Dear editor and referees,

We again thank the referees for their feedback on our manuscript, and suggestions for improvements. We have previously replied in detail to the review and indicated how we would propose to revise the manuscript. We have now revised the manuscript significantly with the main revisions being:

- Expanding and restructuring Section 3 and changed the title to "Data Seat and Data Processing". This includes a more detailed description of TanDEM-X data (Sect. 3.1) including an example of the data (Fig 2).

- Revised and extended description of the CryoSat-2 data (Sect 3.3). Here we

have included a new figure (Fig. 3) showing the waveforms from different times over Lake 1 as an example of what the CryoSat-2 waveform looks like.

- Included a section on "Filtering of CryoSat-2 data" (Sect. 4.1) which includes two new figures: Figure 4 shows the spatial coverage of CryoSat-2 data over Lake 4 for different processing steps and choices. Figure 5 shows processed swath points using different thresholds and the associated waveform over Lake 1, to improve the understanding of the data.

- We have removed the calculations and discussions of basal melt rates (l. 517-523) and Subglacial lake volumes (l. 324-344) from the manuscript.

- General revisions throughout the manuscript.

In the following, we will reply in detail again to all issues raised by the referees and explain how we have revised our manuscript accordingly.
We show the referee's comments in black and our response in blue italic text. Line and section numbers in our reply refer to the revised manuscript including track changes. The line/section numbers in black refer to the first version of the manuscript.

We hope that you will find that the revisions will have improved our manuscript.

On behalf of the authors,

Louise Sandberg Sørensen

Specific reply to referee #1

This manuscript uses a combination of Cryosat-2 laser altimetry and DEMs from SAR and optical measurements to provide detailed measurements over four previously identified subglacial lakes in Greenland, and one prospective, but not previously identified lake. It provides some details of a set of techniques for combining measurements from these sensors, and offers a longer time series of elevation changes for the lakes than previous studies did, with somewhat more temporal detail. The use of Cryosat-2 data allows the authors to measure the depth of the lake under Flade Isblink immediately after its drainage, and finds a depth for the collapse feature that is significantly deeper than that measured in previous studies. I had trouble identifying the scientific questions that the study answered. Since four of the lakes had been identified in previous studies, the fact of their existence is not news, and the behavior documented in this study is not especially surprising.

The objective of our analysis was never to document any surprising behavior of the subglacial lakes investigated. Contrarily, we wanted to investigate whether CS2 SARIn data and TanDEM-X data can be used to improve monitoring of subglacial lake activity in Greenland, and therefore, we chose those lakes that were already described in the literature as these provided the possibility to benchmark our data.
By documenting that these data are actually useful for monitoring the lake activity, they can/should be included in future subglacial lake studies. This has been emphasized in l. 10-12.

The fifth, potential lake identified here is extremely small and is close to one of the previously known lakes, so I am not sure what significance I should attach to its existence.

We agree with the referee that this lake is small, and we do not claim that it will have great importance in the overall hydrological system or in the runoff from that basin. In spite of its small size, we do think that it is important to document our findings since the active subglacial lake activity is one of the very few ways of actually observing what is happening beneath the ice sheet. We also think that the fact that two lakes might be

connected is interesting since this can provide some information about the hydrological pathways.

The study may be interesting to researchers with a deep knowledge of, and interest in, the particular subglacial lakes studied here, but I am not sure how wide this audience is likely to be.

We are sorry to learn that the referee thinks that this study will not be interesting to a larger audience. We do, however, not share that point of view. For the entire scientific community that works on subglacial lakes/hydrology, we believe that it is an important conclusion that additional datasets can be used to improve future monitoring efforts.

The authors suggest that measurements over subglacial lakes have the potential to inform our understanding of subglacial water flow, but I really didn't see much development of this potential in this study. The abstract identifies the demonstration of techniques as a goal of the study, but the technical discussion of the techniques is brief and the presentation of the measurements is not very detailed. I would recommend reworking the study, either to focus on how each of the techniques performed at lake 4 (which had very large relief and elevation change) and at lakes 2 and 3 (which were small, and where the Cryosat-2 data didn't work well), or to try to better understand the implications of the measurements for the subglacial hydrology of the ice sheet.

We see that referee #2 also states that it would be beneficial to rework the manuscript to make the objective clearer. We have re-structured and re-focused the manuscript to include more information on the data, including uncertainties, quality and methods.

Line 34: Should note that this possibility was investigated in some detail by other studies (Stearns 2008, https://www.nature.com/articles/ngeo356) (Smith et al, 2017 (cited in the manuscript) And (Zwally and others, 2002, https://www.science.org/doi/10.1126/science.1072708), and that net dynamic changes after very large water inputs were negligible.

