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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 anonymous referee #1 for the time dedicated to this manuscript and his/her constructive comments to improve the general quality and readibility of the manuscript. We will try to give a proper response to his/her comments. The manuscript has been revised to include more references, background material and discussions

about the description and the limitations of the model and the methodology. For each referee's comment (written in blue), we included below a response (written in black) and proposed means to improve the manuscript.

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.

We agree with this general comment and we have made changes to the manuscript to account for this comment. See answers below to major comments and specific comments to questions about p.4 l.11-12, p.4 l.24 and p.9 l.32.

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.

The referee is right in pointing out that the model resolution and the flux condition should be discussed in more detail given that they represent two major limitations of the simulations. In our opinion, using a 20-km model resolution and a flux condition at the grounding line are acceptable assumptions in large-scale and long-term ice-sheet simulations and large-ensemble simulations (see discussion below).

We are aware of the limitations of the flux condition (short transients, buttressing) but we expect the impact of this parameterisation to be limited when compared to the impact of uncertainties in model parameters and future scenarios. Furthermore, Pattyn et al. (2012, 2013) have shown that the use of a flux condition at the grounding line allows to reproduce qualitatively the marine ice-sheet instability (MISI) and simulations of the Antarctic ice sheet using a flux condition (Pollard and DeConto, 2012; DeConto and Pollard, 2016) have also been able to reproduce qualitatively MISI in large-scale ice-sheet simulations. In addition, it is true that a higher resolution would allow 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, which represent only a few grid points with a 20-km resolution. Yet, Pattyn (2017) has shown that using the f.ETISh model with higher spatial resolutions gives similar results for given perturbations. We have modified Sect. 2.1 to discuss in more detail the applicability and limitations of using a flux condition at the grounding line (see also answer about p.4 l.11–12 in the specific comments).

The referee is also right in pointing out that using an emulator allows to limit the number of computationally expensive runs. Yet, in this manuscript, we are interested in performing an uncertainty quantification analysis of the model under different sources of uncertainty. A typical uncertainty quantification analysis with a Monte Carlo approach (or related approach such as Latin hypercube sampling) requires in general tenths/hundreds of thousands to millions of runs to achieve proper estimations for the statistical descriptors (mean, variance, . . .) of the output. This is intractable even with a 20-km model resolution (with a computational cost of ~ 8h per forward simulation). In addition, we tried to investigate a sufficiently broad ensemble of uncertainties with an ensemble of five parameters and an ensemble of 20 distinct model configurations (4 RCP scenarios with 4 sliding laws and Tsai's flux condition). Each of these 20 distinct model configurations requires the construction of a different emulator because these model configurations represent discrete/categorical variables. For each of these configurations, we ran a set of 500 forward simulations (training set) to build the PC emulator, that is, we had to perform a total of 10000 forward simulations for the 20 model configurations. This is still a large total computational time (given our computational ressources) even with a 20-km model resolution. The emulator does not replace the forward ice-sheet numerical model, which is necessary to capture all possible nonlinear effects of the ice-sheet response. The choice of a 20-km model resolution is a tradeoff between the total computational time and the number of distinct configurations and uncertain parameters investigated in this paper. We clarify the choice for this resolution in the manuscript and its limitations (notably the inability to represent accurately important ice streams such as Pine Island and Thwaites glaciers).

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 parameterisation 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 parameterisations are unlikely to accurately represent these changes. This should be more clearly acknowledged and it may be good to provide some perspectives on the uncertainties associated to the future ice shelf melt.

This suggestion is totally relevant given the very large impact of sub-shelf melting on ice dynamics and there is a clear need for more discussion about this process in the manuscript. The proper way to include ocean warming would actually require the coupling of the ice-sheet model with an ocean model in order to account for oceanic processes such as changes in ocean currents. Still, the representation of sub-shelf melting heavily depends on how ocean models represent these processes.

We have developed the discussion about sub-shelf melting and the limitations of its representation in Sect 2. We have included a new description of the link between atmospheric and oceanic warmings in Sect. 2.1 and we have also included a more detailed discussion about the physics of sub-shelf melting and how it is implemented and linked with atmospheric forcing in the f.ETISH model in Sect. 2.2.5 and given more details about the limitations in Sect. 4.5 (see also the answers to the specific comments about p.4. l.17, p.5 l.5 and p.7 l.28–30 for more details about these changes in the manuscript).

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.

