

Replies to the 2nd round of comments from referees 1 and 2 of: “Antarctic sensitivity to oceanic melting parameterizations”

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This document contains the responses to the comments made by Referee 1 firstly and by Referee 2 secondly, for their second report in regard with the revised version of our manuscript. Note that our comments are in blue.

In the text below, we respond to the specific points raised by the referees. Apart from the comments, we have changed some citations that have changed since our last submission, especially the reference to the ISMIP6-2300 dataset.

Referee 1 comments:

I would like to thank the authors for their careful considerations from the first round of reviews they have received. I believe they addressed most of my comments. The extensive new text added to the manuscript is leading me to make additional minor comments.

We would like to thank the reviewer for helpful and thorough comments which have helped to improve our manuscript. In addition, we know now that Seroussi et al. (2024) has been accepted, so following your suggestion in your previous report and after personal communication with Dr. Seroussi and the ISMIP6 team, we will change the reference for the new ISMIP6-2300 dataset.

I will begin by making a style comment that is personal (I hinted at it in a few places in the first round of review). Many parts of the manuscript are written using a passive voice and I find it, at times, unnecessary and making a sentence more difficult to read than it should. I would encourage the authors in the future to try to use a more active style which (in my opinion) helps with the flow and the delivery of the information. Again, this is a personal taste.

We have taken into consideration using a direct voice when possible.

Below you can find the one-by-one response to your technical comments.

Technical comments

Line 134: remove the first reference to “(Lipscomb et al. 2019)”. The one at the end of the sentence is enough. Also, add the limit in ice thickness you are using similarly to your response in my first review.

We have deleted the first reference and added the limit for ice thickness (200 m) as in the first review.

Line 136: define the acronym for ELRA.

We have defined ELRA as the Elastic Lithosphere Relaxing Asthenosphere method.

Line 144: replace the citation of (Seroussi et al., 2020) by (Favier et al., 2019).

Done.

Line 150: rewrite “where it has been denoted by $TF(x,y,z_{draft})$ ” by “where $TF(x,y,z_{draft})$ denotes”. Also, z_{draft} is not the thickness of the ice shelf but rather the depth of the ice shelf base (or the draft of the ice shelf).

We have redefined that sentence in the new version and clarified the definition of z_{draft} conveniently. Thank you for this correction.

Line 168: please, define the acronym AOGCM.

We have defined it as Atmosphere-Ocean General Circulation Model in the new manuscript version.

Line 177: “Therefore,...” I will reiterate my statement from my first round of review. The ocean thermal forcing does not react independently from the atmospheric condition (given that is directly linked to ocean temperature and salinity). The way I interpret your sentence is such that if you were to warm atmospheric temperature by say 4 degrees worldwide (using various mechanisms) in CCSM4, the ocean will not react to this change. I find it very hard to believe. If, for similar forcing, a model yields a different ocean response, it could be tied to many aspects of the model including model resolution, model physics, model sensitivity, ... Again, I would simply remove this statement which does not add to any points you are trying to make in this paper.

We agree with this suggestion; therefore we have removed it in the current version.

Line 177-180: this sentence is a direct repetition of the previous one. I would remove it.

Yes, we have removed it.

Line 182-184: this seems to be out of context here because the evolution of grounding lines is dependent of your model and its transient behavior. Are you trying to say that in WAIS, many places of the bed topography are reversed sloping, and the grounding line will retreat to location with increasing thermal forcing?

Yes, indeed, we wanted to illustrate that when the ice sheet retreats, the grounding line evolves and thus the thermal forcing changes. We will add that information to the sentence.

Line 185: Looking at Figure 2a, it looks to me that the temperatures in the Weddell sea and over the Ronne-Filchner ice shelf have risen more compared to the regions you

are referencing. I would simply rephrase as: “The temperatures over Ronne-Filchner and Ross ice shelves and the Amundsen Sea rose more than 15K ...”

You are right, we will change that sentence for the new version.

Line 206: replace “variables” by “ice thickness and bed elevation”

Yes, it is more specific as you say. We will change that for the next version.

