Responses to Reviewer 1

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.

Thank you to the reviewer for his thorough and very helpful review of our manuscript and for these positive comments. We are pleased the reviewer recognises the 'worthwhile contribution' and 'filling in of gaps' our paper makes and are glad to hear he thinks it is generally 'clearly written' with 'valid' and 'suitable' data analysis with the main conclusions 'justified'. We will clear up the 'inconsistencies' and remove 'overly speculative comments' as detailed below.

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.

The temporal coverage we include in our analysis spans 3 years. While, of course, time span can always be increased (as can spatial coverage) we note that many published papers investigating lake drainages or other phenomena on ice masses only cover 1 or 2 years. We are keen for The Cryosphere to publish what we believe is the first documentation of winter lake drainage on the GrIS. It will be up to others to adapt and extend this analysis to cover other time periods and other parts of the ice sheet, and other ice masses.

However, following the reviewer's comment we did investigate imagery from later years from the same relative orbit as we'd used in our analysis and unfortunately the temporal resolution is not significantly improved. We wish to examine just one relative orbit to remove ambiguity of backscatter associated with using different relative orbits. As a way of background, we first started this work in 2017 [Note Corinne Benedek has taken Maternity Leave since this time]. We chose an area of the ice sheet where others had worked and where we knew there were plenty of lake drainages. We chose a relative orbit where temporal resolution was good over the previous 3 winters, and that is how we arrived at the data set we have.

Specific comments

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

We will replace ‘immediately’ with ‘rapidly’.

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.

Recognizing this point, we will change line 3 to “and then can allow melt water. . .” and change line 26 to “may permit meltwater”

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

We will change “This” to “This drainage”

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’.

We will change to “down-hydraulic-potential direction”

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.

We will add this suggestion

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.

We agree. Thank you. We will rewrite our text to make these exact points

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

Sorry this should read “Conventional understanding is that lakes that completely or partially drain during the summer then freeze during the winter, either freezing through completely or maintaining a liquid water core (Selmes et al., 2013; Koenig et al., 2015; Miles et al., 2017; Law et al., 2020).

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

Yes, we will change this - The Cryosphere, 7, 1433–1445, 2013

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.

Please see our response to this point in the General Comments section above

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.

We think the place to make this point is not here in the Methods but perhaps in the conclusions / suggestions for future work and so we will add it there.

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?

We will remove the word "therefore". We will also explain more fully why we'd expect a slow lake freezethrough to be associated with a gradual backscatter increase. This is explained in the paper we were both involved with (Miles et al, 2017) but we will summarise things here and refer to that earlier paper. Briefly, liquid water absorbs HV backscatter, whereas frozen water reflects more of the signal as bubbles entrained within frozen lake ice increase the relative backscatter compared to liquid water . The backscatter signal of unfrozen and frozen lakes is therefore sufficiently distinct to allow freeze-through identification.

Similarly, we will add a sentence or two with reference to previous literature about why a lake drainage would lead to a sudden, significant and sustained increase in backscatter. We agree with the referee that both of his suggested processes are relevant. The former would produce a very high backscatter that is greater than the surrounding whereas the latter would produce an increase in backscatter to around the background values. We saw examples of both associated with summer lake drainages, which we reported in Miles et al 2017.

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.

Yes we agree. We think this point is implicit in what we have said but we will make it more explicit.

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

We agree and will add this, i.e. 8000 m^2. [Note the resolution of GRD scenes used is 40 x 40 m].

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.

Sorry there was an error made here in the description and in the calculation of area, depths and volumes. We agree that the calculations presented should be from the latest unfrozen Landsat-8 image prior to freeze over. We have recalculated areas, depths and volumes and will change the table values to those shown below. We will also add a listing in the appendix of the image scenes used for these calculations. Compared to previously the lake areas have all decreased. The exception is Lake 5, which has increased slightly, as a result of us accidentally excluding some peripheral pixels in the previous calculation that are now included. Compared to previously the mean lake depths have all increased and are now closer to the estimates derived from the photoclinometry method.