We assume here that the referee is referring to Lines 32-34 and the statement that: "The sudden drainage and outburst flood of a subglacial lake might temporarily affect ice flow velocities downstream from the lake location Palmer et al., 2015; Liang et al., 2022)."

We agree with the referee here, which is also why we have written that it might impact ice velocities. We do not agree however that all the papers listed by the referee conclude that the effect is negligible. Contrarily, some quotes from those papers are:

"Our findings provide direct evidence that an active lake drainage system can cause large and rapid changes in glacier dynamics." (Stearns et al., 2008)

"The indicated coupling between surface melting and ice-sheet flow provides a mechanism for rapid, large-scale, dynamic responses of ice sheets to climate warming." (Zwally et al, 2002).

The Zwally et al (2002) paper focuses on surface melt and not subglacial lakes though, so we do not see the relevance here – even though the surface and basal hydrology are connected. We agree that the Smith et al., 2017 paper describes a case where no connection between drainage and ice velocity is observed, but we do not see how this contradicts our statement in the manuscript.

We have revised the paragraph in the manuscript l 40-42.

Line 88: "Classified" is not the right verb here. "Asserted" might be better

This sentence has been rephrased (l. 108-109).

Section 3-1: Is there any way the selection of thresholds can be formalized? The thresholds selected here seem ad hoc, and it would be useful to discuss how they were chosen.

We agree that the threshold selection seemed ad hoc. We find that the threshold is very case-specific and dependent on e.g., surface conditions (scattering properties), the geometry of the satellite orbit versus lake location, and the geometry of the surface depression. We have included figure 5, which shows the processed swath point from a single waveform and the effects of applying different threshold choices. We have also elaborated on the data processing description in Sect. 4.1.

Line 140: "highly dynamic" should be "rough"

This sentence is no longer relevant and has been deleted (l. 241)

Line 141: Is the incoherent component in the processing, or in the radar reflections?

This sentence is no longer relevant and has been deleted (l. 242)

Line 145: should be “assumed to be representative”

Agree. It has been revised accordingly (l. 246).

Line 145: “were deemed as errors” should be “were assumed to be errors”

This sentence is no longer relevant and has been deleted (l. 247)

Line 146: “Across swath tracks close to the basin rim” should be “swath-processed data from tracks close to the basin rim” .

This sentence is no longer relevant and has been deleted (l. 248)

Line 148: remove commas around “which is removed”

We have revised sentence (l. 252).

Line 183 “vertical alignment” should be “vertical offset”

We have revised accordingly (l. 288).

Line 184: delete “found to be”

We have revised accordingly (l. 288).

Line 197: “but we see” should be “and we see”

We have revised accordingly (l. 302).

Line 201: “such as” -> “including”

We have revised accordingly (l. 308).

Line 211: What is the basin shapefile?

We have clarified, that we refer to the manually delineated basin outline (l. 318).

Figure 1 (and all similar figures)

We assume that this comment is actually about figures 7-11

1. The map extent is too broad to give a useful context for the lake location. Should instead show a context map with the regional topography and the locations of adjacent glaciers in some detail, with a reference map in a separate figure to indicate the locations of figures 1-7

We agree that these figures can be improved. We have included a figure to show the locations of all the lake sites (Fig. 1).

2. Need to provide a color bar for panel c

Agree, we have revised Figures 7-11 accordingly.

3. The yellow lines in panel b are very hard to see

We agree that they were hard to see. We have revised the figures (7, 8, 9, 10, 11) to make the lines more clear and changed the colormap.

4. The range of contrast in the colors in panel b does not really allow the distinction between different CS2 dates. Different symbols should be used to denote different dates.

We have revised the figures to include different symbols with a different colormap.

5. The legend should explain the blue shaded bar

We have revised accordingly.

Line 224: Subtracting the median height does not make sense, as the offset subtracted is will depend on the height distribution of the rim. It would be better to subtract a median height anomaly relative to some reference DEM. Is this what the authors mean to say?

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

Line 226: "cubing the 2sigma": What is this, and why does it give an error estimate? This needs much more detail to explain and/or justify what is done here.

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

Line 228: Need to specify which depths and volumes are used here, and need to connect these, using consistent terminology, with the depths derived from the DEMs and from CS2. Are "the depths" referenced here the depths of the deepest point from CS2?

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

Line 231 / equation 1. How does the derivation of R and V take the error bars into account? More detail is needed.

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

Line 236: It would be useful to demonstrate how R~(t) varies in time based on the available DEM data.

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

Lines 225-236: The methodology here does not seem to capture the true uncertainty in depth (and volume) estimates based on the CS2 data. When there is a large spatial variation in elevations in the DEM data, they are assessed a large error based on the slope and roughness within the relevant part of the lake, but CS2 data generally give a small number of elevation measurements at these times, and are assessed a smaller error. Would it not make sense to apply roughness information from the DEMs to the CS2 data to assess their errors?