Line 226: The content here does not deserve its own section. I would simply move it to section 2.3 where you describe your experimental protocol. Otherwise, move your entire experimental protocol here.

Yes, after the comments of both referees the section has been reduced to three lines so we have decided to move it to section 2.3 in the next version of the document.

Line 229: replace “held until 2500” by “held constant until 2500”.

We have replaced it as it is important to clarify that.

Line 240: the ice shelf velocities are overestimated almost over all ice shelves except Ronne-Filchner. Please add this distinction.

Yes, indeed. We have made the distinction to give that information.

Line 244: replace “climate” by combined “atmospheric and oceanic”.

We have decided to rename this section titled as: “Sensitivity to combined atmospheric-oceanic forcing and γ_0 value”

Line 267: rewrite “On a regional basis,” as “Regionally,”.

We have changed for the next version of the manuscript.

Line 268: I believe you mean “more than 2m” as opposed to “up to 2m”.

Yes, it is what we meant. We have changed it.

Line 275: replace “the reasons behind the behaviour of the time series shown” with “our results”.

We have done that for the new version.

Line 277: replace “(Figs. 8 and A4, respectively)” with (Figs. 8, A4, A5).

We have replaced that reference to the figures as suggested.

Line 279-280: simplify the sentences by removing some of their text and writing something like: “the EAIS also loses ice particularly in the margins of the ice sheet, while...”

We have changed that sentence by: “the EAIS also loses ice mass particularly in the margins of the ice sheet (e.g. the Amery ice shelf), while there is gain of ice in the interior. “

Line 284: replace “between 2200 and 2300” by “prior to 2100”. In the figure you can see the grounding has moved already at 2100.

Yes, but it becomes more clear from 2100. Nevertheless, you are right and we have changed the sentence.

Line 293: replace “in the sense that is gaining ice” with “in the sense that it is gaining ice”.

Thank you for the revision, we have added “it”.

Lines 344-350: this is a direct repetition of your results. I would suggest removing this paragraph since your discussion is now a bit long. I don’t have a strong feeling about this.

We have removed the entire paragraph.

Line 374: “that could explain this difference” This would explain it if the resulted forcing were different. Please rephrase.

We think that the difference lies in the warm biases in CESM2-WACCM (Purich and England, 2021) due to the representation of the cloud feedback (Zhu et al., 2021). We have addressed this in the Discussion section, but in this particular sentence (Line 374) we will make reference to this as these two articles point to the fact that CESM2-WACCM is a model producing more warming than others.

Line 384: the underestimation of Lipscomb et al. (2021) is only true if you consider your model to be the truth. Maybe you meant to say their results is underestimated compared to yours? Please rephrase.

We wanted to point out that Lipscomb et al. (2021) use data from the 21st century that in a changing climate could not be entirely realistic and therefore could be underestimating the future emissions that the climatic component will experience. Nevertheless, we will follow your suggestion and rephrase the sentence in order to incorporate the comparison to our results when referring to the underestimation issue. There is also a comment on the Introduction that has changed after this suggestion.

Line 384: remove the first “but”.

Thank you for noticing, there were two “but” and we have removed the first one.

Line 407: replace “contrary” with “opposite”.

Thank you for the language revision here. We have changed it.

Line 408: replace “with 16” with “using 16” and remove the word “grids”.

Yes, we have changed that and removed the word “grids” in that sentence.

Line 409: replace “nonetheless” with “while”.

We have replaced it and it is more clear now. Thank you for the correction.

Line 410: remove the word “observed”.

We have deleted that word.

Line 410: “This suggests...” Mentioning MISI suddenly is a bit far-fetched without finer analysis. I would remove this sentence.

OK. We have removed that sentence. We took that sentence from Sutter et al. (2023), maybe just referring to it is enough to clarify.

Line 428: replace “and that results” with “resulting”

We will do it for the next version.