Lake	Location	Drainage Date	delta dB	z-score	Pre-drainage Lake Area	Pre-drainage Mean Lake Depth	Pre-drainage Lake Volume
Lake 1	-47.32 , 68.70	11 Nov 2014 to 23 Nov 2014	-4.3	3.5	0.04 km^2	0.57 m	0.000021 km^3
Lake 2	-48.52, 68.91	10 Jan 2015 to 22 Jan 2015	-4.4	3.4	6.12 km^2	3.26 m	0.0200 km^3
Lake 3	-48.75, 69.43	05 Jan 2016 to 17 Jan 2016	-3.8	2.7	0.43 km^2	1.89 m	0.0008 km^3
Lake 4	-48.38, 69.40	05 Jan 2016 to 17 Jan 2016	-2.3	2.6	0.51 km^2	2.56 m	0.0013 km^3
Lake 5	-47.43, 68.62	10 Feb 2016 to 22 Feb 2016	-3.2	2.8	1.84 km^2	0.86 m	0.0016 km^3
Lake 6	-48.03, 68.75	06 Nov 2016 to 18 Nov 2016	-9.3	2.2	2.27 km^2	1.41 m	0.0032 km^3

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?

Yes, the tile included seawater; and yes, seawater was always the darkest pixel. We will amend the text to make this more explicit: "Reflectance of deep water was determined per image by selecting the darkest pixel (which was always a seawater pixel) in each image."

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

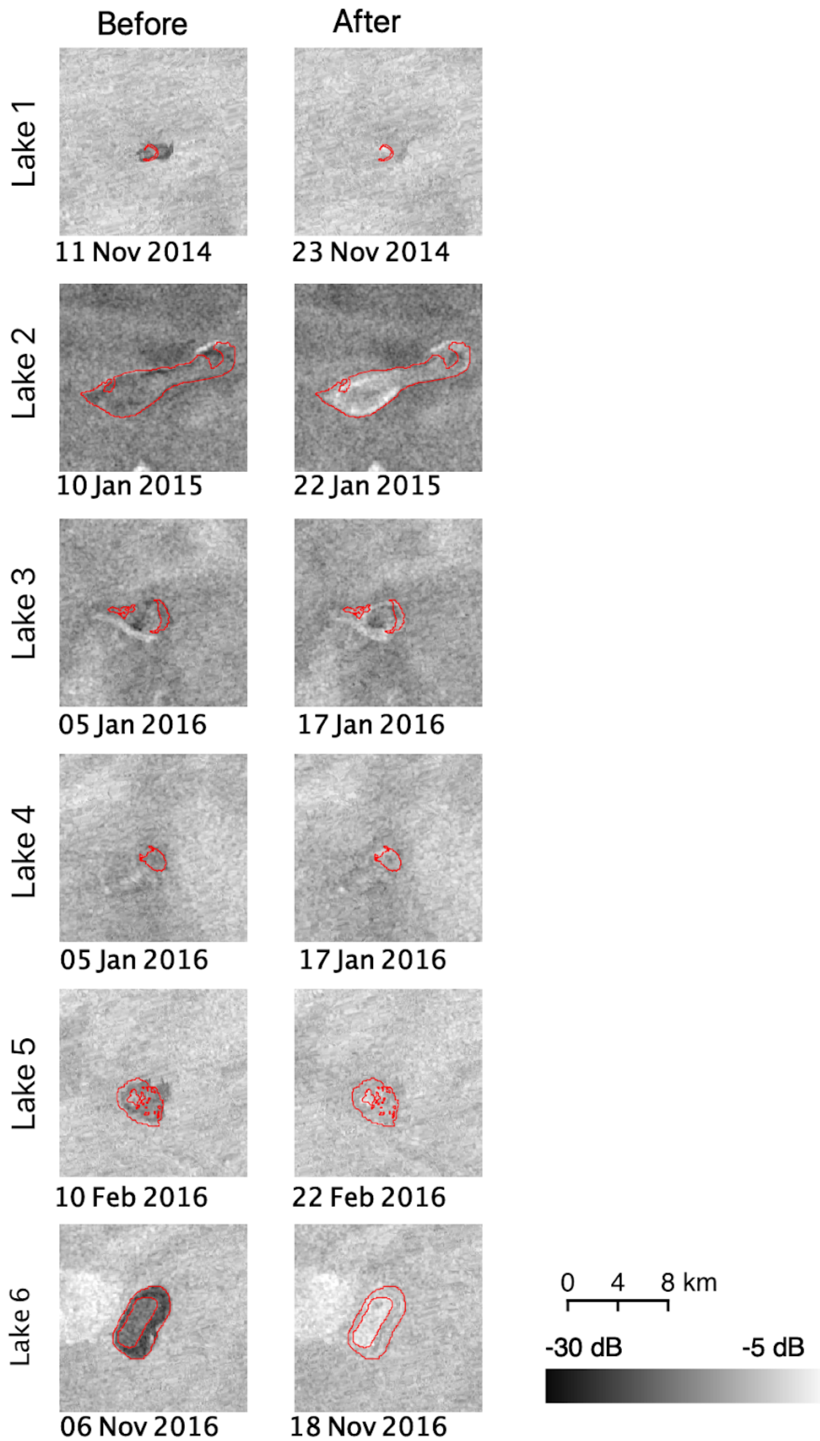
We plan to tighten up the explanation of this method in the text. We mention that the method was used by Pope et al 2013 and described there (without ref to a diagram). But we can also add a simple 2D cartoon of a cross section along one of the transects shown in the Supp Mat Figure A2, first showing the offset and then showing closure of the offset and therefore the final surface . So Supp Mat Figure A2 would then have three components: a b and c.

