Review of O'Neill et al. 'ISMIP6-based Antarctic Projections to 2100: simulations with the BISICLES ice sheet model'

General comments

This manuscript presents new experiments designed to make projections of the Antarctic ice sheet contribution to sea level rise by 2100. To do this the authors follow the ISMIP6 protocol using the BISICLES ice-sheet model and climate forcing from CMIP5 models. They also run additional simulations that were not part of the original ISMIP6 framework using forcing from a CMIP6 model. The key finding is the important compensating effect surface accumulation can have on ocean-driven ice loss under certain combinations of parameters. I enjoyed reading the paper and feel that it is a useful addition to the originally presented ISMIP6 experiments and those included in the synthesis of Edwards et al., 2021. I think it is appropriate for the journal but I have some recommend changes prior to publication. Most of these relate to improving the readability of the manuscript, and some of the more substantial ones are included below. Minor changes are included in the specific comments in the following section.

1. I found the both the Introduction and Discussion sections to be unnecessarily long. There are also some instances of duplicated text. I think the Introduction would benefit from being shortened and more concise in places.

2. I think the paper would benefit by presenting more detail on the initial ice sheet model state and comparing this to observations and the other model states included in the ISMIP6 experiments. It would be good for the Control simulation section of the Results to include more detail on this, and perhaps an additional figure (see Specific comments).

3. I have several comments with regards to the Figures. 1) I think the number of figures in the manuscript is a little excessive and I wonder if all of them are absolutely necessary for the main messages of the paper. I think some could be moved to a Supplementary Information document at the least. 2) In many cases the Figure captions are not as detailed as they need to be and the individual panels on almost all figures should be labelled. For both points I've included suggestions in the specific comments below.

4. The Discussion as mentioned above is very long and in places presents a stream of new results, and often without any reference to Figures/Tables where the results can be found making it difficult to follow. I think it would be good if each section of the Discussion is considered whether it belongs there or in the Results. At the very least I think Section 4.1 should be moved to the Results. In general it would be good to consider what detail is/is not necessary, and I've made some suggestions in the specific comments. Also the Discussion mostly compares the results to those of Edwards et al., 2021 and Seroussi et al., 2020, and could benefit from referencing additional papers to support your findings.

5. The Limitations section could benefit from adding several more. For example how might your results have been different if you had used a different basal melt parameterisation? Also it would be good to mention the choice of sliding law, and how this is a potential source of uncertainty that has not been explored in these experiments. Finally, it would be good to discuss some unaccounted for processes in your experiments, e.g. iceberg calving (fixed ice front), and the SMB-elevation feedback/evolving topography during the simulations (that you do mention on Line 128).

6. I think the Conclusions would benefit from being revised, making sure they are a brief summary of the paper and not introducing any new information, and towards the end providing summary sentences on the key finding(s) of the paper and the wider importance.

In addition to the points raised above I have a number of specific line comments that are included below, that hope to help improve the clarity in places.

Specific comments

Line 6: could include examples of the uncertain ice sheet processes you are exploring

Line 8-10: I think this sentence would benefit from being rephrased for clarity

Line 12: "increases sea level contribution by 25 mm" relative to what? I think the whole range of sea level contributions from your experiments should be stated in the abstract.

Line 16: Does this mean "dominated the total sea level change of 20 cm" or ice sheets contributed to 20 cm?

Line 36: "future" Antarctic contribution to sea level? Also "represent a greater range of interactions and dynamic processes" reads a bit awkwardly, could it be rephrased, or examples of these "interactions" added?

Line 44-47: These sentences duplicate the information in the previous paragraph (lines 39-43), I suggest combining and removing the second paragraph of the two.

Line 49: I think the estimate of sea level contribution from the ISMIP6 experiments could come earlier/in the first paragraph of the introduction.

Line 60: See later comments and in the general comments section. How does your initial model state here compare to the one included in the initMIP experiments?

Line 64: change to "identified in Barthel" to make it clear that these models were not created in this paper, but evaluated.

Line 79: So the model set-up in this paper was exactly the same as the one presented in the initMIP experiments? that is not totally clear from the text. Does that include the choice of basal sliding law, grid resolution, etc? Despite this, I think more space could be given here and in Section 2.3 on how this initial state compares to that of the other models and to observations.

Line 82: A figure of the model mesh would be good in a Supplementary Information document

Line 85: Perhaps state why this sliding was chosen.

Line 91: How long was this relaxation run? and how does the ice sheet state deviate from the one arrived at after the inversion? Some figures on the change in ice thickness/speed during this relaxation and the comparison to observations at the end of the relaxation/start of the control simulations would be good.

Line 98: Perhaps cite Goelzer et al., 2020 here https://doi.org/10.5194/tc-14-833-2020

Line 100: The first few sentences of this section duplicate the information already presented in the Introduction. I suggest removing either here or from the introduction (lines 63-66).

Line 105: Is this the same selection of CMIP6 models included in Payne et al. 2021, if so state this.

Line 118: Perhaps state here that the approach of averaging over the surface ocean is the same as used in ISMIP6 and add the reference.

Line 125: Why did you not use PIGL5th? State this in the text.

Figure 1: "(0-500m)"

Line 127: Why did you choose to use the surface mass balance from Arthern et al., 2006 rather than RACMO?

