Dear Editor and Reviewer,

We would like to thank you for your detailed feedback. We address each of the specific comments below.

The major issue with this manuscript raised by reviewer is the differentiation between subglacial lakes and supraglacial lakes and the processing scheme of the ArcticDEM strips. Considering the previous suggestions, here we summarize the major changes to the revised manuscript:

1) ArcticDEM processing: In this iteration, we only used the strips that provided the co-registration information against ICESat to ensure data consistency, and excluded the strips without this information. In addition, we produced ArcticDEM data by only using the pixels that overlapped with the ICESat-2 ATL11 data to construct a time series of lake elevation profiles and its corresponding elevation anomaly. We removed the flat spots in the ArcticDEM period where the elevation profiles could not be corrected. This DSM quality control leads to fewer DSM tiles, and 6 lakes were not covered by high-quality DSM so were classified as ‘unconfirmed lakes’ (Table S2). However, we still have two datasets to distinguish subglacial lakes and supraglacial lakes, including elevation profiles during 2009-2020 (ICESat-2 period), and the combined elevation anomaly during 2009-2020 (see all our data for each lake provided in Figures S4 to S21).

2) Differentiation between subglacial lakes and supraglacial lakes: We adapted our criterion from large elevation changes to no abrupt elevation changes because supraglacial lakes are characterized by a seasonal fill-drain pattern, whereas subglacial lakes tend to fill over multiple year. For flat surfaces, we used the Watta algorithm to correct the water bottom during the ICESat-2 period, and removed flat spots (which cannot be corrected) during the ArcticDEM period. The addition of elevation profiles from the ArcticDEM period increased the data volume, and therefore the confidence level for discriminating supraglacial lakes. We removed lakes that exhibited sudden elevation changes or that were mainly characterized by flat surfaces, and only retained lakes with profiles exhibiting characteristic patterns like Figure 1 b/c. After this lake confidence-level re-classification, 18 lakes with high/medium confidence level were retained.

Overall, our interpretation of active subglacial lakes of the Greenland Ice Sheet combines ArcticDEM and ICESat-2 to deal with the challenge of discriminating subglacial and supraglacial lakes. We recognize there is some uncertainty (as captured in our medium and high confidence levels), but this study represents a step-forward with the addition of 16 new active lakes (18 in total).

On behalf of all the authors,
Yubin Fan

Reviewer #1

The authors have given a revised manuscript detailing the detection and monitoring of subglacial lakes using a combination of ICESat-2 data and the ArcticDEM. Overall, the manuscript is improved from its previous iteration, with text and figures that are easier to understand. I do have a few suggestions and clarifying questions that I would like to see addressed before it is ready for publication:

We thank the reviewer for the helpful feedback, we are appreciative of his or her help and time.

Page 3, Line 67: Small nitpick, but I suggest being specific here and noting that ATL06 measures land ice height.

Response:

We have specified ‘land ice elevations’ here.

Page 3, Line 79: Since you are using ATL03 to identify supraglacial lakes, I suggest giving the full name of the product (Geolocated Photon Data) and giving a bit more detail on what is in ATL03 data.

Response:

We have added the full name and some details of the ATL03 products here.

Page 3, Line 88: Just to make sure, these published subglacial lakes were found using the ArcticDEM?

Response:

Known active subglacial lakes were detected by ArcticDEM ice-surface elevation change, and the stable lakes were detected by RES data. We have added the information in the revised manuscript (Page 3, Lin 90-91).

Page 4, Lines 103-104: How was it determined if other factors caused the elevation anomalies? I imagine that it could be difficult to distinguish between lakes and rough topography.

Response:

Displacement of the ICESat-2 tracks has been corrected by ATL11 product, and the slope generated by the mosaicked 100-m ArcticDEM product was used as a topographic reference.

Page 4, Line 107: ICESat-2, not ICESat. Also, what exactly was corrected from the DSMs, and using what metadata?

Response:

The correction parameters have been provided in the metadata of each DSM strip, and the offsets were obtained by the co-registration between each DSM and ICESat. These values were not calculated by this paper.