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.

Please see our response to the L125 comment above. Lake volumes have been recalculated based on the last available Landsat-8 image for each lake prior to freeze-over. The dates / filenames of these images will be included in the Appendix and referenced here as well. The text will be changed to reflect the volume calculation and to note that though they were based on the last available image before freeze over, this does not rule out the possibility that lake volume changed between the image acquisition date and freeze-over.

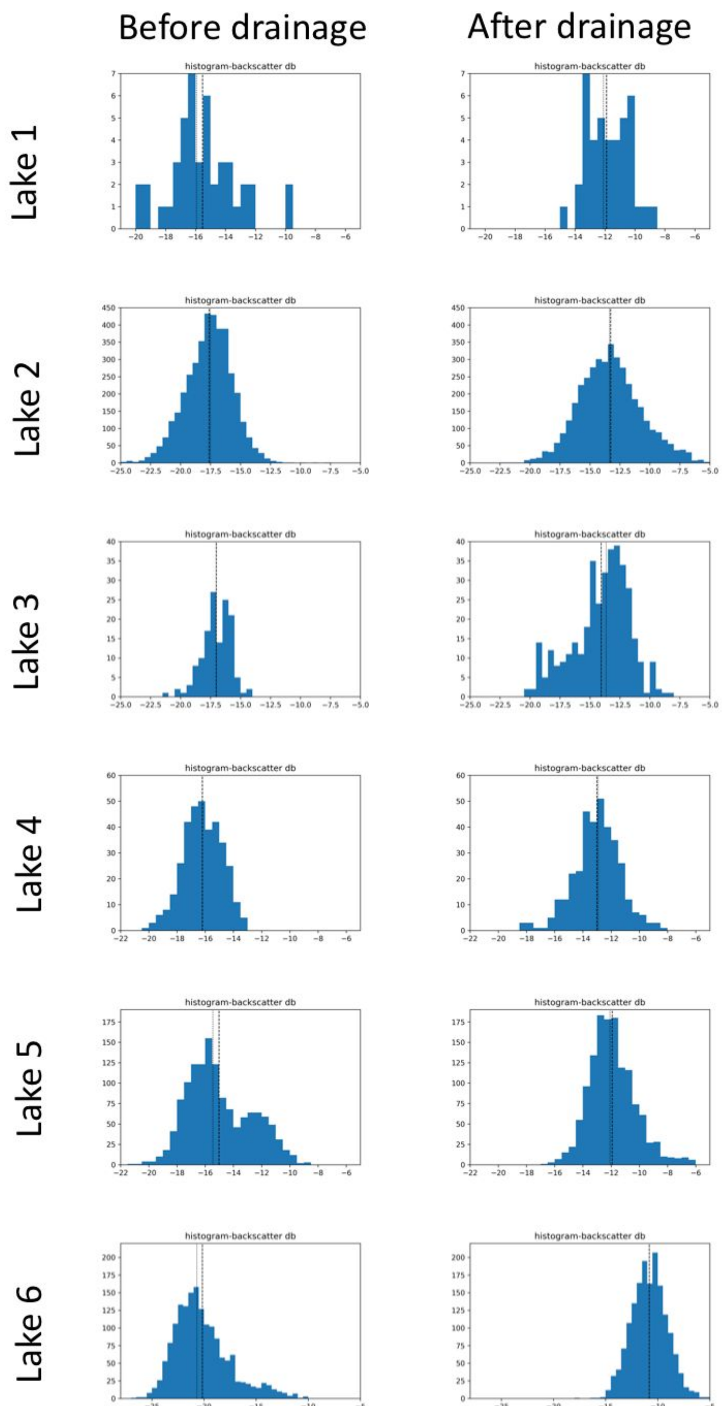
L179: It would be useful to show the extent of the optical lake masks on the Sentinel-1 backscatter images to see over what area the mean change in dB is calculated. Also, might a median value be less prone to the influence of outliers?

