

We are grateful for the very helpful and kind reviews from both the reviewer and editor. Point-by-point responses to comments describing changes made in the revised manuscript are provided below in italics.

Anonymous Referee #1

General Comments

This is my second review of the manuscript, 'Arctic sea ice sensitivity to lateral melting representation in a coupled climate model' by Smith et al., (2021). In my first review, I suggested that this research provides valuable insight and discussion regarding the treatment of lateral melt in sea ice and climate models, highlighting in particular the use of variable lateral melt rate across thickness categories and the application of the concept of open water formation efficiency. This general assessment continues to hold for the updated manuscript.

I previously identified the following general points that I thought should be addressed before publication:

- The description of the methodology was insufficiently detailed.
- The need for more discussion of the limitations of the methodology and any conclusions reached.
- The conclusion reached regarding the role of ice-albedo feedback in changes to sea ice state was not justified by the methodology.
- The inclusion of further figures to add insight.
- The structure of discussion and conclusions.

In my opinion, the above points have been sufficiently addressed in the updated manuscript. I believe the manuscript is within the scope of The Cryosphere and merits publication. I have a few remaining comments and suggested edits that I will list below.

	Specific Comments	Response
1-1	P2 L37-44: In general, this section would benefit from a more detailed explanation of the points being made e.g., 'the representation of the sea ice cover using an ice thickness distribution results in a stronger albedo feedback because of the impact on thermodynamic processes'. Explain what the impact is and how this interacts with the albedo feedback. In addition, the final comment, whilst valid, does not clearly follow from the proceeding section: 'and also the potential effect of lateral melt on driving feedbacks.'.	<i>The sentence referenced here has been edited to read "...because the better resolution of thin ice enhances thermodynamic ice loss." We choose to not further expand this section as the introduction is already relatively lengthy.</i> <i>The final sentence of the paragraph has been re-written with the aim of clarifying what is suggested by the Bitz et al (2001) study. "Bitz et al. (2001) states that "resolving thin ice [using the ice thickness distribution] eliminates the need for partitioning an unrealistically high fraction of heat flux toward lateral melt", indicating the importance of the ice thickness distribution in simulating melt rates. Lateral melt can have an important role in driving feedbacks in a manner similar to the thickness."</i>
1-2	P4 L124-125: It would be useful to explain why you use monthly averages for assessing changes to the mean sea ice state and daily	<i>We have added explanation of why monthly averages are use: "...for computation efficiency</i>

	averages to examine the efficiency of melt processes.	<i>and for better comparison with other studies (which typically use monthly averages)”</i>
1-3	P12 L271-273: Did you use daily or monthly model output for this analysis? Ideally it should be daily to minimise averaging effects.	<i>Yes, this analysis was completed using daily model outputs averaged over the month, for consistency with what we understood was done in the Bateson et al., 2020 study.</i>
1-4	P20 L443-445: Are you able to provide a reference for this statement regarding observations on the MOSAiC expedition?	<i>Unfortunately given the timeline, we do not yet have an appropriate citation for MOSAiC that shows the relevant observations referred to, but we have now included a reference to the Sea Ice Overview paper (Nicolaus et al., 2021) which includes brief mention of this data collection.</i>
1-5	Figure 4: Can you comment on the spatial distribution of positive and negative changes in sea ice concentration in panel (d), since this is a fairly distinct pattern compared to panels (b) and (c)?	<i>The following sentence has been added to Section 3.1: “The changes in spatial patterns with the redistribution of lateral melt (Fig. 4d) results from the patterns in ice thickness distribution, where the relatively high proportion of thick ice to thin ice in summer in the Barents Sea results in an increase in concentration.”</i>
1-6	Figs 3 + 8: It would be helpful to briefly comment in the text on how realistic the mean sea ice state is for your reference simulation.	<i>It is not possible to comment on how ‘realistic’ sea ice is for pre-industrial control runs due to the lack of comparison data for this forcing. However, as we already state in the methods: “Sea ice simulated by CESM2.0 over the historical period has reasonable mean state and variability in both hemispheres (DeRepentigny et al., 2020).” This suggests that the model is producing reasonable ice based on the climate forcing.</i>
1-7	Figs 5 + 10: The plots suggest melt volumes of the order $10^{15} - 10^{16} \text{ m}^3$, however the difference between the minimum and maximum sea ice volume shown in Fig. 3 is about $1.5 \times 10^{13} \text{ m}^3$ i.e., the melt volumes are too high. I am wondering if the axis is supposed to have units of kg rather than m^3 .	<i>Good catch! There was a unit error in Figures 5 and 10 that has now been remedied. This correction has no impact on the results or conclusions. Note that the melt/growth volumes still appear somewhat high as there are small amounts of growth and melt throughout the year that are not reflected in the volume change.</i>
	Technical corrections	
1-8	P2 L34, P2 L49, P3 L61: Define acronyms or otherwise explain terms used.	<i>All acronyms have been defined at first use or written out.</i>
1-9	P4 L108: Replace ‘must’ with ‘much’.	<i>Done.</i>
1-10	P4 L118: I am not sure what you mean by the final clause, ‘included in prior versions’.	<i>This has been changed to “as standard in prior versions of the CICE model”, which is hopefully more clear.</i>