Figure 2: The units between the spatial plots and line graph in Figure 1 are the same. It would be good to do that for the SMB anomalies in Figure 2 as well.

Line 141-142: This sentence is almost identical to the one on line 127-128, remove one of them.

Line 142: As mentioned above, it would be good to have more details on your initial model state, the relaxation run, and the subsequent control simulation. It would be useful to include a figure of the surface mass balance and basal melt rates used during the control simulation.

Line 147: It's confusing to have two sub-sections in close succession using the same heading.

Line 148: I don't think it's useful to include the total mass here, remove it and just focus on volume above flotation (as done in the rest of the manuscript).

Line 151: None of the place names in this paragraph are labelled on a figure. I guess the assumption is people will know where they are, but it would be nice to label the main ice shelves.

Line 158: It's almost impossible to see the grounding line retreat on Fig. 3b, I suggest making the map much larger or even creating an inset(s) for the key regions talked about in this paragraph.

Line 165: Replace "majority" with the number of simulations that show net mass loss. Also put this range of sea level contribution in the abstract.

Line 173: State that higher values of γ_0 are darker colours in the figure caption.

Line 187: Be consistent with use of negative numbers for mass gain/sea level fall. Improve the readability of the following sentence.

Line 194-197: Am I correct in thinking you didn't merge these basins and this was done in Jourdain et al., 2020? If so, just state that you use the basins from Jourdain et al., 2020 and remove the detail here.

Line 200: It would be nice to see the perturbation experiments alongside the control simulation (without subtracting it). Perhaps a figure in a supplementary information document.

Line 206: Make it clear when you say "Filchner-Ronne has a large area..." you are talking about the catchment not just the ice shelf?

Line 209: State the range.

Line 210: The important compensating effect of accumulation on ocean-driven mass loss has been noted for the Filchner-Ronne region by a number of previous studies, some of which might be worth citing here e.g. Cornford et al. 2015 and Wright et al., 2014.

Section 3.4: Throughout this section there are almost no references to any figures where the results can be found.

Figure 5: This figure caption is incomplete. State what each panel shows and add the CMIP forcing to the caption as well.. Also add panel labels to the figure.

Line 224: Could state that an increase in VAF in the Filchner-Ronne and Ross regions in the control simulations is consistent with the present-day trend in VAF and some references e.g. Rignot et al., 2019.

Line 225: Given that this section of the results is quite short it could just be combined with the basin discussions in the previous section. Figure 9 could probably be removed or moved to a supplement.

Line 222: Be careful here, this sentence is quite confusing, you are saying that it is "equivocal" that there is potential for MISI in the ASE, but that there is not evidence that for MISI happening yet. I would rephrase this to improve the clarity.

Figure 7: Needs panel labels and increase the font size in the legend.

Section 4.1. The entirety of this section reads as a stream of new results and reference to new figures. I think this would be better moved to the results. I also don't know how useful the spatial discussion is, and was wondering if Figures 10/11 would be better in a supplement and replaced with a figure that shows integrated ice sheet/regional surface mass balance and dynamic ice discharge instead.

Figure 8: The y-axis label is "SLC" but throughout the manuscript you use "SLE" I would be consistent with one or the other. Also I can see the reason for a consistent y-axis scale, but it means it some basins it is impossible to see any change through time, so I recommend modifying the y-axis scales individually.

Figure 9: I am not sure about showing the SLC lines ontop of the spatial plots, they are difficult to see and in several cases they obscure the text. Also the colourbar for the thinning is quite saturated. Could you just show the inland/grounded ice thinning only instead?

Line 278: Not sure this should be the start of a new paragraph. Also, is the same true for other unconstrained ice shelves, e.g. Thwaites?

Line 279: I did not find a clear statement as to why you did not also use the PIGL sensitivities for these CMIP6 model runs?

Figure 10: The caption is incomplete. Refer to experiments in Table 2 (as done in Fig. 9). See earlier suggestions to replace this figure with integrated ice sheet/region wide SMB.

Line 296: This section also reads as new results. If it remains as a Discussion section I suggest adding references to more papers that have shown similar results. E.g. for statements about compensating effects of increased precipitation with warming in East Antarctica mass for example Jordan et al., 2023- https://doi.org/10.1038/s41467-023-37553-2 and Stokes et al. 2022 and references therein https://doi.org/10.1038/s41586-022-04946-0).

Line 305-312: This paragraph again is a list of new results without any reference to any figures/tables where these results can be found. Same is true for the following paragraph.

Figure 12: panel labels

Line 356: Given that this equation is not used in this manuscript it feels a bit out of place in the Discussion, I would suggest removing it and just referring to the equation number in that paper instead.

Line 414: There are several references to Sections of Edwards et al., 2021. I think in most cases it would be better to briefly summarise the findings in that paper in your manuscript so the reader doesn't have to frequently go back and forth.

Figures 14, 15, 16: The text describing the details of each simulation overlaps large parts of the figure/results, I suggest removing and just directing the reader to Table 2, where the details of the experiments are.

Line 421: It is my understanding that future work is underway to extend simulations to 2300? is that worth mentioning here?

Line 427: Can you state why you omitted the PIGL5 values?

Technical corrections

Line 16: Remove ".0" from "20.0cm"

Line 440: Bulthuis reference error