We rephrased the sentence as follows ‘We only used DSM strips where correction vectors obtained by the co-registration between filtered ICESat altimetry data were provided within the metadata. (Page 4, Line 107-109)'

Page 5, Line 134: If you define ATL06 on Page 3, Line 67, then it will not be needed here.

Response:

We have moved the name of ATL06 product to the Data section.

Page 6, Line 170: 2 km (spacing)

Response:

Accept and revised.

Figure 1: I am assuming that "pt" refers to the pair tracks, but what do the numbers indicate?

Response:

The numbers mean the order of pair tracks. We added the explanation here.

Figure 3: I notice that stable lakes are generally found either on the eastern part of the ice sheet or on the northern margin. Is this a coincidence? I would like to see the authors' interpretation.

Response:

We assumed that it was caused by the different ice thickness and different surface mass balances. High accumulation rates and thick firn limit the amount of surface-derived water that reaches the ice bed in the eastern part of the ice sheet, so the water budget of lakes in these regions are hard to change. We have explained the reason in the manuscript (Page 8, Line 220-221, Line 225).

Figure S3: It is interesting that a large lake was found, but I am not sure what unique information is provided by this figure. Was the drainage rate (or lack thereof, looking at 2013-2017) surprising for a lake that large? If not, then I would consider removing this figure.

Response:

We have removed this figure in the supplementary information.

Figure S4: There is a caption here, but no figure. Is the figure missing, or was it removed?

Response:

This figure has been provided in the supplementary information.

Reviewer #2

The authors are making progress on this study and have fixed some of the material that I objected to in the previous versions. I'm still not sure about what remains, and I think the authors need to spend some time thinking about the quality evidence that they have presented, and whether they, as referees of an article by a different set of authors, would be persuaded by the arguments and data they present.

We thank the reviewer for the helpful feedback, we are appreciative of his or her help and time.

I remain skeptical of the authors' differentiation between subglacial lakes and supraglacial lakes. The authors claim that the use of ATL03 and the Watta algorithm lets them detect elevation changes even when there is water in the lake, but they don't make any distinction in the text or in the tables as to which elevation differences were calculated using the ATL03/Watta method. The one example they show of ATL11 and ATL03/Watta is in figure S1, which is not discussed in enough detail for me to be able to understand how they used the data, or what they thought was happening in that example. I had to cross-reference the coordinates on the figure with table S2 to figure out that this was HAYES_GLETSCHER_N_NN01. Then, looking at table S4, I could see that the authors listed elevation anomalies that almost certainly were measured on the floating ice on top of the lake alongside the elevation anomalies measured when the supraglacial lake may have been empty, and alongside the ATL03/Watta anomalies from August 2019.

Response:

We first added a column 'Watta correction?' in Table S2 to distinguish the lakes corrected by the Watta method. The Watta algorithm does not only estimate depth estimates, but can also provide lake characteristics (the presence of refrozen ice at the surface) (Datta et al., 2021). For each potential lake, we manually checked the elevation profiles and identified the flat surface. We recorded the data acquisition date and downloaded the corresponding ATL03 photon data.

We used a Figure to show how ICESat-2 can penetrate through the water column of supraglacial lakes to measure the lake bottom (Figure 2 in the manuscript). We used example without an ice lid here (Lake NIOGHALVFJERDSFJORDEN01) in the new Figure S1.

The use of the ATL03/Watta algorithm is not possible for the ArcticDEM data. This means that the time series of elevations from mid-2018 and earlier are likely measuring changes in supraglacial water, or in lake ice atop supraglacial water. Unless the authors present good evidence to the contrary, this should be the assumption for what is going on in this earlier part of the record. As a result, examination of the ArcticDEM record does not confirm that the elevation anomalies are subglacial lakes- it just confirms that there is elevation variation in the past that continues into the ICESat-2 period.

Response:

It is true that the ArcticDEM record is not direct evidence of subglacial lake activity and we have added a sentence to state this effect (Page 4, Line 121-122). However, the extension of the elevation change record can give us a more comprehensive picture of the patterns of elevation changes (e.g., whether the elevation changes were abrupt), which was critical for discriminating subglacial lakes from other processes. In addition, we can also identify how many time periods have flat surfaces for one lake.

