Review of Popovic and Abbot 2016 - for The Cryosphere.

Considerable attention is currently being devoted to understanding the evolution of melt ponds on sea ice surface. The ponds are highly important to summer energy balance of the sea ice cover. Popovic and Abbot provide an intriguing discussion of how freeboard loss due to ice melt at different interfaces can influence the growth or decline of melt pond areal coverage over the duration of the melt season. Within the paper the authors discuss the impact of melt at different locations on the buoyancy of the ice and the ramifications of these impacts on pond coverage, assuming ponds have already drained to sea level. The authors create a mathematical representation of the processes discussed and use this to explore several possible scenarios for melt, demonstrating the possibility that this mechanism could leaded to a dichotomy of melt ponding behavior, with ponds above some critical coverage growing, and below some coverage either shrinking or, more likely, stagnating in areal coverage.

The reviewer notes and appreciates that the paper is well written; both of high prose quality and clearly organized to convey the concepts postulated and mechanisms discovered. The reviewer finds, however, that there are several very serious issues with the evidence presented and its ability to explain the dichotomy of pond coverage evolution. The reviewer feels there is more than ample evidence in the literature to disprove this proposed mechanism and notes that the authors were aware of this evidence, at several points discuss this evidence, and then follow the discussion by ignoring the contradicting evidence. As such, this reviewer feels the authors have presented a mechanism that does not explain the pond dichotomy because of a mismatch in timing and because the mechanism relies on several assumptions very likely to be false. The reviewer highlights these several major scientific flaws here and then discusses line by line details below. The authors do, however, have the framework for evaluating and discussing the impacts of freeboard on pond coverage. The reviewer advises the authors re-write the paper entirely to focus on what these relationships appear to be, without trying to coax them into showing a particular result, such as explanation of dichotomous behavior.

Major scientific issues:

1. **The necessary assumption required for hydrostatics to produce a dichotomous behavior is that ice immediately surrounding ponds melts more slowly than other ice. This is extremely unlikely to be true.** Several potential reasons for slower melt at pond perimeter are presented by the authors. Some of these are logical (e.g. a.) while others (e.g. c.) ignore that the mechanism is just as likely to cause enhanced melt as slowed melt. (see detailed comments on lines 318-355) The mechanisms discussed are very likely overridden by much stronger mechanisms for accelerated melt at pond perimeters. In particular, lower albedo is observed in low-freeboard situations where a full surface scattering layer cannot emerge above freeboard (common near freeboard level at pond perimeters) and lateral heat transfer from pond water lapping against the wall of the pond has been shown to encourage lateral melt at the pond boundaries (see SHEBA pond related publications by Freitag, Eicken, and Perovich). Extensive lidar studies by Polashenski et al. and Landy et al. also indicate relatively little melt rate preference, and decidedly do not support a hypothesis of slower melt rates on the perimeter of ponds (as the author notes and then dismisses without cause in lines 395-397). These studies show freeboard change overall does dominating later season pond growth.
2. **Topography of the ice is neglected throughout.** Topography of the ice is intimately connected to bare ice area by hydrostatics. Given two floes of the same mean thickness, both ponded at sea level, the one with the lower bare ice area must necessarily have higher mean bare ice height to maintain the same volume of ice above sea level. This means the bare ice in the high pond coverage case is more resistant to flooding or melt. This is a powerful stabilizing feature that resists pond growth when pond area is high. The neglect of this inherent feedback leads to the dichotomy of behavior.

