

Reviewer 2

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.

As is usually the case with papers where the first author is a PhD student, the student leads every aspect of the paper (including the study design, analysis, and actual writing etc), but the student receives guidance throughout the process in the form of (at least) bi-weekly meetings with their advisors/co-authors. Additionally, the advisors/co-authors help with editing the paper. All of the above was the case for this paper, which amounted to "significant contributions".

We will reword the Author Contribution Statement to read as follows: *"DD conceived the study and led the training of the CNN, the data collection, and the analysis. AFB, JL and RTD helped to develop the ideas and methods throughout the study, and discussed the results. NW ran the firm model; SNOWPACK, and assisted with interpreting the simulation results. All authors involved in editing the manuscript"*

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.

We thank this reviewer for positive comments about our paper.

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.

With regards to the following reviewer's comment: "This has problems because all regions have very large accumulation zone areas without lakes, where the RACMO data are therefore not meaningful.", in our analysis of RACMO model output we were careful to only include lower elevation areas where buried lakes typically occur. In (current) lines 221-222, we clarify this with: "to investigate the discrepancies between total surface and buried lake area across the six GrIS subregions, we analyzed RACMO2 data from 2018 and 2019 **at elevations lower than 2500 m**, comparing temperature and melt with the climatological mean". We chose an elevation of 2500 m above sea level (a.s.l) as a threshold because the maximum elevation that a buried lake was detected at was 2450 m a.s.l. Thus, the temperature and melt anomaly numbers mentioned in section 3.2, as well as the numbers in Figures B3 and B4, concern only the areas of ice sheet that are below elevations of 2500 m. We apologize that this was not clear in the text and was missing from the captions in Figures B3 and B4. To make this clearer in our revised paper, we will reword areas of the text in section 3.2 and add this information to the captions of Figures B3 and B4.

This comment is also along similar lines as major comment 1 from Reviewer #1. Following their suggestion, we will include snow modeling results at 3 different buried lake locations in order to further test our hypothesized connection between climate variables from RACMO, and buried lake distribution and formation. We use SNOWPACK, a 1-dimensional, multi-layer snow model forced with RACMO climate data to investigate the link between climatological variables and subsurface conditions at our 3 locations. These results of this modeling work are presented in the figure below, which will replace Figure 7 in the manuscript. These model simulations support our hypothesis that in the relatively warm 2019 in northern Greenland, snow layers with high liquid water content can remain liquid until the buried lake detection. To summarize the results of the figure, we will update the manuscript to include the following information:

Higher air temperatures in each region during June and July 2019 contribute to higher ice-sheet-wide July 2019 subsurface temperatures (Fig. 7b). For sites X, Y, and Z, respectively, the average subsurface temperature in the top 7 m of the snow column is 2.06, 1.97, and 0.34 °C greater in July 2019 than in July 2018.

Further, higher air temperatures in NW, NO, and NE Greenland from August - November, 2019 lead to correspondingly higher subsurface temperatures than in 2018 in these regions (Figure 7b). For example, at Site X in Figure 7, which is located in CW Greenland, the September 2018 and 2019 temperature anomalies are -0.71 °C and +0.98 °C, respectively and the average September subsurface temperature in the top 7 m of the snow column is 0.80 °C colder in 2019. In contrast, at Site Z in NE Greenland, the September 2018 and 2019 temperature anomalies are -2.54 °C and +4.04 °C, respectively, average September subsurface temperature in the top 7 m of the snow

column is 3.18 °C warmer in 2019. Additionally, at Site Y located in NW Greenland, Figure 7c shows that meltwater exists in the subsurface during both the 2018 and 2019 melt seasons, freezing through entirely in 2018, but lasting through the end of the year in 2019.

