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# *Interactive comment on* "A mechanism that produces dichotomy in melt pond coverage in sea ice floes" *by* Predrag Popović and Dorian S. Abbot

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Full review from anonymous reviewer, posted by editor

This paper is much improved. The reviewer appreciates the authors' careful attention to the prior comments. The authors are tackling a very interesting topic and the paper should be a sound resource to the community as late season melt pond behavior begins to be addressed better in models. There are several additional areas where the reviewer feels the paper can be strengthened. If the authors address these, the paper should become suitable for publication.

Major comments 1. Reviewer feels that the authors use of Multiyear ice dataset for comparison of a model that only tracks FYI processes is a significant problem. Authors must either change out comparison data to a FYI dataset or place clear statements



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Discussion paper

throughout the text indicating the severe limitations of comparison to field observations that amount to a Frankenstein combination of FYI (e.g. Landy Hypsographic curve) and MYI (Perovich Albedos). 2. Reviewer feels the authors miss a substantial opportunity to comment on the impact that differing hypsographic curves would have on the ice. Reviewer suggests that vertical sidewall melting would be primarily controlled by height above sea level of the area... implying that 3. The conclusion of 1.3 percent per month in a warming climate is way to shaky to be allowed in the abstract or conclusion as a strong take home point. There are numerous processes not accounted for here. Trumpeting a pond coverage trend in a warming climate based only on an idealized model of one of several pond formation processes is just too bold to pass peer review. Authors have two options on this point. 1. Produce and include estimates of uncertainty with this number, 2. Drop the specific quantification and discuss the sign of the change. 4. Conclusions need to do a better job clarifying over what conditions the general interactions between melt location and pond coverage apply. Reviewer feels the model results only show how these are true for certain pond coverage and ice thickness pathways, dependent on the key initial assumptions, such as that ice all has the same hypsographic curve. As an example the conclusion that bottom melting's impact on growth rate increases with increasing pond coverage may not hold if comparing two floes with very different pond coverage and different topography. One floe may have very flat ice and low pond coverage at the time ponds drain to sea level, while another may have higher pond coverage and greater topography. Bottom melt will impact the level ice floe more than the rougher flow because of having more ice near sea level to flood, regardless of the initial pond coverage.

Specific comments: L1 Clarify that this is all about FY ice... As the melt season progresses FIRST YEAR sea ice (delete floes) in the Arctic often become(S) permeable...

L4 Clarify that it is the surface relation to sea level that is of concern ...and correspond to regions of the sea ice WHOSE SURFACE IS (delete that are) below sea level...

L10 "relatively well" – relative to what? Can you replace this with a quantitative statement rather than this highly subjective one.

L13 Suggest replacing “demonstrates the somewhat surprising result” with simply “indicates”. I’m not surprised by this! Also see comments later about some issues with the support for this statement.

L15. “Performing a similar. . . 1.3%... per month. This statement is too sketchy for the abstract. This quantitative conclusion is not something that a reader could use as a takeaway number from this paper. Reviewer could permit discussion of the <sign> of this impact here, but if the authors would like to quantify it specifically outside the context of the section where this comes from in the discussion, then error bars will need to be included. Reviewer expects these would be very large and challenging to quantify.

L40 “Typical. . . proceeds in (delete a) fairly consistent STAGES (delete manner).” The stages are predictable, and regular. Their duration is not.

L45 You can actually be clearer here. The stage when ice. . . is typically the longest, OFTEN LONGER THAN THE TWO FIRST STAGES COMBINED.

L72 “we analyze the model RESULTS analytically” (I think its more the results than the model you analyze)

L80 “ . . . and (delete all the meltwater created is) MELTWATER CAN BE.” Need to be clear here that meltwater is not necessarily all removed, just that it can be removed.

L 82 “. . .when (delete: pond coverage drops to its summer minimum, meaning that all) the meltwater on the ice. . . “ It is not necessarily true that the coverage is it its minimum at this point, though it is likely. Better to just leave this point defined by water level and sea level relationship as the remainder of the sentence does

L 86 “ . . .which were above sea level to sink OR MELT below sea level”. . . You allow for vertical melt to cause creation of below sea level surface.

