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The Cryosphere Discussions

Interactive comment on "Observations of Sea Ice Melt from Operation IceBridge Imagery" *by* Nicholas C. Wright et al.

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*Author's replies are denoted with a > before the paragraph. PDF with track changes included as supplement.

The paper describes an analysis of surface conditions of Arctic sea ice in summer. The images are processed from a previously developed algorithm, which is approved upon and well-described, and all methods and output are made publicly available. The paper is very well-written, logically organized, and the figures are illustrated clearly. More details need filling in for parts of the methods, which should be straight-forward to address. The largest concerns I have are the testing of the hypothesis that melt pond coverage over first-year ice is higher than that of multiyear ice and the two pathways of

melt pond evolution suggested for first-year ice. These concerns can be remedied by reconsidering the argument and taking into account the following points:

>Thank you for your review of our manuscript. You have provided great insights that have helped to improve this work. We have made efforts in the revised document to better discuss the time dependent nature of melt evolution, and to reassess how we can investigate these hypotheses with the snapshots provided by our dataset.

Melt pond evolution is variant in nature, particularly over first-year sea ice. Operation IceBridge sampled melt ponds at different stages of melt given the long covered. To assume all pond formation and evolution progressed the same, for ex- ample assuming all ponds had drained (as in the discussion), is a stretch even within the same survey line. By sampling over such large, regional areas, these surveys are sampling different states of melt pond evolution.

>This is true and may actually be a point in favor of assessing regional mean melt pond fraction. While the flight lines are temporally static, their long spatial footprint means, as you point out, that we are sampling ice in many different states of pond evolution. Given that the data samples a range of ice states the fact that we did not observe any statistically significant difference in the mean melt pond fraction between ice types is suggestive. We concede that this investigation alone is not sufficient to prove or disprove the hypothesis that FYI has higher MPF than MYI, but believe these observations are an important addition to that discussion.

The bimodal pathway argument of pond evolution for FYI is a gross oversimplification. While it is an interesting idea to consider, the argument that FYI is either pond-free or heavily ponded during summer is weakly supported. Melt pond coverage on first-year ice ranges from no ponds to heavily-ponded with everything in between based on available data.

>We agree with your assessment that the bimodal pathway is an oversimplification. Early looks at this dataset led credence to this hypothesis, so we set out to test it more

formally. Our results here show that the bimodal pathway is not supported, as you point out, and our intent is to show this. We have therefore reworked several sections throughout the manuscript to be clearer on this point. We want to convey the idea that FYI is more variable than MYI, exhibiting all states from low to high coverage, but that there is not a bimodal path as initially posited.

>Note the last sentence of the introduction: “This new analysis reveals that FYI pond coverage indeed exhibits both pathways, but that there is not a strict duality – FYI pond coverage appears to occupy all states across the near-zero to high coverage space.”

Please see the following suggestions for further improvements: L49. Relatively calm Arctic. Calm relative to what? The Arctic seas are dynamic.

>Changed this sentence to be clearer. Here we are trying to establish the well observed predominance of flat topography on FYI and not get lost in the details about ice growth mechanisms.

L57-60. The introduction would benefit from more description about melt pond evolution. One aspect that’s missing is the transitory coverage of melt ponds with melt. At one stage, FYI melt ponds may have lower coverage than MYI melt ponds. At a later stage of melt, the same FYI melt ponds may have greater coverage than MYI melt ponds. Pond coverage can change substantially depending on the ice state and progression of melt.

>We have added a discussion on the four stages of melt pond evolution. We have also added additional details to this section that looks at previous author’s evidence for FYI with low pond coverage.

L67-68. I recommend tweaking the language here. While the results do show low and high coverage of melt ponds on FYI, which is a valuable finding, the results do not directly link together melt pond coverage and the processes posed in Polashenski et al. 2017.

>We have added this qualification to the introduction: “While the OIB image dataset provides large spatial coverage over long flight transects, the lack of temporal coverage makes it impossible to directly link these snapshots of pond coverage to any specific pond evolution process.”

L77. No flights took place during melt or freeze onset transitional phases. How was this determined?

>This categorization was determined from established knowledge on when melt onset and melt pond formation typically begins. The only summer flights were in late July, well into melting conditions everywhere in the Arctic during 2016 and 17. We have added additional details referencing passive microwave derived melt onset dates to help with this categorization.

L116-117. Were there specific cases of high-quality imagery discarded using this method? It’s worth mentioning in the text in case there are any biases worth considering.

>We did not encounter this issue. This flagging system is conservative and is more likely to not flag problematic images than it is to flag good ones. This is the reason for having to supplement the flagging with manual inspection.