In their rebuttal the authors show three examples where they claim that the irregular surfaces demonstrate that they are not measuring supraglacial lakes. I don't understand why they make this claim—the first example (figure R1a) very clearly shows a supraglacial lake that has filled and drained seasonally. When water is present, the surface is not perfectly flat, but the irregularities could easily be caused by a rough lid of floating ice, or by spatially variable laser-light penetration into water and/or detector saturation effects from a bright water reflection.

Response:

Figure R1 (a) may be a misinterpretation of a subglacial lake signal. We adapted our criterion from big elevation changes to no abrupt elevation changes over time. We reclassified the lake confidence level and removed any lakes that look like Figure R1 (a) (i.e., contains abrupt elevation change) from our dataset.

Figures R1b and R1c are much better examples of potential subglacial lake activity, but having seen the authors misinterpretation figure R1a, I am very worried about the quality of the interpretations of other data in the manuscript, and I would encourage the editor to request that the authors present the height profiles interpreted in the study one-by-one in the supplemental material to a revised manuscript. This would give the authors the opportunity to present the ATL11 tracks with the corresponding points sampled from ArcticDEM to provide a long-term record of change for each of the lakes. I suspect that in many cases, this would show that the ArcticDEM data sample flat surfaces (i.e. supraglacial water) during the high stands for many of the lakes.

Response:

We have recalculated the mean elevation within the lake and its buffer, and the corresponding elevation anomaly during the ArcticDEM period (provided in Table S4) by only using pixels sampled by ATL11 data. We further provided the elevation profiles in Figures S4 to S21.

The authors claim to have looked at Landsat imagery to confirm or deny the presence of liquid water in the lakes and, in their rebuttal, show an image for Academy05 that does not show much water on the surface. However, at the time of this image, Academy05 was at a low stand relative to the ICESat-2 measurements, so if it is a subglacial lake, we wouldn't necessarily expect to see water at this time. Further, the landsat data can't rule out floating ice for the lake.

Response:

We agree that we cannot rule it out from the Landsat imagery alone. Academy05 and other lakes showing the same case were eliminated in this version because they did not show gradual elevation change.

The method for combining ArcticDEM and ICESat-2 time series seems to have the potential to generate nonsensical time series. The ICESat-2 profiles sample a small part of each lake basin, while the ArcticDEM data sample the whole basin, which means that a nonuniform pattern of filling and drainage (subglacial or supraglacial) will produce different values for the two datasets that are likely not comparable. I suggest sampling the ArcticDEM DEMs at the locations of the ATL11 measurements to construct a self-consistent time series, and investigating the extent to which these self-consistent time series agree with the full-basin records derived from ArcticDEM. If they don't agree, the two should be presented separately, not combined as they currently are.

Response:

We agree that the different sampling between ICESat-2 and ArcticDEM may lead to bias in the long-term elevation series. We recalculated the mean elevation within the lake and its buffer, and the corresponding elevation anomaly during the ArcticDEM period (as provided in Table S4) by only using pixels that can be sampled by ATL11 data (Page 5, Line 130-133). We further extend the elevation profiles along the ICESat-2 track to 2009 to increase the confidence level of the detected lakes.

In my previous review, I pointed out that the spatial pattern of surface change was much more irregular than what we have seen in Antarctic subglacial lakes. In their rebuttal, the authors contend that under thin ice, the pattern of change associated with a subglacial lake can have sharp gradients, and cite two studies that looked at subglacial lakes that were under thin ice at the edges of glaciers. However, most of the lakes in this study are far from the edge of the ice sheet, and many are under ice of considerable thickness. The authors need to evaluate the ice thickness of their lakes and consider whether it makes sense that there would be large spatial variability for the locations they are considering.

Response:

We interpolated Bedmachine v5 ice thickness at our potential lake locations (Table S2, column 'Thickness'). The ice thickness for the lake in the Hodgson paper is only about 50 m (their Figure 4), which is much thinner than the vast majority of ice beneath our lakes. While the transition may not be as smooth as for deep subglacial lakes in Antarctica (more than 2000 m), we might expect it to be smoother than that shown in Fig. R1a.