We agree that it would be useful to include the lake mask boundaries on Figure 3 and will add them. A draft edit of this figure is here and also included in the responses to Reviewer 2.



Regarding using the mean vs median dB. We have checked the frequency distributions of dB for several lakes and they are normally distributed, with the mean and medians being the same value or only very slightly different. Please see distributions for the drained lakes below, showing the mean (dashed line) and the median (dotted line). We propose,

therefore, to stick with our use of the mean and can justify this by summarising the above in the text.



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

We will remove this sentence in response to the next comment

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.

We will remove this sentence as the reviewer suggests

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

Thank you for spotting this. The sentence does not belong here and will be removed.

L198 (subtitle 3.2): It would be useful to state in the section title what you are confirming - ’Confirmation of winter lake drainage. . .’

Thank you. We agree. The section title will be changed to ‘Confirmation of winter lake drainage by optical imagery’.

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.

Yes, using photoclinometry we only calculate the elevation change within the lake - so upto its shoreline. We will clarify this in the text. See also our reply to comment L157-160 above - we hope the method will be clearer with the addition of the extra diagram. Note also that the recalculations of the lake depths using the optical imagery with the last available image from the previous summer have resulted in slightly deeper mean lake depths (see our new Table 1 above) which bring them slightly closer to the mean depths calculated using the photoclinometry. However, it is still the case that the photoclinometry method of lake depth calculation produces lake depths that are bigger than those produced using the optical band method , > 2X for Lake 2, around 1.5 X for Lake 5 and nearly 2.5X for Lake 6.

We mention likely reasons for the discrepancies in lines 172-175 and also lines 294-303. These are all to do with errors in the two techniques of course. We propose to remove lines 172-175. Around L219 in the results we propose to say that possible reasons for the discrepancies will be discussed below in the Discussion. Then we will ensure that the errors in both the optical band method and the photoclinometry method and the likely reasons for the differences in lake depth calculations are discussed fully in the Discussion around what is now lines 294-303. We will quantify the depth errors in the two techniques with reference to previous literature. From Pope et al (2016) we estimate error using the optical band method is 0.46 m and from Pope et al (2012) we estimate error using the photoclinometry

method is 1.61. Please see our responses to Reviewer 2's comments for 'Table 1' and 'L 218' for derivation of these errors.

Finally, please note that these calculations of lake depth are subsidiary to the main point of the paper, which is to document winter lake drainages (rather than quantify precisely the volumes of water drained). These two additional 'tests' support the SAR backscatter changes by showing: i) that water depths were shallower in the subsequent summer than the previous summer; and ii) that surface elevation dropped over the winter.