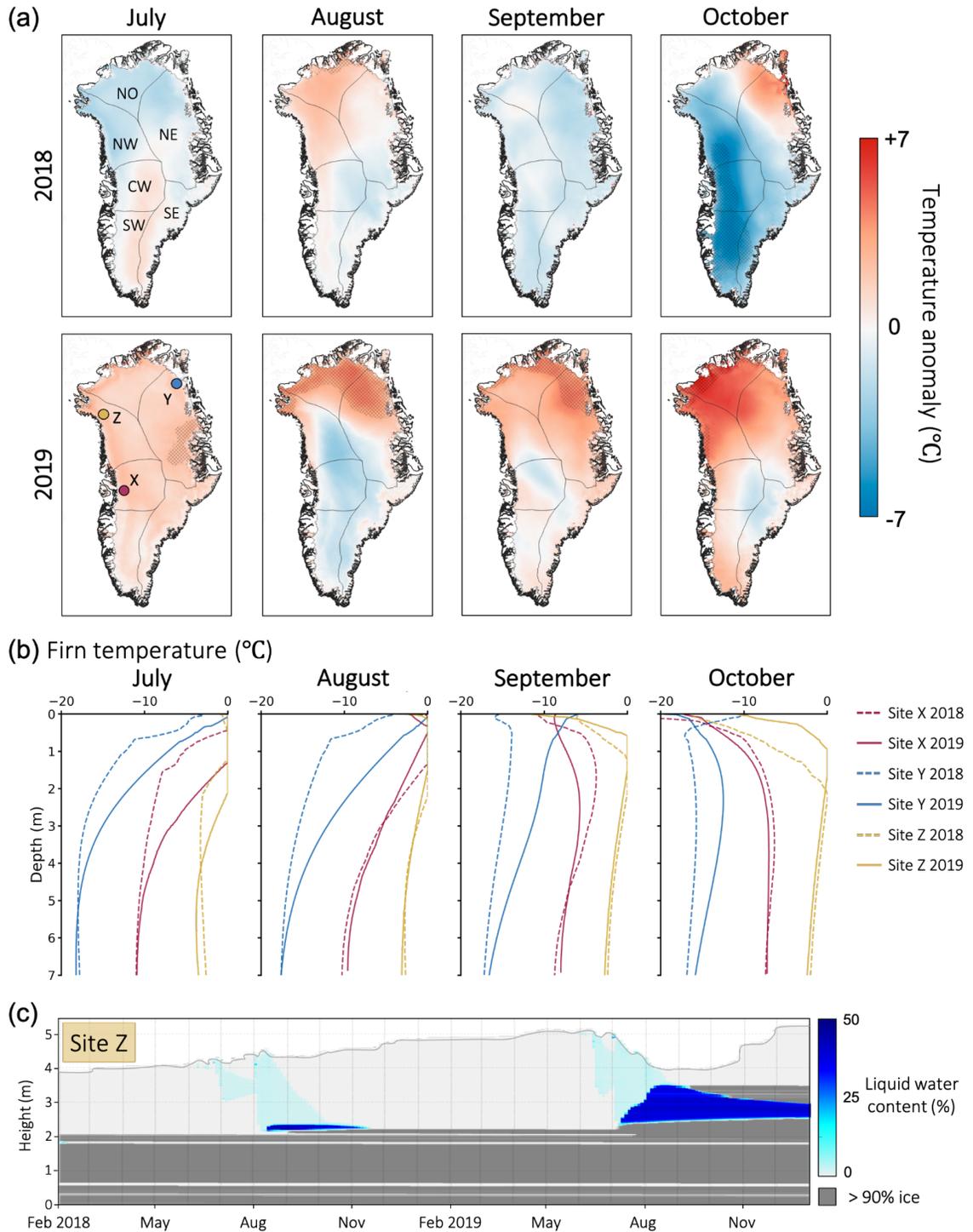


Figure 7. Atmospheric and subsurface temperature modeling results. (a) July through October 2018 (upper) and 2019 (lower) monthly temperature anomalies (°C) from the 1958-2017 climatological mean derived from RACMO. Areas with monthly temperatures that are significantly different from the climatological mean at the 95% confidence level are shaded with a cross-hatch pattern. (b) Subsurface firn temperature profiles at 3 sites (X, Y, Z; locations indicated in panel (a)) from July through October for 2018 and 2019. (c) Time series of liquid water content (%) with depth at site Z. Areas with >90% ice are shaded in grey.

(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.

The hypothesis that buried lakes may be formed by subsurface penetration of shortwave radiation has been evaluated in the past. For example, MacAyeal et al. (2018) use a numerical model to show that solar absorption below the surface can generate a subsurface meltwater/slush layer without the presence of surface water. Meanwhile, Leppäranta et al. (2013) present field observations (their Figure 3) that shows the development of a near-surface lake due to subsurface penetration of shortwave radiation, again, without surface meltwater. Because this process has already been evaluated through modeling and documented in field observations, we do not think it is necessary to re-evaluate it in our study.

However, as an alternative due to the process of buried lake formation via burial from snowfall, we agree with the reviewer that it is likely that the downward percolation of meltwater from the surface is the more likely process. Though we also suggest that shortwave radiation may enhance this subsurface melting. We will add the following sentence to the manuscript at the end of the second paragraph of Section 4.2: “While downward percolation of meltwater is the more likely explanation for buried lake formation without the presence of surface meltwater, subsurface penetration of solar radiation may enhance melting of pre-existing buried lakes that form via a different method (Dunmire et al. 2020).”

Finally, some of the figures are of relatively poor quality. Some are screen shots with uncaredful 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.

We apologize for the poor quality of some of the figures in the previous version of our paper. We will update the quality of Figures 3, 4, 6, 8, 9, A1, B1, and B2. We will also add location information to the panels of Figures 4, 6, 8, and 9 and a scale bar to Figure 1.

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.