3. **<u>The dichotomy in system state between high pond coverage and almost no pond coverage presented by Perovich et al., and further discussed by Webster et al., 2015 does not match the proposed mechanism in timing.</u>** Some observations indicate the dichotomy occurs very early, in the melt season, (i.e. ponds never form in the first place) while others suggest the dichotomy is initiated at the time ponds drain to sea level. The mechanisms proposed here operate after the drainage of the ponds to sea level and take considerable time to produce effects. By timing, therefore, it appears the mechanisms proposed here are not well situated to create the observed ponding dichotomy.
4. **<u>The reviewer is not aware of any observations that indicate pond area decreasing after ponds have drained to sea level.</u>** The available observations appear to indicate that any area ponded and at sea level will remain ponded for the rest of the year (e.g. Polashenski et al., 2012, Landy et al., 2014). All observations of pond area reduction that this reviewer is aware of are due to drainage of above sea level ponds. This mechanism, as described by the 1d model, creates declines in pond coverage after ponds have drained to sea level – a behavior that does not appear to be documented anywhere in the literature. The mechanism as described by the 2d model does not create declines in pond coverage, but if this is the case, it does not explain the creation of low pond coverage ice, just the its maintenance.

The reviewer also finds that a few other processes are important enough in controlling hydrostatics that they probably cannot be neglected and should at least be discussed even if they cannot be incorporated into the model.

a. Density change both above and below sea level caused by internal melt – a very common mode of melt accounting for more than 1/3 of total ice melt in most cases.
b. Sub-ice freshwater lenses which commonly form in the Arctic reducing ice buoyancy.
c. Timing of surface melt and bottom melt is not coincident. Surface melt is often occurring for a considerable time prior to initiation of bottom melt.

The reviewer feels that correcting these assumptions will show that the ability of this mechanism to produce dichotomous behavior is removed. The paper will have to be re-written to reflect this reality. It appears to this reviewer that the dichotomy is established prior to ponds draining to sea level anyhow, so perhaps the authors are trying to make this mechanism explain something it should not explain. All is not lost though! An honest evaluation of what freeboard interactions should do to pond coverage would likely still be useful to the modeling community. It may be that freeboard control of pond coverage can explain some pond evolution after ponds have drained to freeboard even if it cannot explain the dichotomy.

<u>Line by Line comments:</u>

Line 28: Landy et al., 2015 did not provide any evidence to demonstrate that pond coverage should increase in a warming climate. If anything the colder year had more pond coverage. This and several other instances throughout the paper seem to indicate a style of citation this reviewer does not support. Specifically, the authors are citing an offhand statement within the prior work's introduction, which is not specifically supported by the prior work's data or analysis. Proper citation should only reference the content of a work where that work has actually demonstrated evidence that supports the assertion being referenced. If the prior work has only conjectured that a trend may exist, it would be proper to discuss the expected future state of melt ponds as such.

Line 30: "This should cause… ecosystem." This is only true where light, rather than nutrients, limit productivity. Nutrient limitation is the norm over most of the Arctic basin, so at very least a caveat clause should be inserted to this effect.

Line 47: The reviewer notes that Perovich et al date the photo JULY 15 – not June 15. This is an important correction. If June is actually correct, the mechanisms being discussed would not have had time (weeks to months as discussed later) to operate at this stage in the melt season. The dichotomy in system state would have occurred very early, and potentially prior to freeboard impacts. Though the reviewer believes this is a simply typo, the timing of when the dichotomy first emerges is important for evaluating whether this mechanism is an appropriate explanation for the behavior. Webster et al., 2015 suggest the dichotomy occurs much sooner, perhaps pointing to other causes.

Line 59 – consider changing ‘relevance’ to ‘existence’.

Line 64, 67, and throughout: consider changing ‘pondedness’ to ‘ponding’ which would be more consistent with terminology in prior works.

Line 117 – The reviewer finds this first condition implausible and unsupported by existing evidence. See lidar studies by Polashenski et al, and or Landy et al. Data appears to be available from both authors if the figures are inadequate for fully resolving whether this is likely.

Line 128 -135 – the authors consider only melt at the interfaces. This ignores that sea ice experiences considerable internal melt. Internal melt causes substantial changes in ice density both above and below freeboard. This is likely a very important mechanism to add to consideration. Maybe conditions which favor internal melt above sea level (e.g. dirty ice) dis-favor ponding?

