The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-67

Anonymous Referee #1

Referee comment on "Arctic sea ice sensitivity to lateral melting representation in a coupled climate model" by Madison M. Smith et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-67-RC1, 2021

General Comments

This paper revisits the longstanding parameterisation of lateral melt used within the CICE (Los Alamos) sea ice model and explores how assumptions about the representation of lateral melting impacts sea ice within a coupled climate model (in this case CESM2.0). As the authors note, significant progress has been made recently in terms of representation of floe size within sea ice models, but there has been a corresponding lack of attention paid to the lateral melting parameterisation. Whilst other studies have explored similar themes exploring sea ice model sensitivity to floe size and lateral melting e.g. Steele et al. (1992), and more recently Bateson et al. (2020), this is the first study I am aware of that addresses the assumption of a constant lateral melt rate across all sea ice thickness categories. The application of the concept of open water formation efficiency to provide further understanding of how lateral and basal melting processes impact the sea ice cover is a particularly strong and valuable feature of this work. I therefore believe that this paper initiates an important and valuable discussion into the lateral melting parameterisation in sea ice models and will make a valuable contribution to the literature.

The scientific quality of the work presented is generally strong, with good associated analysis and discussion. The methodology could be more thorough in terms of the details provided, and there is perhaps insufficient consideration of the limitations of the conclusions reached. I am also unconvinced that some aspects of the conclusions reached are justified by the results presented and these either need to modified or further evidence provided. The figures are of a good quality and appropriate to the discussion. I have suggested a couple of additional figures that might be helpful to illustrate some of the discussion, but this is not essential. Similarly, the structure generally seems fine, though I do have some questions about whether some of the discussion should be moved to the results section, and some of the conclusion section then moved to the discussion. The paper reads well, is clear in its conclusions, and also has a representative abstract and title.

Overall, I believe that this paper is within the scope of The Cryosphere and, with some moderate edits, merits publishing.

Specific Comments

- P2 L52-53: Could you add further details on what you mean by the following: 'such as related to model resolution,'?
- P2 L58: Re following, 'i.e. Bateson et al., 2020)'. e.g. would probably be better here rather than i.e. since the study referred to is one of several on this theme.
- P3 L62-64: Re following statement, 'different results might be expected in a coupled climate model that allows feedbacks related to the formation of open water'. There is some evidence of this in Fig. 5 in Roach et al. (2019). Simulations with a standalone sea ice model generally showed a reduction in lateral melt and increase in basal melt of comparable magnitude, but in a coupled sea ice-ocean setup the reduction in basal melt was significantly smaller than the increase in lateral melt. Might be worth referring to this?
- P3 L74-76: I am unconvinced you have achieved the final aspect of this objective with the results presented: 'as a result of ice-albedo feedback'. Later comments will further address this. You may need to modify this paragraph depending on how you decide to address some of the later comments.
- P3-4 L79-96: I think in general this section would benefit from a more complete discussion of details of the model setup that are pertinent to this study e.g. additional details of the SOM (given the importance of surface ocean properties to lateral and basal melt rates), a more complete description of the forcing and how it is applied, and details on how the sea ice is initiated.
- P3 L80-89: Given the significant focus in this paper on the ice-albedo feedback, I think some discussion is required here or elsewhere about the possible impact of using a prescribed mixed-layer depth without a full representation of sea ice-ocean feedbacks.
- P3 L85: Re following statement, 'although not specifically constrained in the model'. Can you clarify what you mean by this?
- P4 L93-94: It would be helpful to add a brief comment on the tuned albedos. How are they different from standard values used?
- P5 L127: Re following statement, 'if it does, reductions are made to the lateral and basal melt rates by a constant factor'. A more detailed explanation would be helpful here on how the limits to the lateral and basal melt rates are calculated and applied.
- P6 L144: Can you provide more details on why you specifically selected this form of lateral melt redistribution (as opposed to an inverted r_n, or higher / lower spread of values for r_n)?
- P6 L145: Re following statement, 'these values were distributed around 1 with the aim of keeping the total lateral melt volume approximately the same, such that the effect of the redistribution can be uniquely observed'. Does this not rely on an equal distribution of ice volume between thickness categories? In locations dominated by thin or thick ice, would this setup not produce abnormally high or low lateral melt rates?
- P7 L179-180: Re following statement, 'lateral melting rate is applied to all categories equally'. You should clarify that this is for the standard lateral melt parameterisation only, not the simulation using Eq. (4).
- P8 L196-197: Re following statement, 'contrary to intuition, increasing the lateral melt does not necessarily reduce sea ice area and volume'. My understanding from Fig. 3 is that in both simulations where the lateral melt rate is increased, the sea ice area is reduced, and the same is true for volume from March to August? I think this statement should be reworded to better reflect the results presented in Fig. 3.