We will update these lines to read: *"For the years 2018 and 2019, which represent low and high melt years, respectively, we compare total areal extent..."*.

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

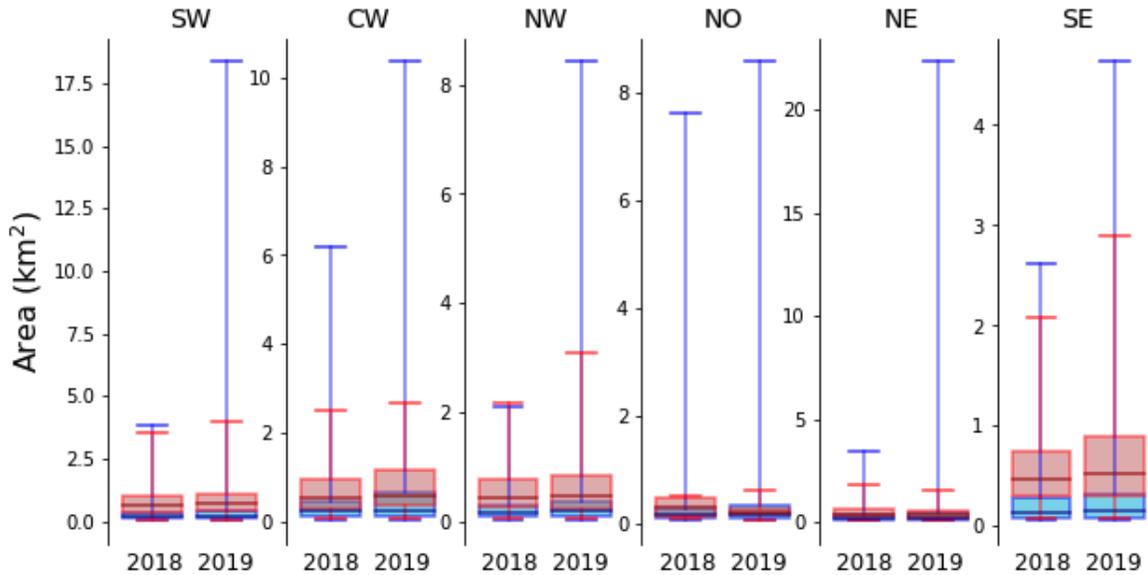
We will replace "observes" with "has" on line 8.

Line 27 - the sponge analogy is too informal

We will remove the sponge analogy and update these lines to read: *"Firn takes up meltwater and buffers against mass loss (Harper et al., 2012)"*

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.

We thank this reviewer for this suggestion, and we have produced the figure that they suggest; please see below. However, personally, we don't think this is a valuable addition to the paper because the distributions of lake area are very skewed, particularly for surface lakes. Additionally, mean lake area for each region is already located in Tables B1 and B2 and we will also add the maximum detected lake area as a column in these tables. We can add this figure to the appendix if the editor/reviewer requests.



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.

We will add information on the number of lakes in the NO and NE regions, reading: “(4 and 19 buried lakes in the NO and NE regions, respectively)”. Information on the number of buried and surface lakes detected in each region is also present in Tables B1 and B2, but we will also add this information to Figure 3 (for buried lakes) and Figure B1 (for surface lakes)

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), "Sentinel 1 detects firn aquifers in the Greenland Ice Sheet". The word "directly" helps, but some expansion is recommended.

Reviewer 1 made a similar comment to this, so we have simply copied and pasted our response to them below.

In Greenland, firn aquifers are likely buried too deep to be detected directly by C-band radar. For example, the average depth of the perennial firn aquifer in SE Greenland is ~22 m below the surface (Miège et al., 2016); much deeper than the penetration depth of C-band radar. Thus, our method can only directly detect buried lakes, which are located at shallower depths than firn aquifers. However, temporal changes in microwave backscatter from C-band radar have been used to infer the locations of firn aquifers in Greenland. For example, on this subject of using C-band radar to detect firn aquifers on the GrIS, Brangers et al. (2020) says:

“An important hypothesis here is that the radar will likely not directly sense the water table (only for shallow perched water tables). Instead, it is likely that the slowdown in refreezing

of water in the upper profile (above the water table) provides a distinct signature and serves as a proxy for the detection of the aquifers.”

To clarify this point we will change (current) lines 216 to 219 to read: *“Firn aquifers, unlike buried lakes, are buried too deep to be directly detected with S1 microwave imagery. For example, the top surface of the perennial firn aquifer in SE Greenland is about 22+/-7 m below the ice surface (Miège et al., 2016) and can currently only be detected from S1 images by using the temporal change in microwave backscatter as a proxy to infer the locations of firn aquifers (Brangers et al. 2020). Although it is unclear what relationship the buried lakes and firn aquifers have, we suggest that the buried lakes may feed firn aquifers by draining vertically.”*

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.

