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The Cryosphere Discussions

Interactive comment on "Winter drainage of surface lakes on the Greenland Ice Sheet from Sentinel-1 SAR Imagery" *by* Corinne Benedek and Ian Willis

Andrew Sole (Referee)

a.sole@sheffield.ac.uk

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General comments

This manuscript presents the first quantification of the drainage of supraglacial lakes in Greenland during winter. Such events have previously only been described qualitatively, or their occurrence inferred from proglacial river data. As such, the authors make a worthwhile contribution to help fill in some gaps in our understanding of ice sheet hydrology. The paper is on the whole clearly written and the data analysis is valid and suitable (barring a few inconsistencies – see specific comments below). The main conclusions are justified, although there are some overly speculative comments made at

the very end of the manuscript. My main comment is that the temporal coverage of the radar data used is limited. Sentinel-1b only started consistently retrieving data from west Greenland in October 2016, so a 6-day period for the same relative orbit is only possible from then. This raises the question of why the authors did not look for winter lake drainages over more recent years (i.e. after 2016/17). Doing so might improve the temporal resolution of the data and thus avoid some of the limitations. There are also a number of more specific points that need to be addressed.

Specific comments

L2: 'immediately' seems to contradict the 'hours to days' later in the sentence. I suggest removing it.

L3 & L26: Is meltwater access always sustained for the rest of the summer? If the ice is thick (so that creep closure rates at the base of the moulin are rapid) and surface meltwater input following lake drainage is low (i.e. the lake and moulin are at high elevation), the moulin might close and the lake refill.

L26: 'This' should be 'Drainage' otherwise it is somewhat vague what is being referred to.

L27: Not necessarily the 'down-glacier direction'. The direction of subglacial water flow is determined by the subglacial hydropotential surface, the slope and aspect of which will vary from that of the ice surface (due to the bed topography) and may be different from the broad definition of 'down-glacier'.

L32: It might be worth adding that the ice speed often decelerates below the pre drainage value because of the temporary increases in basal hydraulic efficiency.

L36: Although lakes contribute to total runoff from the ice sheet, they do not 'control' it. If you look at a seasonal hydrograph (e.g. Bartholomew et al. (2011, doi:10.1029/2011GL047063)), the overall shape is determined by atmospheric temperatures and ice surface melt rates. Because the highest melt rates are closer to

the margins at lower elevations where there are fewer lakes, most meltwater enters the subglacial drainage system via crevasses and moulins not associated with lakes (Koziol et al. 2017). Lake drainages are typically superimposed on this seasonal pattern.

L48 - 49: This last part of the sentence doesn't quite make sense to me.

L52: You should use the final TC reference which is 2013 (also in the reference list).

L64: More recent data acquisitions from Sentinel-1 a and b are more consistent and regular. Did you look over the 2017-2018 and later winters and not find any lakes? Or have you not looked at these data? Doing so might remove some of the temporal frequency limitations you mention later in the manuscript.

L90: I wonder if it is worth mentioning somewhere that subglacial lake drainage (and the resulting formation of so-called 'collapse basins') might lead to a similar change in radar backscatter. The fact that you used a supraglacial lake mask to search for the backscatter changes suggests that the changes you identified were supraglacial lake drainages, but it might be worth a mention nonetheless.

L105. The 'therefore' does not quite follow as written, but needs more explanation in the previous sentence justifying why you'd expect gradual freezing to lead to an increase in backscatter. Also, you should provide more details about why you think that a lake drainage would lead to a sudden, significant and sustained increase in backscatter. Is it because the collapsed lid of the lake would create chaotic relief and therefore be bright, or is it just the change from the radar 'seeing' through the frozen to the lake surface, to the radar instead seeing the ice of the drained lake bed?

L106: I think the comparison with a summer lake drainage is probably valid but requires a bit more explanation. In the summer case, the backscatter values change because the surface changes from water to ice. It is likely the same change that is seen in winter (even though the lake might be partially frozen over) because C-band SAR can

penetrate a few m of ice - likely thicker than the frozen lake surface, at least in the early part of the winter.

L121: It would be useful to also state the actual area in metres squared

L125: Should it not be the latest rather than the greatest? Otherwise the estimated volume might be significantly greater than it was at the time the lake drained. Later in the manuscript you do refer to the volume estimates being for the last Landsat image of the season, so I think there is a mistake somewhere here.

L133: Did the image tiles include any seawater? If so, was this used as the darkest pixel? Might the darkest pixel not be from a lake with sediment at its base and thus not truly representative of the spectral signal of deep water?

L157 – 160: Understanding of this process would be greatly aided by the addition of an explanatory diagram.