We agree that the manuscript should be made accessible to the largest possible audience. We tried to reduce technical jargon at a minimum while being consistent with a coherent probabilistic/uncertainty quantification framework.

The emulator is calibrated separately for each RCP scenario because we modelled the uncertainty in the climate forcing as a categorical variable (with one of the RCP scenarios). Constructing an (PCE) emulator for a categorical variable is not easy. In addition, we think that it is more coherent to build a separate emulator for each RCP scenario because we do not intent to study the uncertainty to climate forcing for a general forcing.

The emulator is built for each of the model configurations (that is RCP scenario and sliding law) 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.

See also answers below to questions about p.9 l.32 for additional details.

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.

We agree with this suggestion. We have added additional references and background material in the introduction and the description of the model (see answers to the specific comments for more details about these changes in the manuscript).

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.

Based on our experimental set-up, the relaxation times do no contribute to the uncertainty (at least significantly). We have removed the word "perhaps" to have clearer conclusions in the abstract and left the discussion about the impact of the relaxation times on the uncertainty in the main text.

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

We have added in the abstract that the contribution of the uncertainty in sub-shelf melting to the uncertainty in sea-level rise projections goes from 5–25 % in RCP 2.6 to more than 90 % in RCP 8.5.

- p.1 l.20: references missing

We have added some references (Fretwell et al., 2013; Vaughan et al., 2013) for this sentence.

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

We have added a physical interpretation of a confidence region in glaciology to make it clearer. We also have added references (Bolin and Lindgren, 2015; French and Hoeting, 2015) regarding the definition and construction of confidence regions in uncertainty quantification analysis.

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

The sentence has been changed following the referee’s suggestion.

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

We have added references for the SIA (Hutter, 1983; Greve and Blatter, 2009) and SSA (Morland, 1987; MacAyeal, 1989; Weis et al., 1999) 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., 2018b) demonstrating that this flux condition does not accurately represent confined ice shelves, which is the case for most ice shelves around Antarctica.

We agree that using a flux condition at the grounding line may be not appropriate to represent grounding-line migration notably in the presence of (strongly) buttressed ice shelves. We agree that a flux condition is an approximation (parameterisation) for the dynamics of the grounding line at coarse resolution but it has been shown to capture the steady-state behaviour of grounding lines under weak buttressing (Schoof 2007; Docquier et al., 2011; Pattyn et al, 2012). In addition, the implementation of a flux condition at the grounding line has been shown to reproduce qualitatively ice-sheet dynamics as determined with other ice-sheet models (especially the SSA model); yet, it cannot reproduce quantitatively changes in ice-sheet mass and sea-level rise contribution as determined from models with a higher level of complexity, such as the Blatter–Pattyn and the full Stokes models that include vertical shearing at the grounding line, especially for short transients (decadal time scales) as these flux conditions have been derived at steady state (Drouet et al., 2013; Pattyn et al., 2013; Pattyn and Durand, 2013). We are aware that a proper representation of grounding-line migration without the need for a flux condition would require a very fine grid resolution (possibly less than 200 metres), which is intractable for large-scale AIS modelling within an ensemble. Using a flux condition may also suffer limitations in the presence of buttressed ice shelves as suggested by Reese et al. (2018b). Yet, Reese et al. focus primarily on the physical representation of buttressing and not on the parameterisation of buttressing, which aims at translating a complex physical process into a simplified representation. Reese et al. also predict and discuss the appearance of non-physical negative buttressing, which is not allowed with parameterisations (Pollard et DeConto, 2012; Pattyn, 2017). Furthermore, the analysis of Reese et al. is purely diagnostic, while many of the effects described in their paper vanish with an

evolving ice sheet (as it is the case with the f.ETISh model).

We have written a new paragraph and added additional references (Docquier et al., 2011; Pattyn et al., 2012, 2013; Drouet et al., 2013; Pattyn and Durand, 2013) to discuss in more detail the applicability (reproduce the migration of the grounding-line at coarse resolution for the SSA model) and limitations (short transients, buttressing) of using a flux condition at the grounding line.

- 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.

We have changed this sentence to specify that sub-shelf melt rates are computed with an ocean-model coupler based on the Postdam Ice-shelf Cavity mOdel (PICO) ocean-coupler model (Reese et al., 2018a) rather than a simpler parameterisation of sub-shelf melting (Beckmann and Goosse, 2003; Holland et al., 2008; Pollard and DeConto, 2012; de Boer et al., 2015; Cornford et al., 2016).