This is no longer relevant, since we have chosen to remove the section on Subglacial Lake Volumes from the manuscript (l. 323-343).

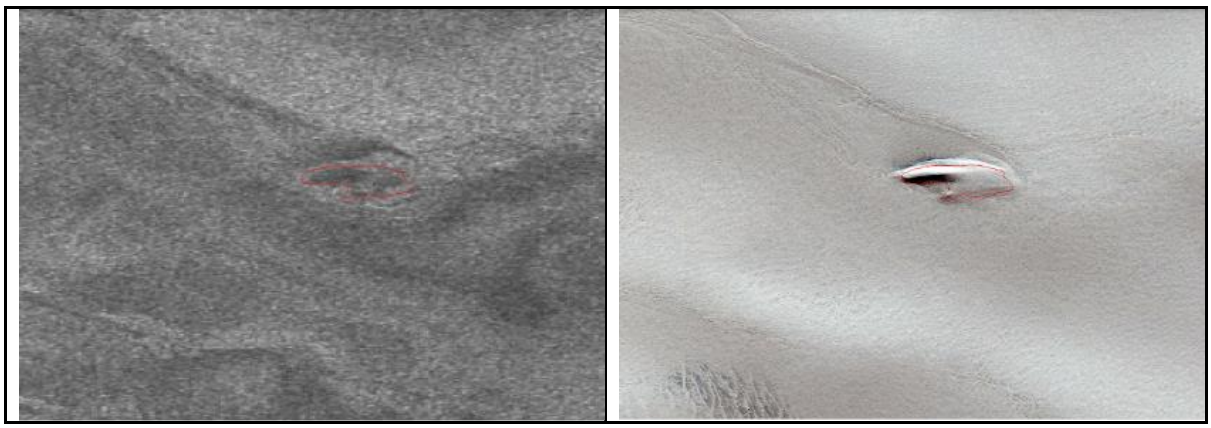
Line 246: add comma after “coverage”

We have revised accordingly (l. 352).

Line 264: It would be useful to explore why CS2 did not provide data over lakes 2 and 3. Were there no footprints that intersected the lake boundary? Was the coherence too low?

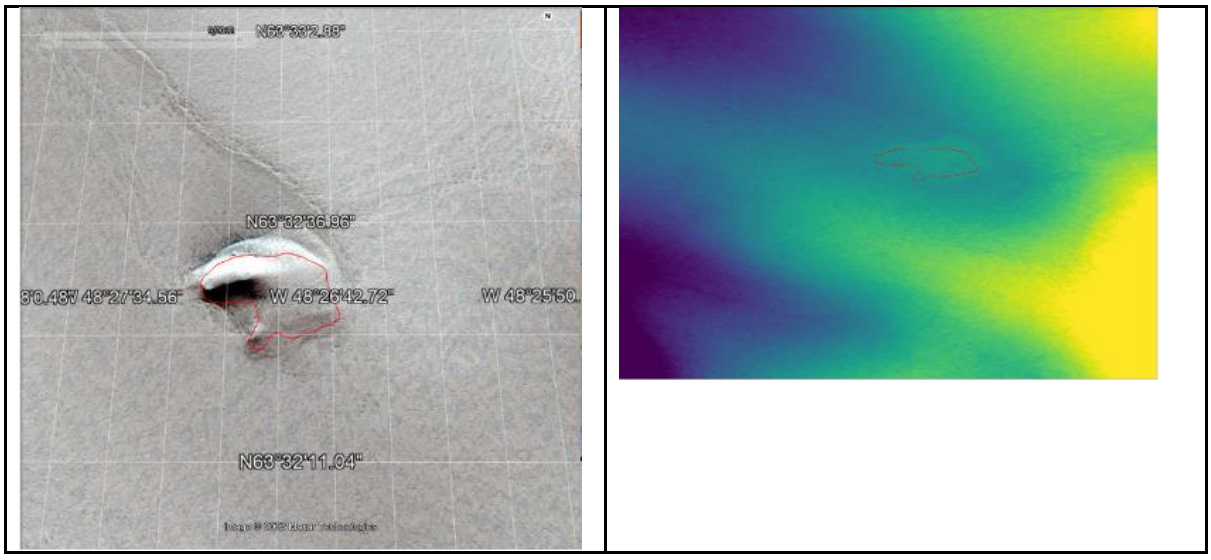
The lack of data is likely due to the fact that the collapse basins are small, making it difficult to differentiate multipeaked waveforms as a reflection from both a depression and a surface. Furthermore, the narrow structure of the depressions also increased the incoherent component of the phase difference, thus making it tough to do a phase unwrapping. We hope that the inclusion of Figures 3 and 5 will help the readers understanding of the challenges associated with obtaining useful data from the smallest collapse basins.

Line 265: Please show the power image from TanDEM-X for early 2011. It would be interesting to know if there are any reflectance features associated with the about-to-drain lake.