In their response to my comment about uncertainties in the Watta algorithm, the authors again seem to interpret an elevation difference between a filled and a drained lake as evidence of subglacial lake activity (figure R3). Even using the Watta algorithm, most of the profile for 5 August 2019 is on floating lake ice, and the Watta algorithm only measures the bottom of the lake in a couple of small sections where the edge of the floating ice has melted. In these places, the bottom elevation is fairly close to the profiles from 2 Aug 2020 and 1 Nov 2020. The elevation change between 2 Aug 2020 and 1 Nov 2020 is fairly substantial (+5-10 m) but this is in an area where snowfall can be heavy, and it is not implausible that a local basin in the ice-sheet surface could trap a considerable amount of snow.

Response:

We used a Figure to show how ICESat-2 can penetrate through the water column of supraglacial lakes to measure the lake bottom (Figure 2). We used example without an ice lid here (Lake NIOGHALVFJERDSFJORDEN01) in the new Figure S1.

The authors quote the Fair et al study to say that the Watta algorithm can only measure ~7 m water depth, and quote Pope et al, 2016 to say that lakes are shallow (<10 m), but their own figure S1d shows a lake that is clearly more than 15 m deep (I'm assuming they too the refractive index of water into account in interpreting the apparent depth in S1c). These assumptions don't seem to be valid and should not be relied upon.

Response:

It is right that supraglacial lakes are not necessarily shallower than 10 m. Datta et al. (2021) detected 5 lakes that are 10-15 m depth (supplementary table in their paper). In addition, Hsu et al. (2021) used the ATL03 data to derive water bathymetry for lakes (depth < 20 m). Therefore, ICESat-2 has the potential for detecting some surface lakes that are deeper than 7 m. We rephrased these statements in the revised manuscript.

One thing I don't see in the manuscript is much critical assessment of the data. An example of this is the left panel of figure 1, where the authors plot a time series of elevations from Academy_01. This time series shows a gradual gain in elevation from 2012-2019, followed by a decline after 2019. The time series, however, includes several large upward and downward spikes in elevation, that are not explained in the text or in the caption. How do the authors interpret these spikes? I would suggest that they most likely represent errors in DEMs, but I don't see that the authors recognize this, or that they acknowledge the possibility that other sharp features in time series for other lakes might be the result of DEM (or ICESat-2) errors. The authors need to acknowledge that the data that they are working from are fallible and need to explain how they differentiated between errors in the data and real signals.

Response:

We used a Hampel filter to remove the outliers in the time-series, and we acknowledge that some errors may remain particularly with the ArcticDEM. However, the long-term trend of lake activity (e.g., quiescent at high stand) can still be identified.

Throughout the manuscript, the authors present elevation changes normalized to rates of change. This does not seem appropriate for changes that are episodic (i.e. seasonal drainage and filling of lakes), and especially in table S3 it makes the data difficult to compare against each other. Unless there is a good reason to the contrary, most of the changes should be presented as elevation differences, not elevation rates.

Response:

We added a column of elevation range (the difference between maximum elevation and minimum elevation) in Table S2 to describe the magnitude of elevation change during the ICESat-2 period. We have changed the elevation-change rate to elevation differences in Table S3.

Figure 1 presents a really unusual lake as if it were a typical lake for the study. This lake is very large compared to the others in the study, and has a large, obvious, subglacially-driven change that is not typical of the other lakes, where the subglacial-vs-supraglacial difference is much less clear. It would be a much better use of space to present one or more ambiguous cases, and explain how each was interpreted, especially as the authors are claiming to make the very difficult (arguably impossible) distinction between change in supraglacial lakes and change in subglacial lakes paired with supraglacial lakes.

Response:

We have presented different scenarios of subglacial-vs-supraglacial difference in Figure 2, and have published all of the elevation profiles and relative elevation anomaly time-series in the Supplementary section (Figures S4 to S21).