L 89 “ . . . at the surface of the ice”. Need to clarify that this is at EITHER surface, top or bottom. Suggest “. . . at the surface or bottom of the ice” for clarity.

L90 “ we will further assume. . . “ You do not follow this. New ponds can form by flooding isolated areas by freeboard loss based on your use of the hypsographic curve. This clarifying constraint applies only to your vertical melt enhancement around pond perimeters. If you were to change the vertical melt enhancement to be based on surface height above freeboard, which is probably its strongest dependence, you could remove this weakness altogether and freely permit new ponds to form in low topography areas by both freeboard loss and accelerated vertical melt. Reviewr strongly suggests you consider doing this – it will make your model much more robust.

L 99 “. . . when the ice at that point melts AT THE SURFACE” – must clarify that this is only surface melt impacted.

L107 “. . . but up to its flexural wavelength. . . “ please provide order of magnitude estimate of this and compare to melt pond size (e.g. 10 m) to show whether this assumption is supported.

L114 “. . . ponds can grow OR SHRINK as a result of freeboard CHANGES (delete sinking).” The freeboard changes work both ways, under certain circumstances L116 “Both rigid. . . and local. . . contribute to. . . sinking” This is inaccurate. Mass loss above sea level (local melting) contributes to freeboard rising in a relative sense.

L121 “ assume no new ponds can form” You should clarify that this assumption only applies to this mode of growth and not to freeboard sinking as parameterized. As stated above strongly suggest making vertical sidewall melting actually based on a reduced albedo as a function of freeboard, which removes the need for this constraint altogether.

L128 “. . .not the horizontal EROSION (delete motion)”

L 132 “i) it is LIKELY near sea level”

L 139 “. . . pond perimeter melts slowly (delete enough) RELATIVE TO THAT FURTHER FROM THE PERIMETER” the rate is relative, not absolute and this wording was too qualitative anyhow.

L 146 How is mass above sea level determined? You have only height above sea level in the Hypsographic curve. You must use a density here as well. What is this density? (it should be lower than the density of ice below sea level)

L174-5 This parameterization entirely neglects light transmission, and assumes all the change in albedo is seen by the surface. In actuality much of the difference in energy ends up being in short wavelengths deposited deeper in the ice. This would be a substantial extra complication. Reviewer would be OK if you address this simply by acknowledging that the assumption all extra energy of a lower albedo is not actually deposited at the surface and that this is a first order approximation.

L 202 “. . . contribute to pond (delete growth) AREA CHANGE. (delete Since we assume new pons cannot form)” This constraint only applies to vertical sidewall melting, not to freeboard loss.

L211 “needs to equal one” This means that bare ice far away from the ponds must be greater than one if ice near ponds is less than one. The discussion here of some area being less than one, the rest = 1, and the overall average = 1 is not really sensible for any reasonably finite area. The reviewer feels the low albedo area subject to vertical sidewall melting is actually finite and of several to tens of percent of the surface. Therefore some adjustments are needed to this.

L225 You clarify this below on line 228, but suggest including here. . . “ the average freeboard height OF THE BARE ICE FRACTION, h”

L240 “ not important” This is only true so long s ponds GROW and k>1. Need to add some clarification of this.

L247 “ we believe that such a statistical description. . . should be general. . . across different ice types. . ., however" This statement is not supportable. At a minimum it must be deleted.

Reviewer thinks that the authors are sweeping what may be the single most important control over this process under the rug. Reviewer respects that authors many not want to tackle this now and leaves the option to authors as to whether to address the possibility of different hypsographic curves having impacts. Reviewer feels changing hypsographic curves would exert considerable influence over how ponds evolve late in the melt season as well as their overall coverage. Using this model to comment on this would be a very powerful expansion of this paper. Also, Hypsographic curves evolve during the melt season with ponded and unponded ice increasingly creating a bimodal distribution. This would change the relative impacts of the growth mechanisms. . . vertical sidewall growth may 'run out' of area to impact, for example. Since reviewer has revealed their identity to assist with review completion, reviewer is available to discuss this idea if authors choose to pursue it.