L131-132. Are these limits subjective to each image or is a standard value applied all? How were the limits determined?

>These limits are standard, but only applied to select images. The limit for the white reference value is 200. These limits are only applied to images that do not contain both ice and ocean, which is determined by the number of peaks in the intensity histogram and the dynamic range of the image (the difference between the darkest peak and the brightest peak). We have added these details to the text.

L134. Is a clear, binary division true for flights where freezing and recent snowfall took place?

>Yes. While we agree there is much variability in sea ice conditions – specifically that periodic freezing and snowfall events often occur in summer months – our intent here is to separate the obviously different ice conditions between March/April (prior to melt pond formation) and those of late July (after melt pond formation). As this division is solely because of melt pond detection, we feel comfortable separating the flights into "expect melt ponds to be present" and "expect no melt ponds".

L137-140. Is there an option for using melt pond and shadow detection in the algorithm on late spring or early summer images when both conditions are present? It would be worth noting this in text here.

>The algorithm allows for this, but this was not done for the dataset described here. A new training dataset could be produced to incorporate both melt and shadow surface classifications, or even other surfaces entirely. We have added some text explaining this flexibility of the OSSP code.

L144/Section 2.3. What new information does the number of pond-free areas provide that the areal ice fraction doesn't? It would be helpful to discuss this in a sentence or two here. For one, the distribution of pond-free ice has implications for disparate surface melt rates and the new pond-free metric would seemingly give more information in this respect.

>You are exactly right, the primary benefit here is the information it provides on the spatial distribution of melt ponds. We see a difference in this metric between certain types of FYI and MYI even if the total MPF is the equivalent between them. This is because on MYI the ponds are evenly distributed across the surface (few pond-free zones) while on FYI the ponds can be clumped in areas of high pond fraction with other regions pond-free. This metric also provides insight on different types of FYI – FYI that has many pond free areas is experiencing some difference in melt evolution than FYI with well distributed ponds.

L148/L152. 15 m and 27.5 m values are specific. How were they chosen?

>These values are misleadingly specific but were chosen to be roughly 2x and 4x the mean caliper diameter of melt ponds. We have changed the values to be 12m and 25m for clarity, rerun the analysis (results were the same) and added our justification for the threshold to the text and a citation for the mean caliper diameter value.

L170. What is meant by targeted processing?

>Here we meant tailoring a training dataset to process a specific subset of images, rather than one that performs well across a large variety of input images. We have changed the wording here to make this clearer.

L176. What are the results exactly? Are they segmented images or simply surface fractions of all images? Please clarify here.

>The results are classified images – where each image pixel has been given a value based on its classified state. These can then be readily converted into simple surface fraction numbers.

L177. Please define melt pond fraction. Is it the areal fraction of the image scene or of the sea ice? How are melted-through ponds within an ice floe classified?

>MPF is a fraction of the ice area, not image area. We have added a sentence here clarifying how melt pond fraction is determined. Melted through ponds are classified as open water following from the arguments in Wright and Polashenski, 2018. In short – we approach this from a solar radiation energy balance perspective where melted through ponds are more similar to ocean in their radiative properties. Submerged ice is classified as "melt pond" for the same reason.

L177-178. Why were images with 70% ocean area discarded? Melt pond fractions in these images would be useful information.

>A single IceBridge image typically only covers 600x400m. If 70% of this is ocean, then melt pond fractions calculated from this small area are very easily skewed by large ponds (this area is well below the "aggregate scale"). Note that the images are

still processed, we just don't show the pond fraction in this plot. Even full images have a small enough area for the melt pond fraction to be skewed by large melt ponds, as shown by the orange dots in Figure 5.

L179. What is meant by low source image quality? Does this mean that there were images that had low light, were hazy, that the automation didn't catch before? If so, it would be helpful to state how many images (the fraction of the total) the automation removed. This can tell us how much work the automation saves us from doing and approximately how much work is left to do using this method.

>Yes – the manually removed images were ones with clouds/haze that were not detected by the automated system. We have added the percent caught automatically versus manually.

L180. Not enough to do what? Do the authors mean that there was usable imagery from that flight?

>There were not enough clear images to justify the effort needed to process and filter the results. A statistically relevant sample would not have been created with the small number of usable images.

L189-190. It would be useful to see the equivalent segmented image of 6c as an additional panel to the figure.

>We assume here you mean the final labelled image? Image segmentation is a specific term to describe an intermediate step of our algorithm. We have added a many of the images presented in the text as classified images in a supplemental figure.

L196-198. What's the error associated with the ice type classification? How was second-year ice classified?

