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Interactive comment on "Uncertainty quantification of the multi-centennial response of the Antarctic Ice Sheet to climate change" *by* Kevin Bulthuis et al.

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***Response to the Interactive comment on* "Uncertainty quantification of the multi-centennial response of the Antarctic Ice Sheet to climate change" *by* Kevin Bulthuis et al.**

We would like to thank Andy Aschwanden for the time dedicated to this manuscript and his constructive comments to improve the general quality and readibility of the manuscript. We will try to give a proper response to his comments and revise the manuscript accordingly. For each referee's comment (written in blue), we included

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below a response (written in black) and proposed means to improve the manuscript.

1 Summary statement

This manuscript uses a framework comprising an ice sheet model, an emulator, and uncertainty quantification methods to assess the response of the Antarctic Ice Sheet to climate change. The paper is generally well written and boasts beautiful figures. Its strength lies in the comprehensive probabilistic approach, with a thoughtful use various uncertainty quantification methods. While I remain suspicious of the applicability of emulators, as they could miss discontinuities and strong non-linearities, I must also admit that I am not sufficiently familiar with the topic to have an informed opinion.

Best wishes,

Andy Aschwanden

We would like to thank Andy Aschwanden for this kind summary statement. The referee is right in pointing out that discontinuities and nonlinearities could pose challenges to the construction of emulators. However, we believe that our approach is valid when considering the global response of the Antarctic ice sheet (AIS) (e.g. ΔGMSL) as we may expect possible local and regional strong non-linearities and discontinuities to be smoothed out at the continental scale. Our belief is further backed by other studies of the continental response of the Antarctic ice sheet (Ritz et al., 2015; Golledge et al., 2015, 2017; DeConto and Pollard, 2016; Schlegel et al., 2018) that also suggest that this response does not exhibit strong non-linearities and discontinuities. Conversely, we did not compute the confidence regions with an emulator as the applicability of an emulator to the local/regional behaviour of the AIS is more questionable and we used directly the training points and forward simulations to estimate them. We leave for a further work the investigation of emulators for estimating confidence regions (Bulthuis et al., in preparation). We have added a sentence in the manuscript to clarify the use of an emulator to represent the continental response of the Antarctic ice sheet, as measured through ΔGMSL.

2 Major comments

I am a bit puzzled by the model setup. With reported computational costs of 16 CPU hours per simulation, one wonders if an emulator is indeed need as on the order of thousands simulations are computationally tractable? As discussed in the manuscript, the horizontal resolution of the ice sheet model is coarse, and topographic details will be missed. A demonstration of convergence under grid refinement would provide some confidence that the ice sheet model simulations are indeed robust. Nonetheless, one should not hold this too much against the manuscript, because, as I wrote above, the strength is clearly in the uncertainty quantification. Main-effect Sobel indices could become a standard tool in ice sheet modeling. Aschwanden et al (under review) use a similar approach for Greenland.

We agree with the referee that a resolution of 20 km may be not able to capture properly certain mechanisms that control grounding-line migration such as bedrock irregularities and ice-shelf pinning points as well as important small glaciers such as Pine Island and Thwaites glaciers. We refer the referee to our response to anonymous referee # 1 regarding the limitations of this approach and the changes we brought at the manuscript to discuss the applicability and limitations of this approach (https://doi.org/10.5194/tc-2018-220-AC1).

The referee is also right in pointing out that our ice-sheet numerical model would allow to perform thousands of simulations, thus questioning the (full) need for an emulator. Yet, this approach would be feasible only for a few configurations of the model. In our manuscript, we investigated a set of 20 distinct configurations given by each combination of RCP scenario with a sliding law ($m = 1, 2, 3$ or 5) and each combination of RCP scenario with the TGL parameterisation in an attempt to investigate a sufficiently broad ensemble of model configurations. For each of the configurations, we ran a set of 500 forward simulations (training set) to build a PC emulator as performing thousands of simulations for each of the configurations would have been less computationally tractable. In addition, we had two definite goals in writing down this manuscript such as (1) providing the reader with a comprehensive probabilistic approach and various uncertainty quantification methods for uncertainty analysis in ice-sheet models (as highlighted by the referee) and (2) investigating a sufficiently broad ensemble of uncertainties to give a large insight into the impact of uncertainties in ice-sheet models.