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

Agreed. We will change the text as suggested.

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.'

Thank you. We agree and will alter the text as suggested.

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

This is a good idea and we will reiterate briefly what we mean here and how we used the z-score to identify <u>large</u>, <u>anomalous</u> and <u>sudden</u> changes. The point here, of course, is that we also need to ensure the changes are also <u>sustained</u> to identify lake drainages correctly.

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?

We would like to keep the order of sections 3.3.2 and 3.3.3 as this is the order in which the methods are done and the images are processed (equivalent to methods sections 2.1 and 2.2). and the method proceeds.

We will add a bit more to the end of the methods section (2.2), when we talk about using HV polarisation data to image shallow subsurface lakes, that HV data penetrates several metres through the surface, including snow, firn and any nascent lake ice lid. We will add the Rignot et al reference there. We will also add to the sentence on L250 to reiterate that we're using HV polarisation data, which is sensitive to volume scattering and therefore may be detecting water below the surface not seen in optical imagery. See also our response to comment L105 and L106, where we propose to clarify that HV backscatter changes are due to shallow subsurface processes.

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.

We thought of this but decided not to make the assumption that doughnut-shaped or other irregularly-shaped lakes necessarily contained water beneath a snow/ice lid. We wanted to focus the analysis of backscatter change solely on those areas which irrefutably showed evidence for deep water in the optical images. Using the Otsu thresholding method to 'fill in' lake interiors would have dampened the backscatter change signals we found if, in fact, those areas were not actually part of a lake. We would then have had 'less luck' in finding lake drainage events.

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).

Yes we will change the text accordingly and refer to "a few metres"

L272: Be clear that this is temporal frequency

Thank you, yes, we will add the word "temporal" to refer to "temporal frequency" here.

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

Agreed.

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.

As per the earlier comments, we have corrected the area, depth, and volume calculations presented in Table 1 to show quantities based on the <u>last</u> available Landsat-8 image before freeze-over.

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.

Please see our response to comment for L219. We will add error estimates to our calculations of water depths based on both the optical band and the photoclinometry methods. We will clarify why the optical band method may underestimate water depths (crucially there is a depth threshold beyond which light attenuation is unaltered - Pope et al 2016; Williamson et al, 2018) and why the photoclinometry method may overestimate water depths (differences in the date of the DEM and the dates of the imagery used to calculate the slope-reflectance relationships; and shadowing in the lake basin not seen

outside of the lake basin introducing error in slope calculations inside the lake basin when using an empirical relationship defined for areas outside the lake basin).

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.

Both of these things could be done but we think they are not relevant to and would therefore detract from the main purpose of our paper. The main point of our paper is to provide what we believe to be the first method for identifying automatically lake drainages using changes in SAR backscatter within lake basins. This has not previously been reported in the literature. Furthermore, we have applied the method and identified winter lake draianges. This phenomenon has not previously been reported in the literature either. We wanted to verify our method using other remote sensing techniques, which we have done using available optical imagery in two different ways. First, we have shown that water depths in the lakes prior to winter drainage in the previous fall are greater than those after drainage in the subsequent spring. Second we have used photoclinometry to show that there is a collapse in the lake surface elevation over the winter. In response to a comment by both referees, we have also used 2m resolution ArcticDEM strips to verify elevation change associated with the drainage of Lake 6 (see below). So providing a new method, applying it, and verifying it is the purpose of our paper.

The calculations of water depth and volume are very much a subsidiary part of the paper, but we provide these for general interest.

We think the referee is implying that dB values of SAR imagery (presumably imagery collected at the same time as the first available optical imagery the following spring) could be used to determine whether there's been any lake filling between the time of the winter lake drainage and the time of the 1st available optical image in the spring. This could be done but it would still not allow us to adjust the optically derived lake depth to allow us to get a better estimate of drained lake volume. The same procedure would have to be applied between the last available optical satellite image the previous autumn/fall, and the time of the lake drainage to determine whether water entered the lake (or froze in the lake) over the intervening period. Again, we would not be able to quantify the volume of water involved.