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

In f.ETISh version 1.0 (Pattyn, 2017), the positive degree-day model was based on the implementation by Huybrechts and de Wolde (1999), while it is based on the implementation by Janssens and Huybrechts (2000) in f.ETISh version 1.2 for numerical efficiency. We have changed the sentence to give more details about the description of atmospheric forcing in f.ETISh version 1.2 and added corresponding references. We now explain that the description of atmospheric forcing is based on a parameterisation of the changes in atmospheric temperature and precipitation rate (Huybrechts et al., 1998; Pollard and DeConto, 2012), a parameterisation of surface melt with a positive degree-day model (Janssens and Huybrechts, 2000) and the inclusion of meltwater percolation and refreezing (Huybrechts and de Wolde, 1999).

- 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.

We agree that stating that the model is "essentially scale independent" is certainly a very strong statement and such statement should be avoided. Yet, Pattyn (2017) has suggested that his model was able to produce rather scale-independent results for a resolution of the model around 20 km due to the flux condition that makes the model results rather independent from the resolution (at least for the SSA model (Docquier et al., 2011; Pattyn et al., 2012)) and allows to reproduce MISI in large-scale ice-sheet simulations (the flux condition has been shown to pass the tests on reversibility, which is a major criterion in establishing MISI (Pattyn et al., 2012, 2013)). However, such spatial resolution cannot capture properly certain mechanisms that control grounding-line migration such as bedrock irregularities and ice-shelf pinning points even using sub-grid parameterisations of these mechanisms. Hence, we may expect discrepancies between our results and results at higher spatial resolutions (<5 km), especially for important small glaciers such as Pine Island and Thwaites glaciers, which represent only a few grid points with a 20-km resolution. Yet, using a higher spatial resolution (even with an emulator and given our computational ressources) remains rather intractable for large-scale and long-term ice-sheet simulations and large-ensemble simulations. We expect the uncertainty in the results due to the choice of the resolution to be limited when compared to the uncertainty in the results due to

the uncertainty in the model parameters and forcing.

We have changed this paragraph to discuss more thoroughly the choice and the limitations of using a 20-km 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.

We have changed the sentence to make more explicit the link between the atmospheric forcing and the oceanic forcing. The link is explicitly written as $T_{\text{oc}} = T_{\text{oc}}^{\text{obs}} + F_{\text{melt}}\Delta T$, where T_{oc} is the ocean temperature on the continental shelf, $T_{\text{oc}}^{\text{obs}}$ is the observed present-day ocean temperature on the continental shelf, F_{melt} the ocean melt factor and ΔT the change in background atmospheric temperature. The equation for T_{oc} with $F_{\text{melt}} = 0.25$ has been shown to reproduce trends in ocean temperatures following an analysis of the Climate Model Intercomparison Project phase 5 (CMIP5) data set (Taylor et al., 2012) for changes in atmospheric and ocean temperatures (Golledge et al., 2015).

- 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.

This is a good question. Such initial drifts are not necessarily undesirable (given that the present-day Antarctic ice sheet is not at equilibrium) and simply subtracting the results from the initial drift may be a questionable assumption as it assumes that the model response to perturbations is totally independent of this initial drift. Yet, we have decided to ignore these initial drifts for practical experimental considerations as carried out similarly in other experimental set-ups (Golledge et al., 2015; Goelzer et al., 2018, Schlegel et al., 2018). The initial drifts for the reference runs reach a quasi-equilibrium at the end of the simulation and range from 0.1 to 0.2 metre of sea-level rise by 3000 for the different sliding conditions. The impact of the initial drift is more important for projections on a short-term time scale and has a limited impact on medium-term and long-term time scales. To isolate the impact of the sliding conditions on short-term projections and avoid any spurious impact of the initialisation procedure, we had to correct for the initial drifts, with differences between the reference runs for the different sliding laws comparable to the ordre of magnitude of the projections by 2100. We have added a bit of clarification about this correction in Sect. 2.1 and Sect. 4.5 and its limitations.

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

We did not mention the uncertainty in sub-shelf melting in the list as this source of uncertainty is accounted for through the uncertainty in a physical model parameter (F_{melt}). We agree that this source of uncertainty should be written more explicitly in the sentence given its significance for the response of the Antarctic ice sheet. Hence, we have added the phrase "sub-shelf melting" in the sentence. We have changed the overall sentence to list the uncertain physical processes that we consider and added a new sentence that explains that we account for uncertainty in these physical processes by considering an ensemble of uncertain parameters in the f.ETISh model.

