Ice-shelf freshwater triggers for the Filchner-Ronne Ice Shelf melt tipping point in a global ocean model

Author(s): Matthew J. Hoffman et al.

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Response to Reviewer 2

Reviewer comments are in black font, and our responses are in blue font.

This is an interesting and generally well-written paper that discusses the drivers of ice shelf melt regime change in the south-western Weddell Sea as inferred from a relatively coarse resolution global ocean/sea ice model. The authors discuss how modifications to the model can reduce present day biases in the results, and that those changes are critical to the preservation of the current regime under current climate forcing. They further investigate the occurrence of a domino effect whereby increased ice shelf melting in the eastern Weddell Sea can trigger regime change in the south-west and describe how coarse resolution models may be pre-disposed to such a change.

The paper is logically structure and the results clearly presented. The text is pleasingly free of typographic errors and easy to read. Overall, I would recommend publication more-or-less as is, although the clarity could be improved further with a few minor modifications along the lines suggested below.

We thank the reviewer for their feedback and appreciation for the goals of the manuscript.

Teleconnection: I question the use of the term teleconnection. As used in the atmospheric context it refers to a rapid connection between two regions, typically established through a standing planetary wave pattern. If I understand the discussion here, the links are established slowly through the advection of anomalies by the mean circulation. I think teleconnection is a slightly misleading description of such a process.

We appreciate the reviewer pointing out this incorrect application of this term. We will replace it with a term more appropriate for the process described, such as “remote influence”.

Water masses: There is a long and often confusing history of water mass names and acronyms, many of them regionally specific, that have appeared in the literature. While this paper does a reasonable job at navigating a way through that, I think some things could be clearer. DSW is a term that is not clearly defined in the literature or in this paper. It is apparently not just another term for HSSW (as used elsewhere?) because at one point it is stated that model DSW corresponds with observed LSSW. While I think the meanings are for the most part clear, I would encourage the authors to think about adopting the Whitworth et al (1998, Antarctic Research Series, vol 75) classification. They defined four water masses in the shelf/slope region, and while precise boundaries in T/S are region specific, nomenclature and origin/role are consistent in a circum-continental sense. In their classification, Shelf Water (SW) is the key water mass, defined as water that is denser than the regional variety of MCDW.

Thus, presence or absence of SW is the key determinant of the transition in melt regime. Personally, I think Whitworth et al did the community a great service in proposing that simpler, more consistent and more intuitive circum-Antarctic water mass classification, which could clarify the sort of arguments made in this paper were it to be more widely adopted.

Thank you for bringing our attention to this classification, as the lack of consensus in the literature is a source of frustration to us as well. Since our model configurations show significant water mass biases (largely shifted to lower salinity space), the Whitworth et al. classification proves misleading, classifying almost all of the modeled water in this region as ASSW. In the text of the manuscript, we intend the water mass terms to refer to the relative role that the simulated water masses play in the dynamics despite these biases. For this purpose, we believe that DSW, AASW, mWDW and WDW are adequate and connect well with recent literature (which has also tended to use regionally specific terms). We would also emphasize that the water mass definitions shown in Figure 4 are not used in any quantitative analyses (e.g., Figure 7 uses the thermocline); we intend these boundaries as reference points for the reader. We will revise the manuscript to provide additional clarity around the use of the term DSW, which encompasses both HSSW and LSSW in our uses (though our model does not have HSSW on the Weddell shelf).

Model: The model naming is sometimes a little unclear. It is referred to throughout by the acronym ESM, but it is not the ESM used in Commeau at al. It does not have the interactive atmosphere, but are there any other distinctions? The various modifications made to the model parallel modifications to the full ESM, I think. That point only becomes apparent (to this reader at least) when Figure 12 is discussed. I think it would help if the model structure and experiments were set more clearly in the context of the Commeau et al work in section 2.

This manuscript describes the same ocean and sea-ice components from E3SM that were used by Comeau et al. (2020), with the only difference being a prescribed atmosphere instead of the interactive atmosphere component. Reviewer 1 had a similar concern about referring to our model as E3SM throughout the manuscript when being run without the atmosphere model disabled. We will adjust the language in Section 2 and other spots to emphasize we are using a "global ocean-sea ice model" configuration from the components of E3SM.

Region: A location map with key features and all the ice shelves mentioned would really help orient readers, especially those less familiar with the region.

This suggestion was also made by the other reviewer and is a good one. We will add a location map with key features.

Minor comments:

Line 43-44: I'm not sure what feature you are referring to as the "Antarctic Coastal Countercurrent".

We will change this to Antarctic Coastal Current.

Line 54-55: "… the iceberg melt term; …".

Thank you for catching this typo.

Line 148-153: I'm confused by what appear to be slightly contradictory statements about the prescribed melt rates. First you give specific values, but then you say that values are progressively halved relative to the maximum. Most, but not all quoted values fit that description, but is it superfluous anyway given that you quote specific numbers? But maybe it is the numbers that are obsolete (?), as they do not appear to coincide with lines in Figure 11 (at least two of them don't).

We agree this description can be simplified, and we will ensure it matches the actual simulations shown in Figure 11.

Line 165: "… complete forcing cycle is complete". I know what you mean but the wording is a little odd. A complete cycle is complete by definition.

This is a good suggestion. We will reword this phrase.

Line 182: "… highest melt rates occur near the grounding …".

We will correct the tense.

Line 233: "… on the Weddell Sea continental shelf (Fig. 6)."

We will correct the omission of "Sea". We will cite both Figure 6 and 7 and adjust the text to reference the increase in salinity and density of DSW.

Line 279: " … of these leading to a FRIS …"

We will correct this typo.

Line 289: "… simulations experiencing rapid transition …".

We will correct this typo.

Line 321: This is now the first citation of Figure 7, if my earlier correction is OK. I still don't really see that Figure 7 supports the statement. Did you mean to refer to a different figure again? Maybe figure 6 again? In which case, is Figure 7 needed at all?

This statement was referring to small changes above the thermocline that are apparent in Fig. 7. We will clarify the statement by adding the clause "namely, the increase in density above the thermocline due to the change in iceberg distribution is less than the 0.1 kg m^{-3}".

Line 414: "… rapid change to continental shelf temperature …".

We will correct this omission.

Figure 9 caption: Note to self needs deleting.

We will delete this wayward note.