Following the referee's suggestion, we have added a new figure (Fig. S1 in the supplementary material or Fig. 1 in the response) in the supplementary material that provides a comparison for the AIS contribution to sea level as a function of spatial resolution (20 km in Fig. 1a vs 16 km in Fig. 1b) to give an idea about the impact of the model resolution on our results. This figure suggests that the uncertainty in the projections due to the model resolution is (far) less important than the uncertainty due to the uncertainty in the parameters.

Why this confusing setup? p.10, l.26–26: "All results to follow have been obtained with the SGL parameterisation for the grounding-line migration, except in a discussion at the end of the section, where we discuss the impact of using the TGL parameterisation." Wouldn't it be easier to discuss the experimental design and results if the choice of grounding line parameterization were an additional parameters in the uncertainty quantification (i.e. by prescribing a uniform probability density function).

We agree with the referee that this sentence sounds a bit awkward and does not bring any additional information to the reader, so we have removed it from the manuscript. In addition, we make it clear in Sect. 2.2.3 that most results were obtained with Schoof's grounding-line parameterisation (reference parameterisation) and Tsai's grounding-line parameterisation is only discussed in Sect. 3.8. The choice of the grounding-line parameterisation can be seen as an additional parameter, but as this parameter is categorical (either Schoof's grounding-line parameterisation or Tsai's grounding-line parameterisation) it cannot be prescribed with a probability density function. We could have assigned a weight to the choice of the grounding-line parameterisation, but we have no evidence for a suitable choice of weights.

3 Minor comments

I find the introduction, discussion and use of "Uncertainty Community" somewhat awkward. While I think I understand what the authors try to express (and I like this approach), some clarification is warranted. What is the "recently formed uncertainty quantification community"? Maybe there is a white paper or similar that could be cited? UQ has been an integral part in numerical modeling outside glaciology but is only now making its way in ice sheet modeling (well, better late than never).

We agree that our terminology might sound a bit awkward, especially for most readers of the Cryosphere journal who are not familiar with uncertainty analysis. Uncertainty analysis has been a long standing part of numerical/experimental studies in science and engineering, but the coherent formalisation of uncertainty analysis in a probabilistic framework is rather recent and is still an ongoing work. The phrase "Uncertainty

Community" refers to this community that aims at formalising uncertainty quantification analysis. In this manuscript, we have tried to bridge the gap between UQ outside of glaciology and glaciology. To avoid any ambiguity, we have decided to remove the phrase "Uncertainty Community" from the manuscript and talked more generally about UQ analysis rather than referring to the "Uncertainty Community".

4 Detailed comments

- Everywhere: say "the contribution to sea-level is..." instead of "the contribution to sea- level rise". "Rise" is not needed here.

We have followed the referee's suggestion and changed the phrase "the contribution to sea-level rise" by "the contribution to sea level" all along the manuscript.

- p.1 l.2–3: . . . remain challenging due to . . .

The words "to establish" have been removed from the manuscript.

- p.1 l.10: sources of uncertainty, except bedrock relaxation time, contribute . . .

We have changed the sentence in the manuscript based on the referee's suggestion.

- p.1 l.12: "as the scenario gets warmer" sounds awkward. Maybe as "temperatures rise"?

We agree with the referee that this phrase sounds a bit awkward. We have replaced it by "as atmospheric and oceanic temperatures rise" following the referee's suggestion.

- p.2 l.10–11: . . . , with differences and uncertainty ranges of several meters of eustatic sea level.

We have changed the sentence in the manuscript based on the referee's suggestion.

- p.2 l.24: . . . initial state, climate forcing, and parameters . . .

We thank the referee for this suggestion. Yet, we tried not to use the Oxford (serial) comma all along the manuscript unless necessary to remain consistent in the manuscript (as suggested in the manuscript preparation guidelines for authors in the Cryosphere jornal).

- p.3 l.2: . . . based on probability theory . . . .

We have changed the phrase in the manuscript based on the referee's suggestion.