- 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.

We have replaced the phrase "uncertain parameters" by "Parameters with probabilistic

representation" to avoid any ambiguity. We agree that the standard value is usually considered as $m = 3 = n$ (Weertman, 1957) with n the exponent in Glen's flow law and this value has been used in numerous studies (Schoof, 2007; Pattyn et al., 2012, 2013; Brondex et al., 2017; Gladstone et al., 2017). Yet, a linear relationship ($m = 1$) is also used commonly in numerical simulations (Larour et al., 2012; Schäfer et al., 2012; Gladstone et al., 2014; Yu et al., 2018). Here, we have adopted an intermediate value ($m = 2$) between these two usual values ($m = 1$ and $m = 3$) as the nominal value of the sliding exponent in our simulations. We have added a new paragraph in Sect. 2.2.2 to clarify this (maybe unconvential) choice.

- 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.

We agree with this remark. We have changed the sentence to highlight that the atmospheric forcing is expected to cause changes in the mass balance of the Antarctic ice sheet rather than dynamical changes. We have added some references for this sentence (Lenaerts et al., 2016; Pattyn et al., 2018).

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

We have followed the referee's suggestion and added this reference.

- 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.

We have added a new paragraph that explains how ocean circulation results in sub-shelf melting and how changes in atmospheric and ocean warmings can affect ocean circulation. We have also added a new paragraph to explain how changes in atmospheric and ocean warmings are linked to sub-shelf melting in the PICO model (Reese et al., 2018a).

- 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.

In our experimental set-up, the climate forcing (represented by the change in atmospheric temperature ΔT) is represented as a discrete/categorical parameter with four different trajectories (the 4 RCP scenarios). This is similar to the sliding exponent, which can only take discrete values ($m = 1, 2, 3$ and 5) in our experimental set-up. Actually, 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. Building an emulator based on a PC expansion for discrete/categorical parameters/configurations is not straightforward and we prefer keeping them distinct for clarity.

- 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.

In our experimental setting, 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.

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

The word "subregions" has been replaced by "regions"

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

The sentence has been changed to follow the referee's suggestion.

- 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.

It is difficult to draw general conclusions as the results are dependent of the characterisation of the uncertainty in the bedrock relation times. Yet, we found in RCP 2.6 that the uncertainty in the bedrock relation time for West Antarctica can account for 10 % of the uncertainty in ΔGMSL when τ_{w} is allowed to vary widely between 50 years and

3500 years. We wrote this number in the text to give the reader an idea about the influence of τ_w.

- p.12 l.9: remove "significantly"

The word "significantly" has been removed from the manuscript.

- 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.

As explained in Sec. 3.2. we used the PC expansions to visualise how the projections depend on each parameter individually (one-at-a-time) while keeping the other parameters fixed at their nominal value. This means in particular that $F_\mathrm{melt} = 0.3$ in Fig. 5(a–c) and Fig. 5(g–o) (this represents a background oceanic forcing). So the response of the Antarctic ice sheet is still driven strongly by ocean warming in RPC 8.5 (even if we assume no uncertainty in F_melt). This explains the difference of magnitude between the difference time scales as ocean warming triggers MISI over time.

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

Beyond 2000 (start of our simulations), the RCP scenarios get warmer (RCP 8.5 warmer than RCP 6.0, RCP 6.0 warmer than RCP 4.5 and RCP 4.5 warmer than RCP 2.6). It is more likely to observe a decrease in sea-level rise in RCP 2.6 than for instance RCP 6.0 or RCP 8.5, that is, under cooler atmospheric conditions.

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

In our own opinion, it is not clear how to separate properly the impact of the ocean and the atmosphere because both are linked in our model. The atmospheric forcing (through the RCP scenario) and the ocean melt factor F_melt both contribute to the uncertainty in the ocean temperature and therefore to the uncertainty in sub-shelf melting. Hence, the ocean forcing and the atmospheric forcing are dependent variables and their individual impact cannot be directly isolated. On the other hand, the five parameters F_calv, F_melt, E_shelf, τ_e and τ_w are (assumed) independent and their individual impact on the projections can be isolated. It would be possible to separate the impact of the ocean and the atmosphere using another representation of these processes. However, the direct impact of the atmospheric forcing is quite limited as sub-shelf melting (indirect impact of the atmospheric forcing) is for instance an order of magnitude larger than surface melt. So basically, we witness response to ocean warming.

- 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.

