosity and basal friction). This sounds reasonable given that we do not know how future greenhouse gas emissions evolve or which RCP scenario is most likely and how boundary conditions change in the future. Could you elaborate more on this in the manuscript: why did you chose this type of distribution and how does this affect your results, e.g., in comparison to using normal distributions?
- page 5, line 31: Is there a specific reason that partitions should have equal areas? And how does it affect the IB-results that the GP do not have equal areas?
- page 6, line 3: Please explain.
- page 6, line 11: Could you solve the problem of how to define reasonable partitions by specifying forcing scenarios for the variables (where possible, e.g. for the surface accumulation based on RCP scenario projections) and by using the UQ method to quantify uncertainties imposed on these?
- page 7, line 24: Can you give an estimate to what amount of surface warming this extreme scenario corresponds?
- page 9, line 5: How large do melt rates of FRIS and in the Amundsen sea get?
- page 9, line 15-20: You could state here, that these bounds also encompass all RCP projections (if this is correct). This would underline that you are not only interested in the strongest emission scenario, but that you quantify uncertainties encompassing all scenarios.
- page 10, line 17: Is it valid to assume that variables can be sampled independently from each other and that partitions can be sampled independently? Please add a discussion.
- page 13, line 7: mass ‘gain’ instead of ‘loss’ .
- page 13, line 31: The bi-modality is very interesting - do you have further hints on how this arises? Is it linked to instability within a specific basin or ice stream?
- page 14, line 26: How large are melt rates around the grounding line in your control run?
- page 14, line 29: How does the mean SLE from the Amundsen Sea Region for the IB experiment compare to observed rates of sea-level rise (e.g. Shepherd et al., 2018)?
- page 14, line 35: The intrusion of relatively warm water into the Filchner-Ronne ice shelf cavity has been found possible within the second half of the 21st century for the RCP8.5 scenario (Hellmer et al., 2012). This would be much later than in the step forcing experiments applied here - how is this reflected within your uncertainty quantification method?

- page 15, line 3: Could you add a figure with an example for grounding line migration?
- page 15, line 16: Maybe reformulate this to 'subsequent reduction in buttressing'. The current formulation might cause misunderstanding, since you account for the reduction of buttressing through changes in sub-shelf melting.
- page 17, line 8: Please explain what you mean with 'well-behaved' and 'normally'? Does this relate to the next sentence about the bimodal structure?
- page 17, line 15: E.g., shorten to '... bedrock geometry and ice-shelf buttressing, which determine grounding line stability.'
- page 18, line 10: Why does your reasoning presented here for the UB experiments (lower resolution provides less bed geometry details and this yields more SLE) not apply to the IB experiments?
- page 19, line 14: Another process missing might be isostatic rebound, as recent evidence suggests (Barletta et al., 2018).
- Appendix A, page 21, line 31: This setup was created as an idealized version of the cavity underneath Pine Island Glacier ice shelf. Did you test also for cavity sizes and shapes comparable to FRIS?
- Appendix A, page 22, line 5: 'average ice shelf melt rate'?
- Appendix A, page 22, line 23: Please clarify 'that does not artifically restrict ice flow'.
- Table 2: Please specify 'mean max melting rate'.
- Figure S1: The upper panel looks squeezed in y-direction. The line colors to indicate the sectors in the lower panels are hard to see.
- Figure S5: It is hard to see the grounding line positions, especially the initial position.

Technical issues

- page 4, line 1: remove 'are' before 'described'.
- page 10, line 7: 'actual uncertainty'
- page 17, line 10: There is a verb missing in '.., as evidence by..'.
- page 20, line 12: This sentence is doubled.
- page 21, line 29: 'setup'

References

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