The Cryosphere Discussions

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Comment on tc-2021-3

Anonymous Referee #2

Referee comment on "Contrasting regional variability of buried meltwater extent over 2 years across the Greenland Ice Sheet" by Devon Dunmire et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-3-RC2, 2021

Summary of manuscript

Dunmire et al. present a new inventory of buried lakes detected over two winters (2018-2019 and 2019-2020) across the Greenland ice sheet. The inventory was derived by applying a convolutional neural network to Sentinel-1 scenes of the ice sheet. This new workflow can automatically detect subsurface lakes in areas of liquid water >0.5 km^2. The reported dataset has good accuracy, with 93% precision (probability that a detection is a true lake) and 88% recall (probability that a true lake is detected). The authors report higher spatial extent of buried lakes following a recent high melt season (2019) than a low melt season (2018), with some variation in different regions of the ice sheet. Northern and Northeastern Greenland changed most across 2018-2019. Finally, the authors find liquid water at the ice sheet surface at some of the buried lake sites (especially in Western Greenland) but not others (especially Southeastern Greenland). From this, they infer contrasting formation mechanisms for these lakes: burial of surface water by new snow, and immediate percolation of water into the snowpack, respectively.

General comments

The Author Contributions section states that the analysis was designed, performed, and interpreted by the PhD student. She should be commended for this leadership. However, the contributions of the other authors are vague and appear not to meet the authorship criteria of "significant contribution to the research and paper preparation". The rest of my review looks beyond this problem, which probably needs addressing separate from the science revisions.

The development of the CNN analysis and the generation of the lakes dataset are the center of the work and I think will be why future researchers cite this paper. These algorithms are well described (Methods and Appendix) and the choices and sensitivities are tested and quantified. Another strong aspect of this paper is the inference that buried lakes form by different mechanisms (burial versus trickle) in Western versus Southeastern Greenland. Finally, the writing is clear and easy to follow.

My major criticisms relate to the cursory analysis of the RACMO data and to incomplete thinking on one of the lake formation speculations. I provide some detail on these points below.

(1) The correlation analysis of the buried lakes and the weather data was difficult to follow. This would be improved by better use of figures (see comments below regarding Figure 7) and a more formal analysis of the climate variables at the buried lakes. If I correctly understand the current analysis, the authors visually interpreted the RACMO model output (Figure 7) and their regional means (Figures B3 and B4) to draw conclusions about the differences in warmth and wetness over the two years and across different regions. This has problems because all regions have very large accumulation zone areas without lakes, where the RACMO data are therefore not meaningful. Analysis on a finer spatial scale more appropriate to the lakes, along with a more sophisticated analysis of the importance of each climate variable and each month (Figures B3-B4) in forming a buried lake, is needed. A simple logistic regression analysis would be well suited and easy to implement.

(2) The hypothesis that buried lakes may be formed by subsurface penetration of shortwave radiation is speculated on and presented as a conclusion of the study (lines 14, 322), but never evaluated. However, this is easy to explore with the Bouguer–Lamber law, used in the Lepparanta and MacAyeal papers cited, and also described well by Perovich (2007) who worked on temperate snow. A simple comparison of the expected penetration depth of the shortwave radiation to that of the C-band radiation that allows detection of these features is all that is required. An explanation for the absence of melting at the surface despite subsurface melting, though, is more difficult to imagine; the authors should consider this as they evaluate the subsurface melting hypothesis. It seems that downward percolation is the more likely explanation.

Finally, some of the figures are of relatively poor quality. Some are screen shots with uncareful trimming (Figure A1) and low resolution. Most of the inset maps are missing location information (Figure 4, 6, 8, 9) and some are missing scales (Figure 1). I eventually found the rectangles on Figure 3 that give location information, but I overlooked this for a long time. A statement should be added to the captions of Figure 4, etc. that their location is shown in Figure 3, or, better yet, the lat/lon should appear directly on these panels.

This is a worthy contribution that will readily become publishable with attention to these points.