I don't understand the time series presented in figure 2. There don't seem to be enough points in the right-hand column relative to the number of ICESat-2 measurements in the left-hand column. Further, the profiles in 2a seem to show the lake filling, while the time series in 2b seems to show the lake draining during the ICESat-2 period.

Response:

We have checked all data in the elevation profiles and the relative elevation anomaly. We only sampled the ArcticDEM DEMs at the locations of the ATL11 measurements to reconstruct a time series in the revised manuscript.

The authors responded to some of my questions in their rebuttal without making corresponding changes in the manuscript (see the question of which lakes were sampled by RES).

Response:

We have made the RES change. 3 of the 18 active lakes were sampled by RES data from 2017 to 2019, but no classic flat reflections were identified (Page 7, Line 210-212). Lake names are ACADEMY02, HAYES_GLETSCHER_N_NN01, and STEENSTRUP-DIETRICHSON01.

Editor

(1) Many arguments of the paper and discussion around it are related to textual descriptions of geometrical features. I thought that a cartoon explaining some basics of laser-light penetration/reflection from an area with/without floating ice, filled/drained lakes (you name it) would help. It will make all discussions and attributing different features to one or another case not only easier, but also will make it clear which characteristics are key for diagnosis in this and follow-up studies.

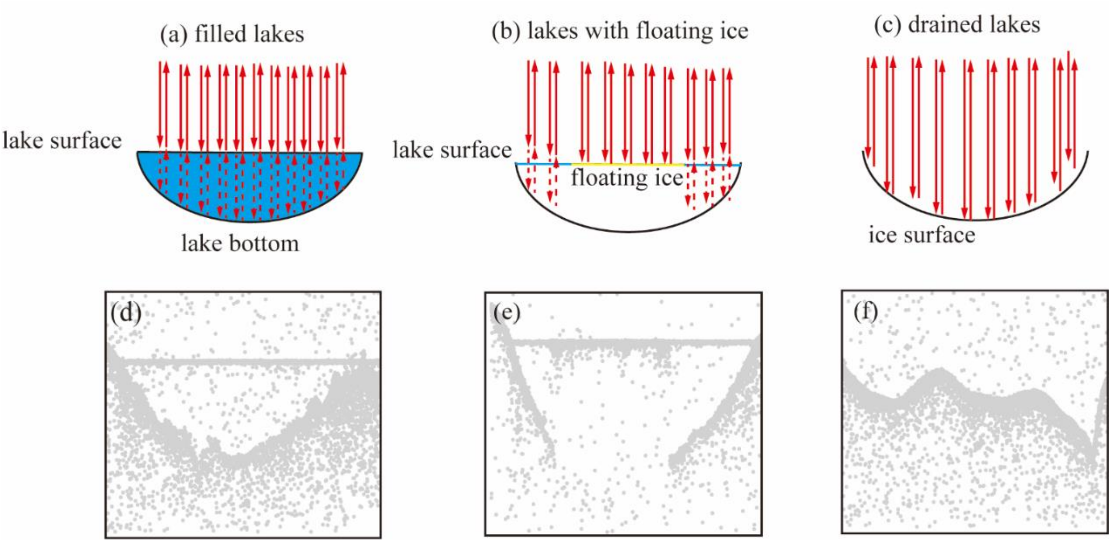

Figure R1. Example of how ICESat-2 can penetrate through the water column of supraglacial lakes to measure the lake bottom. The solid and dashed lines indicate strong and weak reflections, respectively. Figure (b) shows an example of a surface lake with floating ice. ICESat-2 can only penetrate the lake surface, but reflects directly off the floating ice. Figure (c) shows an example of a drained lake that ICESat-2 directly measures the ice surface. The second row (d-f) shows examples of the ICESat-2 photon reflection for the corresponding schematic.

(2) Among minor suggestions by Reviewer #1, it was written that Fig. S4 is missing. However, as I checked your supplementary material file, I did see a histogram, so it was some misunderstanding.

Response:

We have checked the supplementary material file to make sure the elevation-range figure is included.

(3) Line 201 is shown in blue color.

Response:

We have changed the color to black.