Line 128 – consider adding a caveat that all this discussion is only true if the ponds are hydraulically connected to the ocean. The current statement makes it seem like this is ALWAYS the case.

Line 132-133 –“The larger the pond fraction is, the larger their tendency to grow will be and the smaller ther tendency to shrink will be.” This simplistic description of the implications of pond area on the ease of additional flooding (due to hydrostatics) ignores the necessary other implication of large pond area. Assume two floes have the same thickness, but one has greater pond area than the other. The bare ice on the floe with greater pond area must be topographically steeper and/or higher as required to keep the floe in hydrostatic equilibrium. Thus it is more resistant to encroachment, providing a very strong stabilizing effect not included in the model. The one with greater pond area is here asserted to have much easier potential for flooding additional area, but considering the topography of the remaining bare ice, it must necessarily become extremely resistant to pond growth at low bare ice area.

Line 195-6: It would be helpful if the authors consider the practical constraints on this number – while it is mathematically true that it could be greater than or less than 1, it is likely that thought about the system could rule out some values.

Line 194-203 – the assumption of constant slope used to turn the model into a ‘one dimensional’ approximation is invalid for a full range of pond coverage. In order to satisfy hydrostatics, the remaining bare ice in high pond coverage scenarios must necessarily have higher height and likely greater slope at the ice-pond interfaces. This invalid assumption results in the dichotomy of behavior.

Line 243 –This is not an entirely accurate statement, and an odd citation. The major thesis of these and other recent papers by Perovich et al is that bottom melt can be much greater than surface melt under certain circumstances. These Perovich papers also include considerable information about the time offset between surface and bottom melt which may be quite useful in thinking about how top vs. bottom melt impacts the hydrostatics at different times in the year.

Line 258 – these fluxes are somewhat outdated and based on heavy multiyear ice. Consider updating with albedo information more recently published, such as Perovich and Polashenski 2012 or papers by Nicolaus.

Line 265 – the slope of the pond boundaries is shown and briefly discussed by Landy et al, 2014 and Polashenski et al. 2012. These slope values appear to be steeper than most of the FYI boundaries they observe.

Line 270-272 – "a potential…" Discarding contrary evidence in this way is not supportable. A disagreement between the model and established evidence is not necessarily due to just the crudeness of the model – it could be evidence that the model is wrong and that the mechanisms are not the predominate ones! A more appropriate statement would be "Flooding due to freeboard loss is observed by Landy and Polashenski in a manner comparable to what we model. The timescale of the flooding observed, however, is much shorter than our model suggests (days vs. weeks to months). This discrepancy could be due to the crudeness of our model or may indicate that other mechanisms not considered are also important.

Line 280 – A 'considerable' number may be too strong a descriptor. The evidence for the non-ponding floes in the literature is sporadic.

Line 270-300 – this discussion appears to ignore that 'initial' pond coverage as discussed in this paper is actually the pond coverage at the time that ponds drain to sea level (a later stage of pond development) This discussion would be strengthened by considering the impacts of large or small pond coverage during the time period when ponds are above sea level as well. The ponding during the above-sea-level stage exerts considerable influence over later pond coverage by creating the below sea level depressions which will remain ponded later in the melt season (see Polashenski and Landy). It seems likely that low coverage during this time would create few below-sea-level depressions, moderate coverage would create many, and very very high coverage would again create few because the ice surface would melt evenly, creating no pond basins once drainage occurred. Considering the impacts of this early season ponding behavior might suggest a stabilizing behavior on ponding? Maybe this would explain why most floes actually show ponding in a relatively narrow range.

Line 306 – Albedo set as a function of pond depth. This is OK, but the authors should be aware that this is a correlative description, not based on the mechanism controlling the radiative transfer. In other words, there is a modest correlation between pond depth and albedo, but physically speaking there is no relationship. Albedo is controlled by the properties of the ice underlying the pond, and it just so happens that thinner, clearer ice has a tendency to be under deeper ponds. Also, bare ice albedo does show considerable variation when less than about 0.5 m thick, or when it has limited freeboard (i.e. near a pond edge).

