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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.

Anonymous Referee #1

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1 Summary statement

The manuscript "Uncertainty quantification of the multi-centennial response of the Antarctic Ice Sheet to climate change" by K. Bulthuis et al. provides an assessment of the response of the Antarctic ice sheet and the associated uncertainty to several climate change scenarios over the next few centuries, by combining numerical ice sheet model simulations and probabilistic methods. Using new probabilistic methods, and especially emulators, starts to be used in glaciology and provides an interesting alternative to costly ice flow models. The results show the relative impact of uncertainties in different ice flow model parameters as well as external parameters. They also suggest

that the marine ice sheet instability triggers large contribution to sea level for some combinations of parameters for the RCP 4.5 and 6.0 scenarios, while the instability is almost never triggered for the RCP 2.0 scenario and always triggered for RCP 8.5 scenario.

The paper is usually well written and the figures appropriate, but some references and background material are sometimes missing, especially in the introduction and the model references. I am also surprised by how the problems of the relatively low model resolution and simple parameterization of the grounding line are treated and not discussed in more details. Though it makes sense to use such configurations for large ensemble simulations of the entire Antarctic ice sheet, it seems a bit surprising that using such a configuration is needed when an emulator is used; I though that the emulator was allowing to run fewer but more accurate simulations used only for the calibration and then run a large number of cheap emulated runs to analyze the uncertainties in detail. This should at least be better acknowledged, especially in the limitations.

2 Major comments

The manuscript suggests that using a 20 km model resolution and a flux condition at the grounding line are reasonable assumptions and that the impact on the results is limited. I think this is a bit misleading as previous intercomparison experiments (Pattyn et al., 2012; 2013) have rather different conclusions. So this should be at least better explained and acknowledged in the limitations. Furthermore, I thought that the goal of using emulators was to limit the number of runs needed, and therefore that it would allow using more computationally expensive runs to calibrate the emulator, which does not seem to be the case here.

Similarly, basal melting under ice shelves is known to have a very large impact on ice dynamics over both short and long timescales. What is done for future scenarios regarding this melt is however not clear and should be explained in more details. Also, the ocean warming follows a simple parameterization that mostly depends on atmospheric warming. We know that the future evolution of the Southern Ocean remains very uncertain and that changes in ocean circulation rather than ocean warming are expected to cause the largest changes, so that simple parameterizations are unlikely to accurately represent these changes. This should be more clearly acknowledged and it may be good to provide some perspectives on the unceratintes associated to the future ice shelf melt.

This manuscript is submitted to a cryosphere journal, so many readers (myself included) don't have a very strong background in probabilistic methods, so it would be great to detail terms that are not common. Also, it is not clear how the emulator is calibrated (with how many runs, how are these runs chosen, etc.) and why it needs to be calibrated separately for each RCP scenario. I naively thought that the goal was to calibrate over a wide range of climate conditions, and then be able to investigate different scenarios easily, so without the addition of new physical simulations.

References and background material are sometimes missing, especially in the introduction and in the description of the changes made to the model, so it would be good to add some references and to provide more context.

3 Specific comments

p.1 l.10: "except perhaps the relaxation times": Does it contribute to the uncertainty or not? It would be good to put clearer conclusions in the abstract, or to remove this part.

p.1 l.12: maybe quantify how more dominant it becomes with the different warming scenarios.

p.1 l.20: references missing

p.3 l.19: It would be good to define "confidence regions" and add some references.

p.3 l.26: "ice-sheet models; yet, whereas" → "ice-sheet models: whereas ..."

p.4 l.6: Add references for the SIA and SSA approximations.

p.4 l.11-12: I think it is little biased to simply say that using a flux-condition at the grounding line is an appropriate solution. There has been some recent research (Reese et al., 2018) demonstrating that this flux condition does not accurately represent confined ice shelves, which is the case for most ice shelves around Antarctica.

p.4 l.17: "the implementation of an ocean model": is there an ocean model implemented in f.ETISh or a representation of the ocean conditions? I think this part needs to be rephrased.

p.4 l.18: How is the positive degree day model changed? Also add references.

p.4 l.24: The resolution of the model is 20 km, which can be understood for such computations. However, using the argument that the model is "essentially scale independent" is surprising, especially as Frank Pattyn, who is a co-author on this paper, published quite different conclusions following the MISMIP experiments (Pattynet al., 2012; 2013). Furthermore 20 km represents only a few points for important ice shelves such as the Pine Island or Thwaites ice shelves, so it is not clear how accurately these glaciers can be modeled with such a resolution.