- p.6 Table 1 and parameters: Rephrase "Uncertain parameters". Maybe "List of parameters and parameter ranges used in the uncertainty analysis" or similar. Also, it is not clear what distributions are used. Maybe the use of a "nominal" parameter suggests a Gaussian distribution. Or do all values in the parameter interval have equal probability? Note: I found that is is later explained on page 8, lines 24–31. Please add to the table legend that all parameters are drawn with equal probability. I am not sure that this is a good assumption though. Do you have any prior information that supports this?

We have rephrased "Uncertain parameters" as "List of parameters and parameter ranges used in the uncertainty analysis" following the referee's suggestion (The inappropriateness of the phrase "uncertain parameters" was also pointed out by anonymous referee #1 (https://doi.org/10.5194/tc-2018-220-RC1)). We use the term "nominal value" to refer to the accepted/reference value of a parameter that we would have used in the absence of an uncertainty analysis, without any reference to an underlying probability distribution for the parameters.

In addition, we did not specify the probability distribution for the parameters in Table 1 as we only intended to discuss the sources of uncertainty in Sect. 2.2 and discuss the uncertainty quantification framework we used in Sect. 2.3 (notably the probabilistic characterisation of the parameters is given in Sec. 3.2.1).

We agree that assuming uniform probability distributions for the parameters may not be an appropriate assumption but as discussed in Sec. 3.2.1, a proper characterisation of the probability distributions of the parameters with expert assessment, data and statistical methods is beyond the scope on this paper. While a more refined uncertainty characterisation could be carried out, we used a uniform probability distribution for the parameters as this distribution is known to satisfy the principle of indifference (a.k.a the principle of maximum entropy) in the absence of any prior information about the distribution of the parameters except for their minimum and maximum values (Kapur, 1989), which we assume here for the sake of simplicity. Here, we determined the minimum and maximum values for the parameters based on expert assessment and literature.

- p.7 l.15: Please explain what F_{calv} is. It appears to be a scalar multiplier of something (a calving rate, a stress condition)?

The referee is right in his interpretation of F_{calv}. It is a scalar multiplier factor of the calving rate. Based on the referee's comment, we have changed Sect. 2.2.4 to give more details about the parameterisation of the calving rate in our model and the actual meaning of F_{calv} (scalar multiplier factor of the calving rate).

- p.8 l.29: "We limit the probabilistic characterisation to assuming uniform probability density functions and we do not address how this probabilistic characterisation could be refined by using expert assessment, data and statistical methods such as Bayesian inference." OK, that's fine. For the exponent of the sliding law, one could perform a calibration and compare simulated and observed surface speeds to assess how well a certain exponent is able to explain the observations. See Aschwanden, Fahnestock, and Truffer (2016). This then be used as a prior for describing a PDF (done in Aschwanden et al, under review).

We thank the referee for his thoughtful comment. We have added additional references (Aschwanden et al., 2016; Gillet-Chaulet et al., 2016) to clarify the fact that the probabilistic characterisation of the sliding exponent can be based on a calibration of the sliding exponent based on a comparison between simulated and observed surface velocities.

- p.9 l.32: I'm afraid I do not follow here. Please detail how many (and for which parameter configurations) the ice sheet model was run, and how the emulator was used to fill in the space (is the 500 the number of ice sheet model or emulator runs).

This paragraph clearly lacks some explanations as already pointed out by referee #1 (https://doi.org/10.5194/tc-2018-220-RC1). In our experimental set-up, we consider 20 distinct model configurations given by each combination of RCP scenario with a sliding law ($m = 1, 2, 3$ or 5) and each combination of RCP scenario with the TGL parameterisation. An emulator is built for each of these model configurations from an ensemble of 500 training points (hence 500 forward simulations) in the parameter space of the parameters F_{calv}, F_{melt}, E_{shelf}, τ_{e} and τ_{w} with a maximin Latin hypercube sampling design. In total, we carried out 10000 forward simulations of the f.ETISh model for the 20 model configurations. More details about the construction of the PC expansion are given in Appendix A.

We have changed the paragraph and given more details about the construction of the PC expansion. See also response to anonymous referee #1 (https://doi.org/10.5194/tc-2018-220-AC1) for more details on the experimental set-up.