L172 – 174: But you used the Landsat image with the greatest area for the lake depth rather than the latest one (L125). It is also possible that the lake volume reduced following your Landsat-derived volume calculation.

L179: It would be useful to show the extent of the optical lake masks on the Sentinel-1 backscatter images to see over what area the mean change in dB is calculated. Also, might a median value be less prone to the influence of outliers?

L186: 'identified' would be better than 'filtered out' (otherwise it seems like you are removing them from the time series)

L186 – 187: This repeats some of the methods section really. Is it needed here again? 'All other lakes. . .' could follow logically straight on from the previous paragraph.

Figure 3 caption: Does the last sentence definitely apply to this figure? It does not seem to make sense.

L198 (subtitle 3.2): It would be useful to state in the section title what you are confirming

- 'Confirmation of winter lake drainage. . .'

L219: Average depth for Lake 2 after drainage is more than double that calculated when the lake was present. Why do you think the differences are so large? Do you only calculate the depth of the depression to the lake shoreline using photoclinometry? Apologies if I've misunderstood the method, but I found it difficult to follow.

L227: 'calculated using' might be better than 'expressed through'

Figure 7 caption: The second and third sentences are a bit convoluted. I suggest changing to: 'The first column of images shows the collapse vertical distance of each pixel calculated by interpolating and differencing the pre- and post-drainage topography.'

L232: I think it would be worth briefly reiterating how you used the z-score – I.e. the z-score of backscatter change for each lake is calculated relative to the backscatter change of all lakes across the scene

L250: C-band SAR penetrates a few m of ice (Rignot et al. 2001), so likely sees through the nascent ice lid. I think this needs to be stated more clearly early on. You discuss the low backscatter values in a somewhat vague manner initially before offering an explanation in Section 3.3.3. Perhaps it would make more sense to swap the order of Sections 3.3.2 and 3.3.3?

L257 – 258: Based on Figure 5 you might have more luck using Otsu thresholding on the Sentinel-1 images, as this would 'fill in' the interior of many of the lakes that are doughnut shaped in the NDWI composite.

L266: The value of 9 m is for dry cold firn. It will be less for the ice lids on the lakes (a few m or less I expect based on Rignot 2001).

L272: Be clear that this is temporal frequency

L273: Both satellites were only recording image consistently from c. October 2016

L282 – 283: Here you state that the depth estimates were based on the last available image, but on L125 you state that the depth measurement was based on the image when the lake was largest.

L285 – 287: Based on the above discrepancy in how you measured the lake depth, your estimate might very well be an overestimate rather than an underestimate. This needs to be cleared up and the justification of why the lake depth and the photoclinometry depth are so different amended accordingly.

L290 – 291: In terms of determining whether water was transported into the basin from higher elevations, could you not compare the dB values with the maximum achieved over the winter to detect surface melt at higher elevations? You could also use the runoff output of a regional climate model like RACMO.

L293 – 294: Have you considered using the ArcticDEM time-stamped data strips? There may be some that would help to further constrain the volume of the drained lakes. See e.g. Livingstone et al. (2019) https://doi.org/10.5194/tc-13-2789-2019

L301: Maybe remind the reader that this refers to the 5 m mosaicked product so is made up of data from many different times.

L303: Changes in backscatter are 'caused by' lake drainage events

L313: What about short sharp melt events over winter? Have you looked at any available meteorological data? Also do you detect a reduction in backscatter for the non-lake surface at the same time the lake backscatter increases? This might indicate a small amount of surface melting that might have an effect on the (presumably relatively inefficient) subglacial drainage system if it got to the ice bed.

L316: The transient nature of any speed-up probably means that there would be no discernible signal in a winter average velocity estimate.

L317 – 318: I'm not sure your sample size is big enough to be able to say this definitively, so it may be worth including this caveat.

L322 – 325: Without actually doing a rough calculation (basin or lake diameter, velocity and time) this seems overly speculative.

l319: the term ‘cascade draining’ is a little misleading (although I realise it is used in the title of the Christoffersen paper). Perhaps add a very brief explanation of the process – i.e. drainage of one lake creates ice acceleration and a tensile shock that is transferred through the ice and can trigger other lakes to drain etc.

L329: I don’t think it is necessary to repeat ‘large, sudden, anomalous and sustained’ here.

L329 & 332: I think it is worth specifying that you are talking about supraglacial lakes here (for anyone who might just read the conclusion).

Technical corrections:

Figure 2: Lines need to be thicker and symbols larger (and C is very difficult to see)

L148: missing space between value and units

L151: Do you mean Appendix A? Appendix B appears to show ice velocity data.

L230: ‘event’ should be ‘events’

L242: ‘false negative ones’ should be ‘false negatives’

L243: ‘false positive’ should be ‘false positives’

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