- P9 L202-205: A map plot showing differences in sea ice concentration might be useful here to illustrate how the differences vary across the sea ice cover.
- P9 L211-212: Have you done any analysis of the model output to confirm that the available heat content in the surface ocean is the limiting factor for basal / lateral melting? This is not the only mechanism for the basal melt compensation effect in response to an increase in lateral melt in sea ice models e.g. in Bateson et al. (2020), it is demonstrated that the primary mechanism in standalone CICE is from the physical reduction in available sea ice area for basal melt (see Figs 4-5 in that paper). I think you either need to do some additional analysis to confirm that the mechanism suggested is the primary mechanism driving the basal melt compensation effect or acknowledge that it is not the only possible mechanism.
- P10 L221-222: I do not think you have presented sufficient evidence to demonstrate a significant ice-albedo feedback effect. There are other mechanisms that could plausibly result in a change to the sea ice mean state, particularly for simulations evaluated over decadal timescales e.g. a change in how sea ice is distributed between thickness categories, particularly since sea ice vertical growth rates are sensitive to the existing sea ice thickness, or more efficient use of available surface ocean heat content for sea ice melting. Can you directly isolate and quantify the change in sea ice mean state that can be attributed to the ice-albedo feedback mechanism here? Otherwise, you should acknowledge that the ice-albedo feedback is not the only possible mechanism that could cause a change in the mean sea ice state, and further analysis / studies are required to quantify its impact.
- P10 L223-227: Or due to non-equal distribution of sea ice volume between thickness categories? See earlier comment.
- P13 L298-300: Re following statement, 'increasing the lateral melt rate results in similar rates of heat flux from the ocean to the ice in most areas of the Antarctic, but over the smaller resulting ice-covered area (not shown)'. A map plot would be useful here to illustrate this point.
- P13-14 L310-311: Re following statement, 'here, ice-albedo feedback is not the main reason for why increasing lateral melting results in lower sea ice mean state.' I may have missed or misunderstood something here, but it is not clear to me what you propose as the mechanism driving changes in the Antarctic sea ice mean state.
- P17 L352-355 & L359-361: As discussed above, I think you need to modify these conclusions given there are plausible mechanisms other than the ice-albedo feedback to explain why increases in lateral melt change the mean sea ice state.
- P18 L387-388: I suggest you put e.g. in the list of references here, given this is a non-exhaustive list of the different FSD model developments in existence.
- General comment about paper structure: It is not obvious to me why section 4.1 and 4.2 (particularly the former) are classified as discussion sections rather than results sections. Similarly, the final three paragraphs in the conclusions section could be moved to the discussion section since they introduce new material and discussion.
- General comment about conclusions: It would be useful to have some reflection on the limits of these conclusions e.g. the limitations of using the SOM.

Technical Corrections

- P1 L11: The phrase 'well representing' here is somewhat awkward. Maybe replace well with accurately?
- P2 L36: Should be 1980s, rather than 1980's.
- P2 L38: The)'s setup of 'Josberger and Martin (1981)'s formulation' is awkward. Maybe replace with 'the formulation of Josberger and Martin (1981)'.

- Figure 2 caption: 'ncat' is not referred to or defined anywhere else in this manuscript.
- P4 L108: Maybe replace 'Lipscomb (2001) (Eq. 22)' with 'Eq. (22) in Lipscomb (2001)'.
- P5 L124: I do not think you have defined $V_{ice,n}$ in this equation.
- P6 L135: In some places you have not followed The Cryosphere journal style guide e.g. here Eq. 3 should be Eq. (3), and section 2.3 below (L148) should be Sect. 2.3. Also, Fig 2 should be Fig. 2 on L150, and Figure 1 should be Fig. 1 on P7 L179. Similar issues are present elsewhere.
- P6 L147: Should this be 'per unit volume' rather than 'per volume'?
- Figure 3 caption (and other figures): it would be helpful to clarify the number of years the results have been averaged over in the figure caption.
- P10 L230: Should 'open water efficiency' be 'open water formation efficiency'.
- P11 L280: Seasonal should be season?

References

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