- p.11 l.3–6: Change "Under nominal conditions, we find (Table 2) in RCP 2.6 a nominal AIS contribution to sea-level rise of 0.02 m by 2100, 0.07 m by 2300 and 0.20 m by 3000 and in RCP 8.5 a nominal AIS contribution to sea-level rise of 0.05 m by 2100, 0.59 m by 2300 and 3.9 m by 3000." to "Under nominal conditions, we find (Table 2) for RCP 2.6 an AIS contribution to sea-level rise of 0.02 m by 2100, 0.07 m by 2300 and 0.20 m by 3000 and for RCP 8.5 an AIS contribution to sea-level rise of 0.05 m by 2100, 0.59 m by 2300 and 3.9 m by 3000."

The sentence has been changed based on the referee's suggestion (see answer about p.11 l.19 for the use of "in RCP" instead of "for RCP").

- p.11 l.19 (and elsewhere): I'm not a native English speaker, but I think "Figure 4a–c shows that in RCP 2.6" should read "Figure 4a–c shows that for RCP 2.6" or "Figure 4a–c shows that under RCP 2.6".

We thank the referee for this comment. None of the authors is also a native English speaker. Throughout the manuscript, we tried to be consistent and use the same preposition "in" every time we refer to a RCP scenario ("in RCP 2.6" stands for an ellipsis for "in the scenario RCP 2.6"). We did not find a general consensus in the literature regarding the use of a specific preposition when referring to the RCP scenarios (Golledge et al., 2015; De Conto and Pollard, 2016).

- p.11 l.23: "This quadratic dependence can be explained by the influence of E_{shelf} on two competing processes: a higher value of E_{shelf} softens the ice, thus leading to faster ice flow in the ice shelves; but a higher value of E_{shelf} also leads to ice-shelf thinning, thus reducing grounding line ice flux."

The sentence has been changed based on the referee's suggestion.

- p.12 l.18: you use "median" but provide a range. Please clarify. Do the ranges represent the 16/84th percentiles, for example?

This range is simply the range of values taken by the median values for the three sliding exponents $m = 1, 2, 3$ as shown in Table 3. A formulation such that "a median AIS contribution to sea-level rise of 0.07–0.13 m in RCP 2.6 by 2300" takes into account the median value 0.07 m for $m = 1$, 0.13 m for $m = 2$ and 0.09 m for $m = 3$. We have changed our sentence following the referee's comment to avoid any ambiguity.

- p.12 l.22-23: ". . . except for certain cases . . . "

We have removed the word "a" from the sentence.

- p.12 l.26-27: ". . . and starts to increase around 2100 . . . "

We have changed the phrase based on the referee's suggestion.

- p.12 l.29: maybe reference Fig. 6 here?

We agree with this suggestion. We have added the suggested reference at the end of the sentence.

- p.12 l.31: "...which contributes 3–3.5 metres to sea-level followed by a slower retreat of the East Antarctic ice sheet."

We have changed the phrase following the referee's suggestion.

- p.12 l.33: "by the year 3000"

We have changed the phrase following the referee's suggestion.

- p.13 l.1: "For RCP 2.6, we find (Table 3) 5–95 % probability intervals ..."

We have changed the sentence following the referee's suggestion (see also answer about p.11 l.19 for the use of "in RCP" instead of "for RCP").

- p.13 l.4: "...an increase in sea-level, though a decrease cannot be ruled out for a viscous sliding law and cooler atmospheric conditions."

We have simplified the sentence based on the referee's suggestion.

- p.14 l.6: It is interesting that for RCP 2.6 rheology contributes a similar amount 40–60

We agree with this comment. Maybe this suggests the need to properly calibrate E_{shelf} alongside the sliding coefficient in the initialisation procedure.

- p.15 l.23: "ice is certain to . . . " I would refrain from using strong words like "certain".

We agree that using strong words like "certain" is probably not relevant and could lead to misinterpretations. Hence, we have removed the word "certain" as suggested by the referee.

- p.17 l.4–10: Rather than writing down numbers (which are listed in Table 4 anyway), I suggest to tell the reader how the plastic sliding law compares to the intermediate case since this is what one cares about.

We agree with the referee that all these numbers do not bring additional information to the manuscript (as they are listed in Table 4). We have removed all these numbers and written down the paragraph as a discussion about the difference with the other sliding laws.

