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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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"Dual-satellite (Sentinel-2 and Landsat 8) remote sensing of supraglacial lakes in Greenland" by Williamson et al. explores a new method of retrieving supraglacial lake depth from Sentinel-2 imagery, combines it with Landsat 8 to build a higher-temporal resolution record, tracks lake volume/filling/draining, and investigates the impact of lakes of various size on the hydrology of the Greenland Ice sheet.

Williamson et al. have produced a paper which is clear, clean, logical, well-written, and ultimately enjoyable to read. Thoughtful consideration has been given to how to combine datasets and how to interpret the resultant data. However, there are some crucial factors that I believe the paper should consider before being published.

Choosing the Sentinel-2 Method:

This comparison is a big step in the paper and will facilitate many future studies. However, I think there are one or two options which really need to be carefully considered before claiming victory – in particular exploring the use of the S2 Green Band. In Figure 3, evidence of saturation is clearly evident and you note in the discussion around line 465 that this could be related to the use of the red band. So I don't understand why you do not explore using the Green band on its own, or like the L8 method, in cooperation with the Red Band? In addition, Figures 3 and 4 (and other similar) would benefit from using heat maps rather than small dots; the data density is too high for interpretation in this format. Using a 1:1 line (or similar) might also help in interpretation.

Analysis: Error & How Many Lakes?

This study would be more robust if a little more attention was added to areas that help contextualize the data. In particular: *Adding any error bars on values which are calculated for area / volume *For volume (e.g. Line 334), a 10% disagreement between S2 and L8 seems pretty good. However, a big factor that has the potential to be quite variable between image resolutions, is the calculation of the lake shore and therefore lake bottom albedo. Did you explore this effect at all? Or at least it might be good to include it in the discussion? *In Line 330, you observe more lakes in the dual record than in L8 or S2 individually. Why isn't there more match-up of the lakes being tracked in the individual datasets? Is this a result of the higher temporal density? Cloud cover differences? Other? I'm curious because the tests in the Supplement seem to so such close agreement in lake areas being measured.

The Role of Opened Conduits:

There is an assumption that one lakes drain quickly (by opening a conduit) that these conduits continue to remain open. However, there is no discussion about whether is assumption is theoretically sound or observationally verified. Is this equally likely for small and large lakes at various ice sheet thicknesses / stress states? There seem

to be a lot of variables, and this may indeed be valid, but it should be explained – in particular given the conclusions related to the role of small lakes connecting the supraglacial and subglacial hydrological systems.

Other minor comments:

Line 8: Landsat is still what I would call medium resolution, especially with a new era of sub-metre sensors. Maybe fix by changing “high spatial resolution” to “higher spatial resolution?

Line 131: Have you considered providing tables in txt form, too (or perhaps provided with code to batch download) to facilitate reproducibility?

Line 141: You write that tiles were reprojected. How were data interpolated– NN, bilinear, other? These details were carefully described in other steps, so I ask mostly for completeness.

Line 148: For those less familiar, perhaps write “band-6” as “SWIR/band-6” or similar? Here and elsewhere.

Line 151: For cloud shadow – I don’t know that this is enough to handle shadow. Did you check any math / assumptions (based on cloud elevation, solar angle, etc.) that this would really be sufficient? It could be a big deal, especially for false-positives on fake drainage. Again, I’m really unsure and haven’t run the numbers myself, but it seems important enough to confirm.

Line 154: For lack of a better place to put this: I believe that L8 and S2 are orthorectified using different DEMs, so there could be (slight) offsets in lake locations. Have you considered this effect or the magnitude in it? The Kaab/Paul 2016 papers you referencing think about implications for velocity tracking, but I’m just not sure of the impact in this part of the world.

Line 159: You use the cloud masks provided with the data, but is there any evidence on reliability over snow and ice?

Figure 2: Reprojecting step not included? Figure 2: I believe lake area masks should feed into lake depth calculation (e.g. finding lake edges)?

Line 180: The resampling and NDWI steps appear to be described in Figure 2 in the opposite order?

Line 231: Not an issue – I’m mostly just curious – why you chose NN here (and bilinear elsewhere)?

Line 241: Well done with these acronyms ïĄŁ

Figure 3 & 4: Include legend so figure is easier to read

Line 336: Are these symmetrical distributions? Perhaps using Quartile1 / Quartile3 info will help describe the data while also being sure to use non-parametric statistics.

Line 341: Did you ever compare with MODIS to make that the *it* failed the test (as it would be expected to)?

Figure 6/7: I really like how you can display the data in the context of which data is available!! Have you considered if there was any variability in lake distribution / more nuanced than just scaling by cloud cover percent? Like an elevation-dependent extrapolation, or something like that? Also: consider combining into one figure

Figures 8 & 9: Combine into one figure

Fig 11: These colors are not necessarily all distinguishable to a color-blind reader. Revise colors and/or label the lines on the righthand axis.

Line 578: What about sharing your resultant lake dataset? Line 578: The Cryosphere data policy encourages a few things that are different from how this paper handles sharing – most importantly share data AND code in an open, citable repository. Requiring to ask an author is a large barrier particularly in the future. See Gil et al. for further suggestions, and I’m sure that your readership would love to have these tools and dataset available in an open place / GitHub repo.

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015EA000136

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