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The Cryosphere Discussions

Interactive comment on "Impact of refreezing melt ponds on Arctic sea ice basal growth" *by* Daniela Flocco et al.

Anonymous Referee #2

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General comments: Flocco et al discuss the behavior of melt ponds during refreezing and the impacts the process has on refreezing of sea ice in autumn. The authors find that the pond refreezing process delays the onset of ice bottom growth using a 1D model. They suggest that the process is of significant importance based on upscaled GCM results that show lower basal ice growth during September and October by some 25%. The authors argue the loss of bottom growth may be significant to total ice mass balance and to brine fluxes and make some efforts to connect this effect to large scale changes. The result is a recommendation for inclusion of this process in GCM's.

This reviewer agrees with the first reviewer that these conclusions are problematic and that the paper as written does not to give the reader confidence in the methodology or establish confidence in the overall importance of the process. The first reviewer does an excellent job capturing the key issues. The paper tries hard to emphasize

importance but does not thoroughly address the question of "does this actually matter enough to bother trying to get it modeled in a GCM". The authors argue 'yes' – but as the prior reviewer points out, there are issues with building confidence in methodology. This author also feels both the impact on overall mass balance and salinity fluxes (even if correct as stated) are likely negligible to the outcome of a GCM, in the context of other errors and model variability.

This reviewer also feels the importance of the findings that may be available (i.e. their impact on overall sea ice/climate system such as through brine rejection) is not well supported and that the editorial decisions over-reach the available support. Three impacts of this process are candidates for importance, where importance is defined as having a sufficient impact on the sea ice or climate system that omitting the process would meaningfully impact GCM accuracy. (1) Mass balance of the ice - In terms of overall ice mass balance the process seems likely of negligible importance as ice surface growth (in the pond) makes up for any loss in ice bottom growth (and then some). (2) Salt fluxes/negative buoyancy sourcing - The difference in basal growth magnitude or timing might be significant for salt fluxes to the ocean, though this would need to be more carefully considered than the presentation in this paper allows. (3) Entrainment of salt within the sea ice by trapping at the pond bottom (as discussed heavily in Flocco et al., 2015) – the reviewer has extremely extensive experience on ice and has not observed the entrapped high salinity layers at pond bottoms suggested here or in Flocco et al 2015, and believes they are a fiction of the model. In reality salty melt ponds are well connected to the ocean and brine rejection will be possible during growth.

Here are a list of particular areas in the paper's logic that felt need improvement. L89-90 implementation of a C- shape salinity profile for the pond layer as in Flocco 2015 doesn't tell us what salinity you are initializing the model with – some of the ponds in Flocco 2015 are quite salty. Ponds unconnected to the ocean tend to be very low salinity, typically under 2 PSU and often under 0.5 PSU. Many ponds are connected to

the ocean - some directly through large aperatures others through porosity within the ice by summer's end – either can provide an effective means for brine drainage. As such, the trapped salinity layer which is a component of the model is not often found in nature. Further, ponds having high salinities – which do exist – have such high salinity specifically because of a connection to the underlying ocean. The isolation of high salinity water within a refreezing pocket in a melt pond is simply unrealistic. L 140 – The reviewer notes that this agreement with prior studies is all with studies using the same methods, from the same author and same group. The pond coverage and timing is not compared here to independent observations or models. In particular, as this reviewer has noted in prior evaluation of this group's work, melt pond formation in May north of 70 latitude is very rare in observations, but common in this dataset. This isn't a central component of this work, so little needs to be done. This reviewer is still trying to encourage the group to use caution in building a suite of work on shaky melt pond simulations. L148 – this conjecture could be investigated from the model output readily L 150 – 19 days of what atmospheric conditions? L 162 – reviewer is extremely skeptical of these salinity values being realistic for any significant volume of pond water. L215 Thinner ponds omitted – but these are the majority of the ponds you simulate according to Figure 4. Seems like a very selective comparison. What is the impact of the disagreeing pond behavior on the others? L 230 applying a ratio between Stage 1 and II seems sketchy. L 248-49 doesn't the unponded ice have to go through the process of establishing a temperature gradient at the end of summer too? L254 and 255 superscript km3 L256-8 Perhaps. This would be an important finding, however it is conjectural here and unquantified. Is the potential discrepancy in salt significant to something? Does it meaningfully change the state of the sea ice or climate system to miss it? L263 How do you define 'negligible'. The reviewer feels that the overestimated basal ice growth would be negligible if "its impact on the climate system realism in a GCM is negligible." Nothing presented here convinces the reviewer that this is not still the case. L346 Fig 3 – Over what ice area is this percentage? All Arctic sea ice? L363Fig 5 – simulated for where? Under what conditions/latitude. Fig 6... Trapped

pond stays liquid to -4 C. very salty.

Other Comments: There seems to be some recycling of material from Flocco et al., 2015 JGR. The abstract shares several key conclusion sentences and the findings of this paper are very similar to the conclusion of the 2015 paper. It would be helpful to differentiate this paper from that one more clearly.

The confined duration of the simulations within Sept and October only does the reader a disservice, and makes it challenging to understand the real impact of this process. For example it leaves unanswered key questions like: Does the ice 'catch up' in bottom growth later in the year (fresh ponded ice has higher thermal conductivity and lower specific/latent heat capacity)?

L 10-13 odd line returns - appears this should all be 1 paragraph L41 – increase the heat absorption in the ocean (the fate of the heat and how much is stored is not really addressed in the papers cited) L 40-43 run-on sentence. At least needs some commas. L45 – bare ice will not have reflectivity higher than 70%. Dry snow-covered ice is $\sim$85%. L48 –more up-to date references e.g. Perovich and Polashenski 2012 show even larger impacts of ponds. L50 comma – hereafter), L53 skillfully sp. L53 May-June, (comma) L237 space after 1983. L261 are $\rightarrow$is

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