Line 306-307 – "A narrow lip around the pond edge had a slightly higher albedo than bare ice." The reviewer is not aware of any supporting evidence for this, and is aware of many observations that suggest albedo a low freeboard pond-edge sites is lower than elsewhere on the bare ice. The authors must acknowledge that this assumption runs contrary to observation or present evidence that it does not. In acknowledging that this assumption is implausible, the authors will find the ability of this mechanism to produce dichotomous behavior is removed. The paper will have to be re-written to reflect this reality. It may be that freeboard control of pond coverage can explain some pond evolution after ponds have drained to freeboard even if it cannot explain the dichotomy. It appears to this reviewer that the dichotomy is established prior to ponds draining to sea level anyhow, so perhaps the authors are trying to make this mechanism explain something it should not explain. An honest evaluation of what freeboard interactions should do to pond coverage would likely still be useful to the modeling community.

312-317 – the change in behavior for the 2d model discussed here is very important. Once ponds reach sea level, the reviewer is not aware of any observations that show ponds shrinking in area. As stated, "the points on the bottom of the ponds melt faster than bare ice, and therefore cannot move upwards" This should mostly preclude pond shrinkage after ponds reach sea level. If this is the case, the dichotomy in pond coverage is produced by some other mechanism operating prior to the time ponds reach sea level. The mechanism operating after ponds reach sea level does not have the opportunity to reduce a moderate pond coverage to a low or zero pond coverage, at best it can maintain the status quo.

Lines 318 – 355 The reviewer appreciates these efforts to support the assumption that pond edges melt more slowly than other bare ice but finds them unconvincing. A. This mechanism may be valid. B. In practice this reviewer has observed the opposite, with bands of dark algae being washed up on the perimeter of many advanced ponds. Concentrations of light absorbing aerosols (ie. Black carbon) tend to be too small to make much difference. Sediment laden ice may behave as discussed here. C. The 'shadowing' effect of a slope would also be a 'higher incident radiation' effect on a slope with the opposite aspect (i.e. facing the sun). Such a slope would melt more rapidly. With random slope aspects, the effects of this are a wash.

The authors also fail to consider low albedo at pond edges due to limited surface scattering layer above freeboard, lateral heat transfer from pond water, mechanical action of pond waves, etc. The reviewer feels this one-sided examination results in an erroneous conclusion that slower than normal melt rates at the pond edge are plausible, even likely. The reviewer feels they are plausible only under very narrow circumstances and very unlikely to be the norm.

Line 355 – considering only hydrostatics, the slope/topography must be a function of pond area.

Line 360 – 363 – " where the effect of increased light absorption by the water is minimal" The authors provide a perfectly valid reference which establishes that this conjecture is false. It is not true and must therefore be removed.

Line 365 – "However, we believe that even stopping pond growth might be enough to explain the observations". This statement strongly suggests that the mechanism discussed here does not cause the dichotomy in pond coverage. At best it maintains the dichotomy introduced by some mechanism operating earlier in the melt season, prior to pond drainage to sea level.

Line 371 – more recent publications focus on the importance of light transmitted through ponds in areas of high pond coverage. Frey et al., 2011; Perovich and Polashenski 2012.

Line 395-397 – The authors repeatedly ignore evidence that contradicts their theory to the considerable frustration and exasperation of this reviewer. This is poor scientific practice. Polashenski et al., 2012 finds similar behavior to that of Landy et al., meaning that both available case studies do not support a critical aspect of the author's mechanism. Perhaps all this contrary evidence means the mechanism needs to be reconsidered?!

Lines 398 – 404 : This is entirely speculative and insufficiently addressed. Recommend removing entirely.