1-11	P6 L161: Can you double-check the definition for ΔT (delta T)? In general, I would expect the inclusion of Δ (delta) to indicate the term refers to a difference.	<i>Yes, good catch. We have changed the beginning of the sentence to "The temperature of the surface ocean above freezing..." to indicate that the relevant temperature here is the difference from freezing, hence the delta.</i>
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Editor:

General Comments

I thank the authors for submitting their revised manuscript to The Cryosphere. I especially appreciate the authors clear response to the comments raised in the previous review. Thank you.

* 37-44: Comment raised by reviewer:

I suggest to BRIEFLY weave in the argument of "floe-size distribution" to address the reviewer's comment and to round up your background discussion. The paragraph 31-44 contains two major discussion points, which currently appear in conflict. Suggest to split them into separate paragraphs, or to join 31-37 with the previous paragraph.

The referenced sentences have been split into two paragraphs, which indeed better distinguishes the two distinct ideas. In the interest of not lengthening an already long introduction, we elect to not introduce any discussion of floe size distribution to this paragraph. The impact of floe size distribution on sea ice via the albedo feedback is already introduced later in the introduction. As this part of the introduction focuses on components of coupled climate models as they currently exist, it does not seem appropriate to introduce it here instead.

* Use of the term "lateral melt rate".

The authors related the term "lateral melt rate" to the floe size (l65), and from then on equate changes in floe size to a linear change in "lateral melt rate". Pls include a short statement that this is a convenient approximation in the context of this study.

We do have a statement to this effect in Section 2.2: "As such, changes to the floe size are used as a catchall for factors impacting the sensitivity of the lateral melt rate." Acknowledging that this was a bit buried as presently presented, also added earlier in this paragraph "...where floe size is used as a convenient approximation for lateral melt rate in the context of this study."

	Technical corrections:	Response
E-1	123: Correct " 1.0°C to 1.5°C " to " 1.0C to 1.5°C ".	<i>Done.</i>
E-2	124: Add that you reuse EXISTING (??) runs, and why you average over the last 25 years.	<i>The only existing run used is the "control"; all other runs were completed for this study. In regard to why we have averaged over the last 25 years, we have added to the text "...after the system has equilibrated and to minimize the contribution from internal variability."</i>
E-3	Provide start and/or end year for the 25 years used.	<i>We add to the sentence referenced "... (simulation years 35-60)". Note that these are constant forcing runs so no calendar years are relevant.</i>

E-4	171: Correct "i.e." to "i.e.,".	<i>Done.</i>
E-5	487: Rewrite "may be altered by" to "may require revision due".	<i>Done.</i>
E-6	Fig.2, caption: Capitalize "effective" (y-axis) and "ice" (x-axis).	<i>Done.</i>
E-7	Fig.7: Move the legend to the right, so it does NOT partially overlap with the figure itself.	<i>Done.</i>
E-8	Fig.10: Legend of symbols is missing "dotted" for snow-ice. Pls add.	<i>Done.</i>
E-9	Fig.13, caption: For consistency with Fig.6, replace "Black lines denote" with "Black bars denote".	<i>Done.</i>
E-10	Several figures: There are no x-axis labels when "Time" or unit is the x-axis, pls add for all relevant figures. E.g., Fig3 etc.	<i>Done. Appropriate label ('Date' or 'Month') has been added for all figures.</i>