>Second year ice would fall into the multiyear ice category, though it depends on the estimated surface roughness. These delineations are visually based, so the separation is between flat and undeformed ice versus textured and aged ice rather than a definitive

knowledge of the ice age.

L214-215. How were the ice types distributed along the surveys? Were FYI and MYI well-mixed or was one ice type located predominantly north, east, etc.? It'd be helpful to note their distribution here.

>For flights that observed both ice types in the Beaufort/Chukchi regions there were pockets of MYI in the northern regions of a predominantly FYI pack. Otherwise the flights were only a single ice type (using our >90% estimation). We have included this information in the text.

L217-219. The first sentence needs more description. Work by Eicken et al. 2002 and Webster et al. 2015 demonstrated the same result, but what this analysis shows is that it can happen on a regional scale rather than a local scale, and that's important. The second sentence can also be expanded on. Several previous studies showed pond evolution between FYI and MYI differ. What's new with this study is the link to the large-scale variability in pond coverage. For example, one could hypothesize that there should be less spatial variability in MYI pond coverage on a regional scale because it's less variable in time relative to pond evolution on FYI. These results support that hypothesis.

>These are good insights, and we have reworked this section to better reflect what is new in this study and what has been previously observed. We have also added additional content to the discussion section to better address these concerns.

L231-232. This sentence is unclear.

>We have rewritten this sentence.

L241. How was "most" defined? Was this 51% of the ice area or more than 10 times?

>Changed to be ". . . that can be expected on more than half of the ice".

L243/Figure 10. This is a nice result. I was hoping to see the equivalent segmented

image. It'd be worthwhile to include this either in the main text or as supplementary information.

>We have included this as a supplemental figure.

L250-253. Is this shorefast ice? It's worth stating so if it is, as it may be typical for shorefast FYI in this region.

>Yes, this ice is likely shorefast ice north of Ellesmere Island. We have changed this description: "(e) Shorefast level ice in the Lincoln sea. Ponds have started to drain already, as evidenced by the drainage channels visible throughout the ice. This type of relatively low coverage and consolidated ponds were infrequent in the OIB dataset, but may be common of ice in this region"

L254-255. I'd suggest rephrasing this to "infrequent" to the OIB observations, since it may be a common phenomenon.

>This is likely true, and we have added this extra information.

L256. It would be helpful to circle or highlight the sediment-laden ice as it's not apparent in this image. It also raises the question, does the algorithm also detect sediment-laden ice or is it detected as a melt pond?

>This image is actually not a great example of sediment laden ice, so we have removed this description from the text. Sediment-laden ice does not have its own classification category and would likely be put into the gray ice category, or possibly melt pond, depending on its color and darkness.

L258/Figure 11. Similar comment as Figure 10, it'd be helpful to see the segmented equivalent in the main text or supplementary information.

>We have included this as a supplemental figure.

L267-319/Section 4.1. Please see main concerns above.

>Revised discussion section, see comments in response to main concerns.

L280-281. The lack of ponds in Polashenski et al. 2017 seemed to be due to a snowfall event and freezing conditions rather than high permeability and a lack of snow.

>Polashenski et al., (2017) also discusses observations of pond-free ice that appears to have never had a snow cover (Specifically in reference to the satellite image in their Figure 15). We have added a citation to Eicken et al., 2004 here, which discussed the relationship of snow cover to pond formation.

L284-287. Do the results from earlier works using MODIS data not apply here?

>It is the authors opinion, supported by our own recent study (Wright and Polashenski, 2020), that existing MODIS melt pond products do not have the accuracy required to answer this question.

L289. How was high permeability and pond drainage determined on such a large spatial scale? Figure 10b shows no drainage features. This surface condition was classified as common in the dataset, which conflicts with the next sentence.

>If we look at the OIB dataset as a whole, the majority of the observed surface is in an advanced state of melt where the ponds have drained to sea level. This was determined empirically from looking at the dataset. This surface condition is common in reference to ice that is in a similar state of melt. In 10b, the state of melt can be described as ice that has not yet drained to sea level.

L293-294. Is this what's being suggested for the pond-free FYI areas? Before, the argument was that ponds never formed?

>We think that both pathways are possible. If the ice does not have the snow cover to support ponding (as noted by Eicken et al., 2004), or if ice permeability is too high to allow ponding (when the ice warms before surface melt begins the pore space cannot refreeze when freshwater enters, meaning ponds cannot form above sea level (Polashenski et al., 2017)), then the ponds will never form. In this section we are dis-

cussing the mechanisms required for pond free ice to emerge from ice that did have initial ponding.