p.5 l.5: "linked to the atmospheric forcing": How? What is the link used? A few words should be added to briefly describe this or at least mention that it will be detailed later on.

p.5 l.16: What is the impact of this correction? Can we consider the response to be relatively linear to subtract the reference runs? This needs a bit of clarification.

p.5 l.20: How about ice shelf basal melt, which is a dominant driver of the response of the Antarctic ice sheet?

p.6 Table 1: Caption: I don't think using "uncertain" is appropriate given that many other parameters are also uncertain. Consider changing. Also why use m=2 for the nominal value? I had the impression that the standard value is usually m=3.

p.6 l.2: I don't agree that the atmospheric forcing can cause large changes on the dynamics of the Antarctic ice sheet. It has a large impact on its volume and therefore sea level rise, but the impact on the dynamics is rather limited, unlike what is observed with changes in oceanic forcing.

p.7 l.18: it would be interesting to reference the buttressing capabilities of ice shelves around Antarctica (Furst et al., 2016).

p.7 l.28-30: It would be good to explain briefly the link between atmospheric and ocean warming (warming of the ocean surface, at depth, ...), especially as changes in ocean circulation rather than ocean warming are expected to happen around Antarctica.

p.9 l.32: Why is it necessary to build a different PC expansion for each RCP scenario? I would have imagined that the climate forcing was just a parameter that could be varied, similar to the other ones.

p.9 l.32: I don't really understand how the emulator is calibrated. Are the 500 forward simulations the training set or the results of the emulator? How many runs are used to calibrate the emulator and how are they chosen? It would be good to add some details in this section.

p.10 l.14: "subregions" → "regions" or "areas"

p.11 l.25-26: "making ice flux at the grounding line less important" → "reducing ice flux at the grounding line"

p.12 l.4-8: How different is it in this case? How significant are the changes for these additional simulations? It would be good to add numbers here, as the results are not

showed.

p.12 l.9: remove "significantly"

p.12 l.12: How about the order of magnitude difference in Fig.5 for example for several parameters? Where does this difference at different times come from? It would be good to discuss this.

p.13 l.5: Why would it be cooler given the values showed on Fig.2?

p.13 l.7: Is it possible to separate the impact of the ocean and the atmosphere?

p.13 l.16: Maybe add that this is because the forcing is so much larger for the high emission scenarios that the uncertainty caused by model parameters (considered similar in all the cases in your simulations) is therefore automatically reduced.

p.13 l.26: "sliding conditions in RCP 2.6" → "sliding conditions. In RCP 2.6"

p.15 l.22: "both regions": what regions are you talking about?

p.16 l.16: What is the distribution in these intervals? Is it always a uniform distribution?

p.16 l.21: I don't understand why it would be different.

Table 4 and Table 5 captions: maybe re-write what are the probability intervals.

p.19 l.11-12: I don't think the results presented show that the atmospheric forcing has a large impact on the ice sheet dynamics, at least for 2100.

p.19 l.21-22: I think this demonstrated mostly that the results are significantly impacted by the calibration of the basal melt forcing.

p.20 l.3: "the contribution of marine drainage basins": I don't understand what you mean here.

p.21 l.4-5: Add some references here, this is an important point.

p.21 l.14: What is the impact of this choice? How different would the results be with a different choice for the construction of the emulator?

p.21 l.21: "perhaps": Does it impact it or not?

p.24 l.8: What is g_I?

Fig.1 caption: "neglected": Is it neglected because it is negligible or because the melt parameterization used does not allow to compute refreezing?

Fig.3: It would be good to add the ice shelves on panels b to e.

Fig.4 and Fig.5: Is it possible to use the same axis for the three columns so that the comparison is easier?

Fig.5: What causes the change of more than 1 order of magnitude at the different times for most of the parameters?

4 References

Furst, J., G. Durand, F. Gillet-Chaulet, T. Tavard, M. Rankl, M. Braun, and O. Gagliardini, The safety band of Antarctic ice shelves, Nat. Clim. Change, doi:10.1038/NCLIMATE2912, 2016.

Pattyn, F., et al., Results of the Marine Ice Sheet Model Intercomparison Project, MISMIP, Cryosphere, 6(3), 573–588, doi:10.5194/tc-6-573-2012, 2012.

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