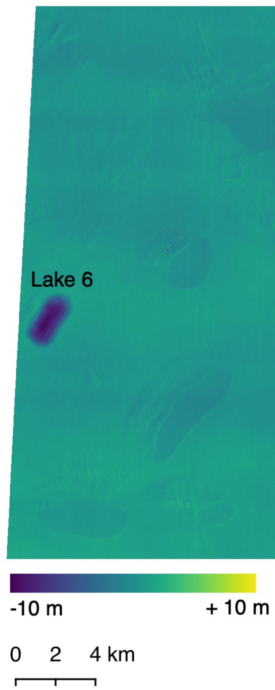
The referee also talks about using RACMO to adjust the lake volumes determined from optical imagery. What we assume he's thinking about here is that runoff into the lake basin between the time of the lake drainage and the time of the first available optical image could be used to adjust the lake volume derived from the optical image according to how much extra water may have flowed into the lake during the spring. Again, presumably the same would need to be done between the time of the last available optical image the previous autumn/fall and the time of the lake drainage in order to adjust the lake volume derived from the autumn/fall optical image according to how much extra water may have flowed

into the lake. What would be required here, is actually a surface hydrology routing model driven by the runoff output from RACMO. This, we believe, is way beyond the scope of this paper. All we are trying to do in the final section of our paper is use independent evidence to verify that a winter lake drainage occurred. We could leave it there but we thought it would be useful to obtain first order approximate values for the volume of the lake drainage event, which we do. And we discuss the errors associated with the derived volumes.

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

Thank you for this suggestion. We have examined the ArcticDEM time-stamped 2 m data strips and a satisfactory pair of 'before' and 'after' images exists only for Lake 6. In this case, a marked difference is shown in Lake 6 surface elevation before and after drainage. Using ArcticDEM 2 m strips from 21 September 2016 (before drainage) and 12 March 2017 (after drainage), we calculate the elevation difference (after minus before) seen in the figure below. If we mask this by the lake mask for Lake 6, we get a mean before/after depth difference of 2.17 m. Note this compares with the mean depth derived from the optical band method of 1.41 m (new Table 1 - see above) and that from the photoclinometry method of 3.38 m (Fig 7 and stated on L219). Note also that this photoclinometry-derived value is less than that quoted in our original manuscript (4.04 m) where we had not masked the lake according to our optically-derived maximum composite lake mask. For Lakes 2 and 5, the optically-derived lake masks are the same as those over which we apply the photoclinometry method. For Lake 6, the optically-derived lake mask is smaller than than over which we apply the photoclinometry. To compare with the optically-derived mean depth estimate, we must crop the photoclinometry-derived and the ArcticDEM-derived depth estimates. We will adjust our manuscript in the relevant places to explain this and make the correct comparisons.

The Figure below is a draft. We propose to add a figure to our paper for Lake 6 which is similar to the current Fig 7. So it will have 3 panels, elevation change and hillshades of the before and after ArcticDEMs for lake 6 and surrounding area. We will adjust the colour bar to be the same as that in Fig 7.



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

Will will change the line to read ". . . in the ArcticDEM 5 m mosaics."

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

Agreed. We will add 'caused by' lake drainage events.

