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The Cryosphere Discussions

Interactive comment on "Dual-satellite (Sentinel-2 and Landsat 8) remote sensing of supraglacial lakes in Greenland" *by* Andrew G. Williamson et al.

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Summary

Williamson et al describe a substantial new contribution to the remote-sensing detection of Greenland supraglacial lake drainage events. Their approach is to combine images from two medium- to high-resolution sensors to achieve near-daily time resolution of a study area in WNW Greenland. The authors apply this new technique over the 2016 melt season and are able to detect smaller draining lakes ($<$0.125 km^2) than previously possible, with good temporal precision ($\pm \sim$1 day). By combining the new lake drainage dataset with regional climate model output analyzed across surface catchments, the authors conclude that smaller ($<$0.125 km^2) fast-draining lakes, which

previous coarser analyses have missed, actually contribute a majority of lake water to the subglacial system across the study area. Their result shows the importance of using high-res techniques, such as presented here, to identify the locations and timing of lake water input to the subglacial system.

Specific comments

The methods portion of the paper is thorough and appears to be robust; the study uses previous work (by Pope and by Williamson) to validate its new methods.

In the results portion, it was unclear which data were used to develop the empirical relationships between L8 lake depth and S2 TOA reflectance (July 1 / 31?), and which data were used to evaluate it (Table 1; all image dates?). Perhaps the distinction is not of great importance (I cannot tell), but this could be easily clarified.

I would suggest against the use of the word "error" in lake-drainage dates. "Error" suggests that the true dates of lake drainage are known; however, they are not known. "Precision" would thus be a more accurate term.

Given the new ability to precisely identify drainage dates of more lakes than ever before, I read the Discussion with great interest. I think two of the inferences made in this section were a bit weak. However, these were not the main contribution of the work, and so scaling them back will not make a great loss to the paper.
1. Lake size and lake drainage date – The authors attribute the high water contribution (61.5%) by small lakes to the fact that they drain earlier in the melt season than large lakes (lines 511-512). While the data in Table 2 do show a significant difference (non-overlapping date ranges) between small and large lake drainage dates, this difference (just 1-2 days) is not substantial within the context of the melt season and the evolution of subglacial hydrology.
2. Lake size and elevation – The authors also state that the lower elevations of small lakes may contribute to their greater water contribution (lines 512-513). These data do not appear in Table 2 (I would suggest adding it: the mean and std elevations of large

and small lakes), and this statement contradicts another statement (lines 400-402) that lake size and elevation are uncorrelated.

What, then, can explain the high (61.5%) contribution of the small lakes? Is it simply a large total size of their basins? This information would be easy to include (I believe it is already calculated).

Finally, and most crucially, the conclusion that small lakes are important to the subglacial hydrological system is based on the assumption that their moulins stay open for the entire melt season (Figure 11). I don't have any especial reason to doubt this, but the assumption is not backed up in the paper although I believe the authors' data could easily do so. Presumably, if a moulin were to close up before the end of the melt season, a lake would re-form on site, and could be seen in the data. I think I can infer from the description of the FASTER algorithm that any such lakes would not meet the criteria for "fast-draining" and thus would not be included in this study – but this is not stated/explained in the manuscript.

Technical comments

Line 7 - I wouldn't start the abstract with "Although"; move it to the middle of the sentence as "however".

Lines 47, 49 - It is not sensible heat, but latent heat that makes both of these effects (Phillips and Mankoff references). Water temperature is not important.

Line 49 - It's actually Poinar et al. 2016 (not 2017)

Line 67 - Replace "this" with its antecedent (since it is the first sentence of the paragraph).

Line 82 - The records have no problems; instead, perhaps the methods have shortcomings.

Lines 85 and 95 - Define or remove SAR, MSI acronyms

Line 135 - Clarify year 2016

Lines 155, 159 - I got a bit confused with the numbers here, since they are similar (38 + 39 = 77). Perhaps recast the sentences to use only the number 39. Also please state the year 2016 again here.

Line 190 - R^2 = 0.999, that's excellent, good for you!

Line 271 and elsewhere - "GrIS interior" confused me; to me it means inland or upstream regions, whereas you intend to say that the water leaves the surface and enters the englacial or subglacial environment.

Line 294 - Here you say $\sim$3 meters but later (line 465) you say $\sim$3.5 meters.

Lines 347-350 - This sentence would benefit from parallel construction.

Line 353 - p=0.00 should be more precise.

Line 443 - "We opted for" sounds a bit informal.

Lines 458-468 - Effect of July 1 "cloud adjacency". Pixels 200 m from clouds were already removed (Data and methods section), so it seems to me that this cloud-adjacency argument would not apply. Perhaps more description of how "adjacent" (i.e., if there are effects $>$200 m away) these effects are is required.

Lines 536-539 - It reads a little harsh on Miles et al. to end the paragraph with the shortcomings of that study; instead wouldn't it be better to end by highlighting the strengths of your own study?

Lines 540-541 - As written, this sentence is false because other studies have combined two optical satellite datasets (e.g. MODIS and L8). Recast by adding "medium-resolution" or moving the sensors out of parentheses.

Line 561 - "less" instead of "not"

Figure 7. I like that Figure 6 was scaled up to the full image region here. The data

show a lot of variability (sawtooth-like) on multi-day scales. I'd be interested in whether this is "real" (perhaps regionally linked lake drainages?) or just noise remaining from the effects of clouded-over regions.

Figure 10. The magnitude of lake volume (x axis) seems much too large: the largest lake would have a volume of 10^{16} m^3, which would be 100 km x 100 km x 10 km, way too big. There must be an error here.

Figure 11. The y axis label is confusing: volume, yet mm?

Table 2. Consider adding the mean surface elevation of the 3 classes of lakes, as described in the "Specific comments" section of this review.

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