L 279 ".. only changes close to the pond boundary. . ." Actually this isn't quite true. Changes near the pond boundary will also impact overall freeboard, resulting in an average reduction of hypsographic curve height across all heights.

L283 " . . . pond perimeter. . . is a function of pond fraction" Hmm. This is not the primary control. See Melt pond fractal dimension paper that came out of Ken Golden's group. If authors switch to treating vertical sidewall melting to be based on freeboard level as suggested above authors could avoid dealing with the significant complications in the relationship between pond area and perimeter.

L334 "decreases as the ponds deepen". This is only true by correlation, not causation. More accurately albedo decreases as the thickness of the underlying ice decreases (also as any snow ice on the surface is removed).

There are substantial issues with the next sentence. "We assume. . . " The difference in albedo of ponds caused by differences in radiative absorption in the water is negligible compared to the impact of pond bottom albedo caused by changing ice thickness. This incorrect parameterization of pond albedo calls into question your conclusion about the small effect of pond albedo on pond evolution. Reviewer believes its likely this small effect will remain, but this parameterization of pond albedo is not supportable and must be revised.

L350 -360

Reviewer has some concerns about the use of this data, which is from multiyear ice, for a model exercise which is really only valid on FYI, where permeability becomes very large (MY ice ponds do not all drain to sea level). These ponds were not all permeable at the time they are discussed. The albedos here are high for first year ice and melt rates are low for FYI. In a way, the authors have created a sort of Frankenstein ice by combining FYI topography from Landy and MYI optical properties from Perovich. Would it be possible to do better using data from Polashenski et al 2012, Polashenski and Perovich 2012; and Landy et al 2014? There may also be some useful data from some of Marcel Nicolaus's papers.

L370 Suggest using Timco et al '96 to refine these densities. Seawater should be more like 1025 kg/m3 and sea ice more like 920 kg/m3 below sea level and 0.87 above (or lower if surface scattering layer is included).

L387 'grows ponds. . . melting, and freeboard sinking" Not true. The melting of bare ice grows ponds through vertical sidewall melting but SHRINKS ponds through freeboard rising.

L 389 Delete ". . . whereas freeboard sinking due to bare ice melting is independent of pond fraction" This is not accurate. The amount of freeboard sinking depends on how much area bare ice melting has to impact, which is dependent on pond fraction.

L 397 Might state why shrinking ponds can't be represented well. Unrealistic pond bottom topography in hypsographic curve. ' L 400 ice thickness of 2 m is very thick

FYI. These experiments would be better run on 1.5 m thick ice as a more typical FYI cover.

L403 Delete "in"

L 404 extra comma

L 410 . . . be ignored, AS LONG AS THE HYPSOGRAPHIC CURVE INCLUDES SIGNIFICANT LOW TOPOGRAPHY AREAS"

L412 pond growth (ADD COMMA)

L419-425. . . this is Multiyear ice. SHEBA ponds are not first year ice, nor were they all permeable during the times the authors use for comparison. This poses considerable issues for this comparison. Suggest finding a FYI comparison in literature. If not, authors must acknowledge specifically and clearly that this is MYI, and not the same as what the model is set up for (FYI).

L428-432. This is all pretty optimistic. There is likely a significant disagreement between the hypsographic curve used by the authors and that of this MY ice. Must definitely state the possibility of such given the Frankenstein nature of this dataset.

Also " This difference in topography could explain the remaining discrepancy. . ." Yes, but its unlikely that it would completely explain it and many other factors are at play. Change to "This difference in topography could contribute to the remaining. . .

L445, add clarification. . . this is not a universal property of ice thickness, since changes in topography that may be related to ice thickness could impact. " meaning that. . . on thicker ice OF THE SAME SURFACE HYPSOGRAPHIC CURVE."

L 469 "Using . . . we get. . ." This is also only true at a specific ice thickness and hypsographic curve. These should be specified as well to clarify that this is not a general result for all ice at this bottom melt flux.

L487-88 " contributes to roughly. . . both to. . . melting to. . ." Multiple split infinitive. . .

way hard to read.