Tables

Table1: you are running more than 5 experiments in your work. I would modify this table to capture them all. As is, this table mentions the forcing and model used for the experiment. You could add 3 columns for each set of experiments: 1 column for “experiment forced with atmosphere + ocean”, 1 column for “experiments with ocean only” and another one for “forcing with atmosphere only”.

Here we think that there is a linguistic problem. As an experiment we mean the model used, that it is certain that has been used three times (atmosphere-ocean combined, ocean-only and atmosphere-only). If we modify the table as you say, we will see the same information in all columns as the Scenario and Model does not change, only the activation or deactivation of the oceanic/atmospheric component which can be said by words as in a previous section (2.4 in the revised manuscript). We have clarified in a better way in the table caption saying: “Description of the five types of experiments carried out based on the protocol for ISMIP6-2300....” and in the paragraph mentioning the “only” cases subset of experiments. See also our next comment for Figure 1.

Figures

Figure 1: these forcings are also used in your other 10 experiments, right? If so, correct the “five experiments” in the caption. Also, in the caption, I would suggest rewriting “evolving grounding lines in the AIS (c)” by “evolving grounding lines (c) in the AIS”. Your version made me believe that the word “AIS” was only for panel (c).

As in the last technical comment, we have only considered five experiments (those in the legend and in the caption) corresponding to the five different models, but it is true that we have used them three times for each type of simulation. We have clarified in a better way in the caption saying: “the five models considered for simulation: ...
We have changed the (c) to clarify as you have suggested.

Figure 7: in the caption, rewrite “since the start...” with “between 2015 and 2500”.

We have taken into account this suggestion in the new version.

Figure 8: in the caption, replace “for the years...” with “for years...”

Thank you for your correction. We have done that in the new version.

References

Favier, L., Jourdain, N. C., Jenkins, A., Merino, N., Durand, G., Gagliardini, O., Gillet-Chaulet, F., and Mathiot, P.: Assessment of sub-shelf melting parameterisations using the ocean–ice-sheet coupled model NEMO (v3. 6)–Elmer/Ice (v8. 3), *Geoscientific Model Development*, 12, 2255–2283, 2019.

Referee 2 comments:

Juarez-Martinez et al. has addressed most of the comments and suggestions well. The modified version looks much better. However, there are another three comments which need the authors’ attention:

Thank you for your comments and for helping to make this research article more structured and appropriate. Please, find the one-by-one reply to your comments and suggestions in the next lines.

1. Numbering of subtitles in Sect 3.1 and Sect 3.2: Why do you need a sub-subtitle if there is only one sub-subsection? For example, if you only have 3.1.1, why not just keep it under 3.1? Same for 3.2.1.

We thought that separating them was more suitable to differentiate the several contents based on your previous report comments. In the case of 3.1.1, it allows to separate the general AIS results from the WAIS and EAIS, for example. Same happens for 3.2.1, as it focuses on the WAIS, and in our criterion makes it more clear to the reader to separate. Nevertheless, we have removed the numbering of sub-subsections while keeping their title just to separate the parts as we have commented before.

2. Putting the numbering aside, it's not clear about the difference in focus if you look at the subtitles of Sec 3.1 and Sec 3.2. I think Sec 3.2 is focused on spatial sensitivity to different climate forcing, which is a better subtitle than "Spatial patterns in the loss of ice" and comparable to subtitle of Sec 3.1.

We have taken that into account and renamed the above-mentioned title of the section.

3. In the discussion about the difference to Lipscomb et al 2021, you used different ways to extend the forcing between 2300-2500. You mentioned Lipscomb et al. (2021) considered the climate variability, which is more reasonable compared to yours. In such case, you should say your result overestimates the AIS response rather than Lipscomb et al. (2021) underestimates the AIS response.

With the underestimation of Lipscomb et al. (2021) we were referring to the fact that they use only the last years of the 21st century to make the extension which could underestimate future emissions that we can capture with the data from 2100-2300.

Anyhow, we have changed the text in accordance with another referee to make clear that with "underestimation" we intend to compare with our results and therefore avoid a misunderstanding.