We have added that this result is due to a much larger increase in the values of the ΔGSLM projections compared to the increase in their dispersion when the RCP scenario gets warmer. Hence, the relative uncertainty in ΔGSLM projections is automatically reduced.

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

This sentence has been corrected based on the referee's comment.

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

The phrase "both regions" refer to the 100 % confidence region for grounded ice and the present-day grounded ice region. We have modified the sentence to make it clearer. "differences between both regions are only of few tenths of kilometres by 3000" has been replaced by "the 100 % confidence region for grounded ice by 3000 only differ from the present-day grounded ice region by a few tenths of kilometres".

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

The distribution is still uniform. In the other sections, we considered a uniform distribution with support $[F_{\text{calv,min}}, F_{\text{calv,max}}]$. In Sect. 3.6, the distribution is still uniform but with support $[F_{\text{calv,nom}} + \alpha(F_{\text{calv,min}} - F_{\text{calv,nom}}), F_{\text{calv,nom}} + \alpha(F_{\text{calv,max}} - F_{\text{calv,nom}})]$ and the size of the support (thus the uncertainty) is controlled by the parameter $\alpha \in [0, 1]$. Idem for the other parameters. We have changed the first paragraph in Sect. 3.6 to make it clearer.

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

The emulator is an approximation to the parameters-to-projection relationship determined by the ice-sheet model. The difference between the response of the emulator and the ice-sheet model should of course be as small as possible.

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

We have rewritten what are the probability intervals in the captions of Table 4 and Table 5.

- 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.

We agree that this sentence is a bit too general and does not reflect the results shown in the manuscript. Hence, we have changed it to point out that the impact of atmospheric forcing becomes important for medium-term and long-term time scales.

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

The referee is right in his/her interpretation. We have changed the sentence as "Schlegel et al. (2018) found that grounding-line retreat is most significant in the Amundsen Sea sector under generalised ocean warming experiments for the Antarctic ice sheet, but, after calibrating sub-shelf melt rates with bounds that vary region by region and are assigned values deduced from the literature and model sensitivity studies, they found that the western Ronne basin has the greater sensitivity" to emphasise more clearly the impact of 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.

We mean that the significance of the contribution of the Antarctic ice sheet to sea-level rise under climate change is primarily controlled by the sensitivity, the response time and the vulnerability of its marine drainage basins. The phrase "the contribution of marine drainage basins" has been replaced by the more explicit phrase "the sensitivity, the response time and the vulnerability of its marine drainage basins"

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

We have followed the referee's suggestion and added some references (Timmermann et al., 2012; Depoorter et al., 2013; Rignot et al., 2013; Moholdt et al., 2014)

- 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?

This is a good point. We have checked that considering a PC expansion of order 4 instead of order 3 gave similar results to those in the manuscript. We may expect that for a PC expansion with a higher polynomial degree the results would be degraded due to the fitting of numerical noise (it has not been checked as the number of training points is insufficient to build a PC expansion with a too high polynomial degree) .

A discussion about a different choice for the construction of the emulator (maybe a Gaussian emulator) is beyond the scope of this paper but it would be worthwhile to gain insight into the efficiency of different choices of emulator in glaciology.

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

Based on our experimental set-up, the relaxation times do no contribute to the uncertainty (at least significantly). We have removed the word "perhaps" to have clearer conclusions.

- p.24 l.8: What is g_I?

The term g_I is just the remaining term of the decomposition of $g(\mathbf{x})$ in terms of the

mean value g_0 and the main effects g_i's. It is simply given as

$$g_I(\mathbf{x}) = g(\mathbf{x}) - g_0 - \sum_{i=1}^{d} g_i(x_i). \tag{1}$$

We did not bring attention to this term as it is not required to compute the Sobol indices (and the interaction index S_I can be computed as $S_I = 1 - \sum_{i=1}^{d} S_i$). We have added Eq. eq:gI in appendix B to clarify 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?

We agree that this sentence is a bit ambiguous. Here, we do not allow for refreezing under ice shelves. Still, refreezing rates are in general small compare to melt rates. We have changed the caption to clarify that refreezing is not taken into account in the model.

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

We have added the ice shelves as suggested by the referee.

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

We prefer keeping different axes for the three columns as our goal for these figures is to show the relationship between each individual input parameter and ∆GMSL rather than comparing the magnitude of the response. Given the difference in the order of magnitude of △GMSL for the different time scales, it would be difficult to clearly see the shape of the relationship with the same axis.

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

See answer to question about p.12 l.12.

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