490 – here is a great example of where the relative impact stated would be highly dependent on Hypsographic curve. In this particular case, it would seem also that vertical sidewall melting appears to be unimportant, the freeboard mechanism dominates. This additional sentence trying to justify vertical sidewall melting importance seems poorly supported. Reviewer thinks that vertical sidewall melting importance in some part of the regime would be clarified by use of differing hypsographic curbes.

L510 –this is an accurate conclusion and likely will stand after the revision, but for right now it is built on a very poor pond albedo parameterization which should be revised.

L525-540 This is an interesting model exercise. Reviewer will allow it here. The quantitative result, however, cannot be discussed outside this section (i.e. abstract and conclusion) unless the authors are able to produce reasonable estimates of the uncertainty. Reviewer feels the result is simply too sketchy for placing it in these areas without error estimates and risking a member of the modeling community pick it up as fact. Reviewer instead suggest discussion what sign these results indicate the melt pond change should have in a changing climate and highlighting this in the abstract and conclusion.

L 542 Start of section 5.1 is a non-sequitur from previous section, suggest a couple sentences to introduce why you are talking about this.

Section 5.1 is problematic in topic based on conclusions. Reviewer thinks it would be more relevant after treatment of differing hypsographic curves shows that sometimes vertical sidewall melt is important. Based on current results, which show dominance of freeboard loss. it struck the reviewer as disingenuous to discuss all these mechanisms and then admit at the end that the available evidence suggests none are important, only as an aside in the last sentence of the section. The discussion should focus on these rather than relict bits from prior version. An analysis of sensitivity to ice topography/roughnes/hypsographic curve would be very highly valuable to the community here instead. The summary of the mechanisms from literature has some serious issues (detailed below). Authors are likely missing the strongest mechanism here as well. Albedo is strongly dependent on freeboard below about 20cm. . . therefore low topographic height areas will have lower albedo and will melt faster.

line 545 - thin layer of water... due to multiple light reflections. Reviewer thinks authors mean refractions and authors should clarify that the refraction occurs more strongly (and hence more backscatter occurs) when the ice is not covered by liquid and air occupies pore/intercrystalline space.

paragraph around line 550. Yes, this generally describes grain ripening, but this process is not significant for large scale 'jagged' features and 'smoothing' ice. It is a microstructural process, and one unlikely to be much more prevalent near ponds than away from them, therefore not a good candidate as described.

c- 557 - needs a reference, or replace 'often' with something softer like 'sometimes' and a personal communication.

L565 – Up to authors, but could be helpful to the reader to clarify why cloudy conditions have lesser effect (lower shortwave and more of surface radiative balance controlled by spatially uniform longwave absorption).

L 570 – 583. These mechanisms are possible, but not likely to be important. Reviewer suggests deleting and saving the words.

L575 – Frankly this seems unlikely

L580 - The ridge accelerated melt is largely fictitious. For every sun-ward side, there is a shaded side. Reviewer suggests deleting this or at least clarifying sunny/shady balance.

L599 – It is important to note that lateral melt does not depend on ice thickness. . . or . . . IN THIS MODEL, THOUGH IT MAY DEPEND ON THESE IN NATURE TO SOME DEGREE. L 655 – C is the likely and observationally supported cause here. See estimates of porosity in Polashenski et al., 2012. Suggest highlighting this option as more likely than the others. L667 through section 5.4 This section is fine, but would be stronger if it included discussion of how changes in topography (hypsographic curve) would impact pond growth. In this case, the stop growing case is dependent on very steep/vertical sidewalls which are common in rough ice. L709 " we also find. . ." this statement is true but authors need to fix up the pond albedo parameterization and make sure it holds L710 "Using the same.. " delete sentence, add error bars, or change to discussion of sign only L712 " The four modes. . . " This statement needs qualification to prevent over interpretation or mis interpretation, specifically indicating what terms (average initial pond coverage/thickness etc) are being held constant and which are changing. The next comment will help clarify the issue. L 713 " growth rate due to ponded ice and ice bottom melting increases with pond coverage" Reviewer thinks the results really indicate that growth rate due to these mechanisms increases with ice thinning. Starting at the same thickness with different pond coverage and the same average freeboard, bottom melting should case greater growth rate on lower pond coverage floes, because more of the floe surface will be closer to sea level.

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