We will clarify that the temperatures mentioned in this section are *temperature anomalies* (i.e +1.52°C temperature anomaly). Additionally, we will test the regional patterns in temperature for statistical significance and indicate statistically significant areas in Figure 7, an updated version of which can be seen in the response on page 3.

Lines 261-264 - Benedek and Willis (2020) found 6 winter drainage events over three years in a 30,000 km² 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.

We would first like to point out that Benedek and Willis (2020) only analyzed drainage events in the winter; we expect that there may be more in the summer. But in any case, we agree with this reviewer and will edit this sentence to read: *“Finally, recent work has shown that buried lakes can drain, and on very rare occasions, even during the winter months (Benedek and Willis, 2021); though this effect will likely be only a very minor source of error.”*

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.

We agree that subsurface ice lenses can occur regardless of the surface (and basal) topography. However, as surface meltwater tends to pond in surface depressions, it seems highly likely that many buried lakes will be co-located with surface depressions.

Additionally, since lateral flow of liquid water in snow and firn can be substantial even in areas with low slope angle (Webb et al., 2021), surface depressions may collect meltwater from a larger area. We have not discounted the possibility that the bottom of the buried lake may be thermally controlled, and we will mention this with the following text: *“These subsurface ice layers could be the result of thermally controlled refreezing of downward percolating meltwater, providing impermeable layers for future meltwater to collect on top of.”*

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

We will change the order of these citations.

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

We agree with the reviewer that based on the results of Benedek and Willis (2021), this assertion may seem far fetched; however, we believe that the possibility of winter drainage, which Benedek and Willis (2020) show true, is important to note. Due to the reviewers concerns we have modified these lines to read:

“However, this buffer may only be temporary given that buried lakes have also been shown to drain (Dunmire et al. 2020), even sometimes during the winter (Benedek and Willis, 2021). Because buried lakes exist perennially and can drain throughout the year, at lower elevations (< 1600 m, Poinar et al, 2015), drainage of buried lakes could provide an influx of water to the bedrock at atypical times of year. However, presently it seems that only a small fraction of buried lakes drain during the winter so the influx of water to the bedrock during winter months may be minimal.”

Line 312 - "to explain the differences" is imprecise

We will change this phrase to: *“to explain the spatial and temporal differences”*

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.

To expand on this point and to satisfy a similar comment made by Reviewer 1, we will add the following sentences following (current) lines 329-330: *“Operation Ice Bridge radar has been used to detect buried lakes on the GrIS (Koenig et al., 2015), however due to this technique’s limited spatial and temporal resolution, it is possible that some lakes could be missed, especially in regions with low spatial data coverage. Continent-wide S1 image coverage dates back to October 2014 and will be a useful tool for expanding our buried lake data set to other melt seasons in future work.”*

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 mislabeled (backward) on Figure 3, causing confusion.

We have updated Figure 3 and Figure B1 to use NSIDC Sea Ice Polar Stereographic North (ESPG:3413) projection. We have also removed the labels on Figure 3 and added location information to individual figures, as described above on page 5 of this response letter.

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.

We will add $1/\sqrt{n}$ error bars to Figure 5 to quantify uncertainty.

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.

This comment is strongly related to the first major comment from this reviewer. Please find our response to the issue on page 2

Figure B4 - The caption says June through November monthly melt anomaly, but only June through August are shown.

Thank you for catching this. We will update the caption of Figure B4 to *“June, July, and August monthly melt anomaly...”*

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 Miegge, 2018) will shorten the link and is the recommended citation rather than a url.

We will update this in the data availability section to read: *“Firn aquifer data is available from the Arctic Data Center (Miegge, 2018)”*

"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.

In our revised manuscript, we will add the following GEE identifier snippets to the Data Availability section. :

- Sentinel 1: `ee.ImageCollection("COPERNICUS/S1_GRD")`
- Sentinel 2: `ee.ImageCollection("COPERNICUS/S2")`
- Landsat 7: `ee.ImageCollection("LANDSAT/LE07/C01/T1_8DAY_TOA")`
- Landsat 8: `ee.ImageCollection("LANDSAT/LC08/C01/T1_8DAY_TOA")`
- Greenland Icesheet Mapping Project DEM: `ee.Image("OSU/GIMP/DEM")`
- Arctic DEM: `ee.Image("UMN/PGC/ArcticDEM/V3/2m_mosaic")`

Figures 6 and 7 have spell-check annotations included in the images.

We have updated Figures 6 and 7 and also increased the image quality. Thank you for noticing this.

References

Webb, R. W., Jennings, K., Finsterle, S., and Fassacht, S.: Two-dimensional liquid water flow through snow at the plot scale in continental snowpacks: simulations and field data comparisons, *The Cryosphere*, 15, 1423–1434, <https://doi.org/10.5194/tc-15-1423-2021>, 2021