- p.17 l.18: ". . . , which could explain the smaller ice loss in our results under $m = 5$ than $m = 3$." That's interesting, I would not have guessed.

We thank the referee for this comment.

- p.19 l.11: "the pivotal role played by atmospheric forcing". I think you mean the role of the emission scenario.

The referee is right in his interpretation. We have changed our phrase according to his suggestion to make it clearer.

- p.19 l.26: Moreover, the lower sensitivity of the Amundsen Sea sector in our simulations may arise. . .

We have changed the phrase "in our findings" by "in our simulations" as suggested by the referee.

- p.20 l.6–10: Very long sentence, maybe split into two. Figures

We agree with this suggestion. The sentence has been split accordingly.

- p.20 l.23–29: I understand what you are trying to say but I'm not sure the formulation "does (not) question the nominal projections". Maybe "in agreement with" or similar? The probabilistic framework expands upon the nominal projections, and your results provide good evidence that a thorough risk assessment must include UQ.

The referee is right in his interpretation of this sentence. We agree with his suggestion to change the formulation of the sentence. We have changed the sentence as ". . . accommodating parametric uncertainty in the ice-sheet model leads to projections in agreement with the nominal projections of limited ice loss . . . "

- p.21 l.20: We found that all investigated sources . . .

We agree with this suggestion. The sentence has been changed accordingly.

- Fig. 3 b–e: Show outline of present-day grounded area for better visual comparison.

We have added the present-day grounding-line position as suggested by the referee.

- Fig. 9: What does the m in the lower left corners of the plot mean?

The parameter m is the sliding exponent in Weertman's sliding law. We have replaced the phrase "under different sliding laws" by "for different values of the sliding exponent m in Weertman's sliding law" to remind the reader about the meaning of m.

5 References

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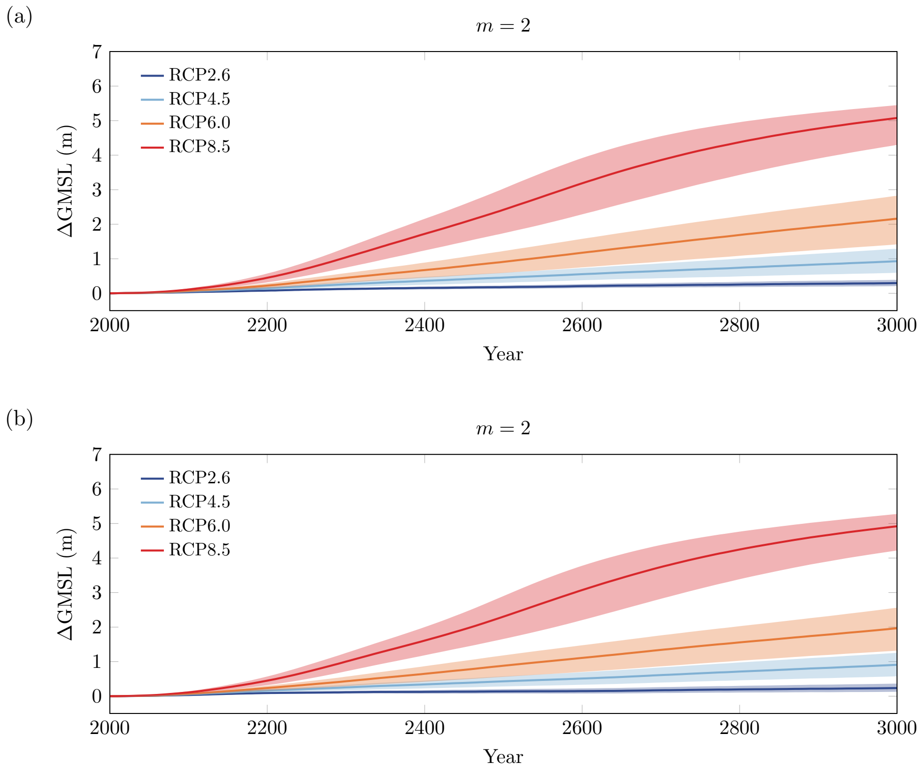

Fig. 1.

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