Specific comments

Line 6 - Make it clear that 2018 and 2019 are chosen as representative high and low melt years. As it's written now, it sounds like a very short attempt at a climatological study.

Line 8 - "observes" is the wrong word, an ice sheet cannot observe something

Line 27 - the sponge analogy is too informal

Lines 75-81 - Naming these bands Red, Green, and Blue would be more helpful than 1, 2, and 3 for interpreting Figure 1.

Lines 178-187 - There should be a figure that shows the lake size data, similar to Figure B2, the box and whisker plot that shows the mean, extrema, and spread. Reference this new figure and Figure B2 in this section.

Lines 186-187 - How many lakes were found in the NO and NE regions? I could not find this information in the manuscript. It should be included in at least this line of the text, if not in a figure or table.

Lines 217-218 - It's worth rephrasing this sentence because the assertion that "firn aquifers cannot be detected directly in S1 imagery" is at odds with the title of Brangers et al. (2020), "Sentinelâ□□1 detects firn aquifers in the Greenland Ice Sheet". The word "directly" helps, but some expansion is recommended.

Lines 226-241 - This section has a lot of imprecision regarding temperature anomalies versus raw temperatures (for instance, is +1.52°C a temperature or a temperature anomaly?), and z-scores being directly compared to temperatures / anomalies (lines 233-238). The regional patterns in temperature should be tested for statistical significance.

Lines 261-264 - Benedek and Willis (2020) found 6 winter drainage events over three years in a 30,000 km^2 area with about 300 lakes, a very small ratio. And, only 2 of the 6 lakes drained before January 1. Thus it seems that this effect would be a very minor source of bias to the dataset. This should be quantified or commented on.

Lines 286-287 - The co-location of a buried lake and a surface depression doesn't necessarily imply meltwater percolation through the snow until it reaches an ice layer. Ice layers occur regardless of surface topography (Machguth et al., 2016), and do not always block percolation (Samimi et al., 2020). The bottoms of these lakes could also be thermally controlled, like firn aquifers usually are - the water percolates down until it reaches an area with sub-temperate ice, where the trickling stops and refreezing begins.

Lines 297-299 - The Leeson (projections) and Howat (observations) citations are backward.

Lines 304-306 - The very low fraction of buried lakes that drain (<1%; Benedek and Willis, 2020) makes this assertion far fetched.

Line 312 - "to explain the differences" is imprecise

Lines 329-330 - No arguments in the paper give support for this sentence. Suggest thinking more carefully about the wider significance of the study and conclusion.

Figure 3 and others - Recommend one of the polar stereographic projections for all map figures over the Mercator projection due to the area distortion. The sub panels of Figure 9 are mislabled (backward) on Figure 3, causing confusion.

Figure 5 - Insufficient information is shown to understand whether the regions (SW - CW versus others) are different with statistical significance. Adding $1/\sqrt{n}$ error bars to these bars, or some other quantified uncertainty, is needed.

Figure 7 - This figure shows minimally interpreted RACMO model output. It does not show any of the new data from this paper (lake locations), which would put the RACMO output into context. This figure or an additional figure should extract the temperature and melt anomalies at the locations of the lakes (such as onto their convex hulls, or some representation by region) to interpret and summarize the RACMO data. The subregion analysis shown in Figures B3 and B4 comes close, but the areas are too large. For example, all subregions have very large accumulation zone areas without lakes, where the weather data are therefore not meaningful.

Figure B4 - The caption says June through November monthly melt anomaly, but only

June through August are shown.

Technical corrections

- The url to the Arctic Data Center link breaks when it spreads onto two lines (Lines 334-335). Use of the DOI (cited as Miege, 2018) will shorten the link and is the recommended citation rather than a url.
- "All other data is freely available on Google Earth Engine" (Line 335) doesn't help someone follow or build on the work. Each of the datasets used should be listed with its GEE identifier snippet.
- Figures 6 and 7 have spell-check annotations included in the images.