The Cryosphere Discussions

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Comment on tc-2021-67

Anonymous Referee #2

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-RC2, 2021

Review of Arctic sea ice sensitivity to lateral melting representation in a coupled climate model

This paper assesses the importance of lateral melting in the context the coupled model CESM. By modifying the parameterization of lateral melting in a coupled model the authors can quantify the contribution of this process in the albedo feedback mechanism. The authors present an elegant quantification of the relative impact of lateral melt in term of efficiency at forming open water compared to the bottom melt efficiency. This is potentially interesting for climate applications via the well known albedo feedback process. I would like the authors to discuss further this impact in terms of a potential increased contribution of lateral melt throughout the 21st century. I am also a little worried that the sensitivity study proposed is not well justified (why is it ok to vary the lateral melt scale by a factor 100) and not very well constrained by observations (in situ constraints or satellite observations of FSD). I am a little worried as well that the results presented here are only representative of this specific model configuration and in particular of the prescribed mixed layer depth (this could be particularly problematic for the Southern Ocean). As is often the case with such modelling sensitivity studies the paper asks more questions than offers insights and answers. The paper introduces a lot of model experiments but they remain at a qualitative level (arbitrary r_n function, extreme sensitivity lateral melt scale factor, unclear partition between lateral and bottom melt, prescribed mixed layer). As such it could be argued that the paper is more suitable for a modelling journal such as Ocean Modelling or GMD. In short I find the authors have presented an elegant modelling study of the contribution of lateral melt to the open water formation efficiency, are clearly well versed in the workings of the model (CICE/CESM), and in the processes controlling lateral melting but do not offer a significant new model development or model constraint from observations. Provided the authors improve on some (most) of the general comments below the paper could make a useful contribution to the community. Alternatively the authors could resubmit this fine modelling study to a modelling journal.

General comments:

1) Highlight main results better in the abstract (rewrite) as it is too generic at the moment

2) Include in the introduction a review of how lateral melting vs vertical melting is currently represented in CMIP6 (Keen et al 2020)

3) You use factors 10x and 100x without much discussion as to the validity of such choices. This correspond to changing the mean floe size by a factor 10 or 100 which is consistent with spatial gradients from pack ice to MIZ. Please explain all this a bit more and why a spatially constant scaling makes sense in your view. Discuss also impact of FSD as in Tsamados, Bateson or Horvat.

4) The participation between lateral and bottom melt in CICE (CESM) is not critically reviewed in my opinion

5) The authors introduce an 'arbitrarily' category dependent lateral melt redistribution function r_n. This is qualitative and not robustly justified or quantified.

6) On a related point this lateral melting scale is a dynamical quantity as more lateral melt leads to a reduction of floe sizes which in turn leads to more lateral melt. I am not sure that you fixed scale approach captures all this complexity and positive feedback.

7) Explain role of ocean and mixed layer heat reservoir in redistributing between vertical and lateral melt. I would like to see how sensitive your results are to this. With this in mind, are the results for the SO really meaningful (there the MLD can vary a lot and reach 100s of metres - definitely not a constant 10m as in your model)

8) Can you please constrain your results by comparing to more recent observations of floe size distributions. For example is it possible to assess which of 100x or control is more realistic in terms of relative distribution between vertical and lateral melt?

Specific comments:

P1 L24 “Vertical melt processes (surface and basal) can only form open water once the ice is very thin, while lateral melt can directly form open water area regardless of ice thickness” are you aware of MOSAiC experiments planning to re-evaluate the relevance of this statement.

P3 L81 How critical is the depth of the this SOB for your results? Sensitivity? I.e. how much of the heat in the SOB would have been lost to the lower ocean?

P4 L104 where all are

P4 L110 cite Massonnet, François, et al. "On the discretization of the ice thickness distribution in the NEMO3. 6-LIM3 global ocean–sea ice model." Geoscientific Model Development 12.8 (2019): 3745-3758.

P5 L120 D=300 as a default. Please discuss this approximation and why it could not be turned into a dynamical variables.

P5 L126 in addition

Figure 2 2 orders of magnitude of changes in the lateral melt rates ‘scale’ seems very unconstrained by observations to me

P7 L164 to clarify definition of dV/dt_lat,n add these terms in eq (4)

P7 L165 remove as n already defined

P7 L169 to the average…in the control

Figure 1 I quite like the drawing but it does not represent 'key melt processes' but rather fluxes and variables of interest. Also I feel that it does not express all the quantities described in the paper as discussed in P7 L179

P7 L180 'open water forms equally in all categories' seems in contradiction with r_n -> clarify this entire last paragraph.

P9 L201 minima

P9 L209 this communicating vase issue is crucial and I am worried that there is not enough discussion on how the relative basal to lateral ratio of melt is affected by the SOB characteristics (depth value) and lack of dynamics (fixed depth)

P11 L250 what about then a 10x & distribution sensitivity run

P11 L262 you mean efficiency in terms of open water formation but other aspects might still be affected (more winter growth…) -> clarify sentence

P12 L276 so which of 100x or control is more realistic? Why not do a 1000x run or a 0.1x run as at present you do not seem to constrain these sensitivity runs at all from observations.

P12 L287 is your SOB appropriate for the SO where MLD can be much larger than 10m

P14 L310 on what basis you state this? Clarify

P16 L334 realistic as in consistent with observations? You have not discussed that much here at all.