L296. It's not clear what is meant by if subtle topography is powerful.

>We have removed this phrase and revised this section.

L298-300. This is not clear.

>This section has been reworked for clarity.

L312-313. Is this statement in reference to the OIB data set? For previous works, this was not found to be the same. It would be worth clarifying here.

>This statement is in reference to the OIB dataset, and we have clarified this here.

L322-324. This description should be described near the beginning of the manuscript. Submerged ice may contribute to a larger proportion of pond fraction for FYI than MYI.

>We have added the official category descriptions to the introduction of this manuscript.

L333-335. Similar to the main concerns above, a snapshot of lower FYI pond coverage than MYI pond coverage does not address the hypothesis. Previous works have shown pond coverage on FYI to be highly temporally variable over summer compared to that on MYI. The temporal average of melt pond fraction for FYI and MYI over the melt period may indeed support the hypothesis.

>We have revised the conclusion section to be clearer about the conclusions that we can and cannot draw from our dataset. As you pointed out, some of our claims were too bold to address with temporal snapshot datasets.

Figure 4. It would be helpful to use a more dynamic color scheme for the melt pond fraction. It's difficult to see the distribution along the survey lines.

>We have increased the contrast for this figure.

Figure 8. It would be helpful to know the sample size for each case.

>This has been added. âĂČ

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Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-288/tc-2019-288-AC1-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-288, 2019.

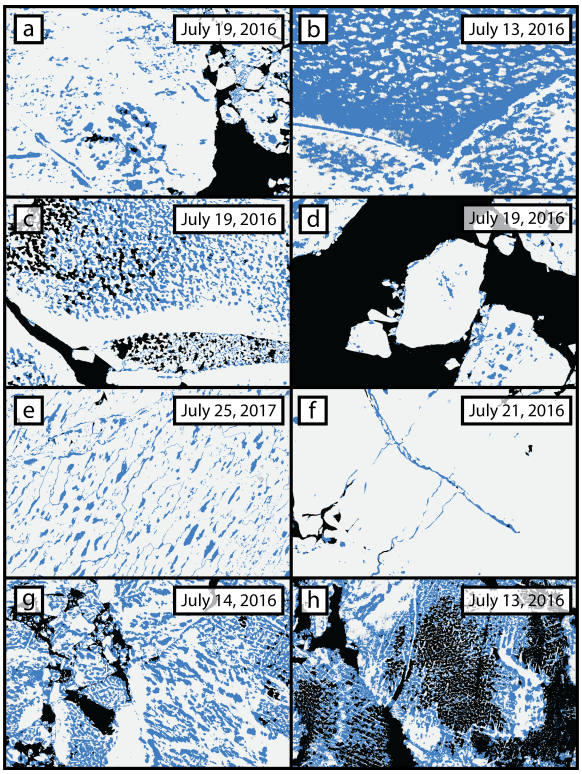

Fig. 1. Supplement figure 1, classified versions of Figure 10. Ice is white, ponds are blue, and ocean is black.

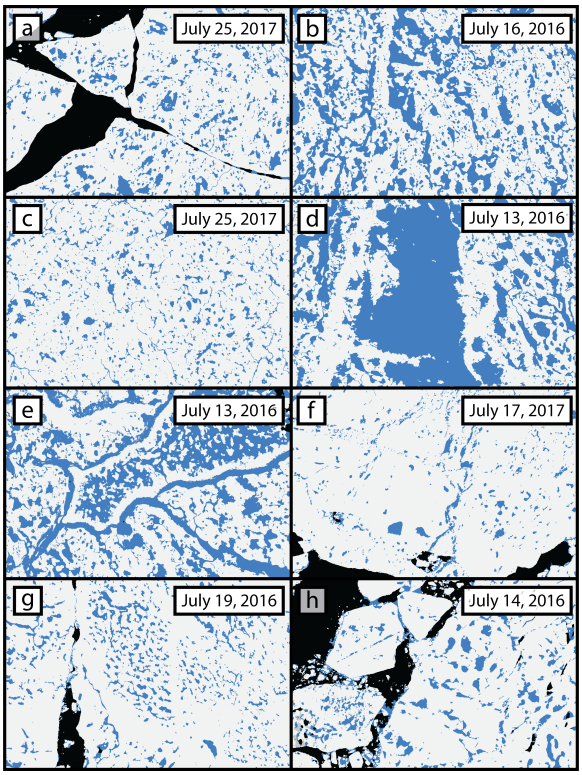

Fig. 2. Supplement figure 2, classified versions of Figure 11. Ice is white, ponds are blue, and ocean is black.