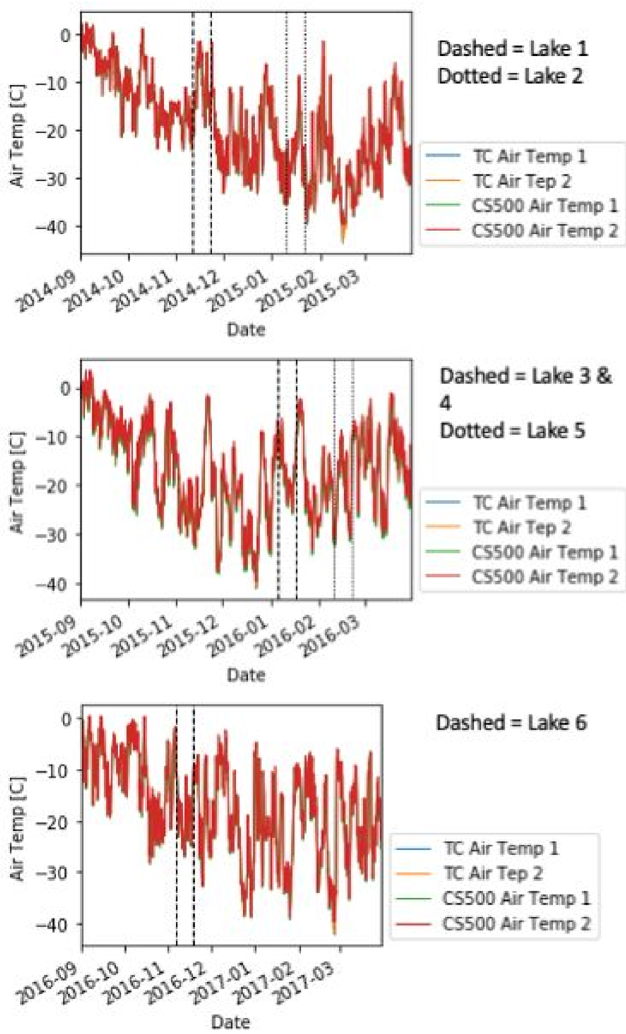
L313: What about short sharp melt events over winter? Have you looked at any available meteorological data? Also do you detect a reduction in backscatter for the non lake surface at the same time the lake backscatter increases? This might indicate a small amount of surface melting that might have an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

We had not looked at meteorological data to see if there's evidence for short melt events coinciding with lake drainage events during the winter. But following the referee's suggestion, we have examined the Swiss Camp air temperature record for the 6 month (Oct-March) periods 2014/15, 2015/16, and 2016/17 covering our 3 winters (see the Figure below, where the 12 day periods during which lakes drain are indicated by the vertical lines).

As you can see, there is no clear evidence that the 6 lake drainage events are associated with especially large increases in air temperatures to above zero that would be indicative of melt events. The only exception might be Lake 1 where there is a large rise in temperature

from -21 to near zero and the highest temperatures since early-mid Oct. But for the rest, air temperatures are either rising but not to above freezing, falling, or fluctuating. Furthermore there are other larger rises in air temperature sometimes rising to zero at other times of the year that are not associated with lake drainage events.

Given this, we do not propose to include this analysis in our paper, but we can in Supp. Mat. if the referee or editor thinks it would be helpful.



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

We think the referee has misunderstood what we're trying to say here, which we agree is not very well articulated. We are not suggesting that we might see the effect of a single lake drainage triggered speed up in the MEaSUREs data set. We are using the MEaSUREs velocity field to see if the locations of lakes are in particularly fast flowing areas of the ice

sheet or areas of high strain rates which they do not seem to be. We propose to rewrite this section to make it clearer.

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.

We will change the text to read "No pattern of lake locations and speeds seems to be visible, although our sample size is small and more evidence is needed to examine this possible association further".

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

We agree, and given the referees previous comment about small sample size we do not think it worth performing these calculations and so we propose to delete these sentences.

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.

We will change the text to: "These concurrent drainages support the observations and modelling of Christoffersen et al. (2018) where the drainage of one lake creates localised ice acceleration, which is transferred via stress gradients to other areas triggering other lakes to drain". Alternatively, they may indicate a larger scale ice movement that triggered both events simultaneously."

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

Agreed. We will change the text to: " We find six winter lake drainage events across a study site containing approximately 300 supraglacial lakes".

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

Agreed. We will add 'supraglacial' after '300'.

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

Agreed. We will make these changes to make the graph clearer.

L148: missing space between value and units

Agreed. And we will check the entire document for this.

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

Agreed, we will correct this to read Appendix A3

L230: ‘event’ should be ‘events’

Agreed. We will edit the text to ‘events’.

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

Agreed. We will edit the text to ‘false negatives’.

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

Agreed. We will edit the text to ‘false positives’.