T*anDEM-X SAR amplitude image of Lake2 (SouthernLakes) from 20-01-2011 (left) and optical image from Google Earth from 09/2012.*

Interestingly, the subglacial lake and its northwestward-flowing channel appear slightly darker than their surroundings in the TanDEM-X amplitude data. There are other darker structures nearby, so identifying the subglacial drainage structures based on SAR amplitude alone does not seem sufficient. However, it could help to identify and locate them, and we have not included them in the revised manuscript.



left the southern lake (Lake2); right the DEM from 20-01-2011 TanDEM-X acquisition

Line 279: “CS2 point data” :should this be “CS2 swath data”

We have revised accordingly (l. 388)

Volume calculations: Except for Flade Isblink, these volumes are exceedingly small. Compared to lake discharges in Antarctica, they are miniscule, and those Antarctic discharges had almost no effect on ice dynamics. What is the justification for saying that the lakes studied here might be important for ice dynamics?

As mentioned earlier, there are references for how subglacial lake drainage can affect ice velocities.

320: Should compare volume-change estimates against surface runoff estimates from (e.g.) RACMO.

We agree with the referee that a study that includes both estimates of basal and surface melt with the subglacial lake activity would be interesting. This would however require modelling/observations of how much of the surface melt water that reaches the bed, which we believe is outside the scope of the current manuscript. Also, since there is a wide spread in the predicted runoff estimates from different RCMs such a study should include several models (Fettweis et al., 2020). Therefore, we have chosen to remove the subglacial lake volumes and basal melt calculations from the revised manuscript.

358: “shortly” should be “briefly”

We have revised the sentence (l. 477-478)

373: “off-nadir” should be “off nadir”

We do believe that off-nadir is the correct term here (l. 493).

376-384: this repeats material found in the methods section.

We agree and have deleted the sentence l. 496-500, but we have kept the last part (500-502) that emphasizes that we do not take the associated error into account.

378: delete “parameters”

Has been deleted.

387: is “highly active” all that can be determined here? This doesn’t seem like a lot has been learned.

As the focus of the revised manuscript is more on data and methods and less on the geophysical interpretations, we have deleted this sentence (507-511).

Section 6.6

To conclude that the activity of the new potential lake affected the drainage of lake 2, the authors would need to present evidence that it is unusual for water to reach the bed in volumes comparable to those discharged by the new lake. Looking at the images in appendix B, it appears that there is abundant water on the surface of the glacier, and it seems likely that this water often drains through moulins. Why, then, should we believe that the drainages of lakes 2 and 3 are anything but coincidental? Even if they were not coincidental, what specifically does this tell us about the hydrology of the glacier bed that we could not have inferred already?

This is true. Here, we simply want to point to the fact that the timing of the events could imply that they are connected. We do not foresee to do any detailed analysis in this work to support this hypothesis. But we agree that the section needed to be improved by expanding on the information and discussion, and we have done that (Section 6.5).

Appendix A: Why would the basal melt rates be important in this area? Water fluxes from surface melt must dwarf these rates by orders of magnitude. Please consider surface melt first.

We have removed the basal melt plot and associated discussion from the manuscript.

Appendix B:

Figure B1: Indicate the location of this lake relative to lake 2.

We have done this in Fig A2

Also- what is being mapped here? The difference between panels a and b seems to mostly be that in panel B the surface is covered with snow, while in panel A it is mostly bare ice. The interpretation of the change in the collapse basin is not at all clear to me.

Figure B2: There is a lot of variability in surface conditions between these images. The interpretation in the text is not at all convincing.

We agree and have revised the Appendice figures and the discussion of it in Sect 6.5

Data availability: I didn't see a statement about data availability for the CS2 swath-mode data.

We will be happy to make the data available. We will do so on data.dtu.dk and provide the link if the manuscript is accepted.

References

Fettweis, Xavier, et al. "GrSMBMIP: intercomparison of the modelled 1980–2012 surface mass balance over the Greenland Ice Sheet." The Cryosphere 14.11 (2020): 3935-3958.

Palmer, S., Mcmillan, M., and Morlighem, M.: Subglacial lake drainage detected beneath the Greenland ice sheet, Nature Communications, 6, https://doi.org/10.1038/ncomms9408, 2015.

Liang, Q., Xiao, W., Howat, I., Cheng, X., Hui, F., Chen, Z., Jiang, M., and Zheng, L.: Filling and drainage of a subglacial lake beneath the Flade Isblink ice cap, northeast Greenland, The Cryosphere Discussions, pp. 1–17, 2022.

Stearns, L., Smith, B., and Hamilton, G.: Increased flow speed on a large East Antarctic outlet glacier caused by subglacial floods, Nature Geosci, pp. 827––831, https://doi.org/10.1038/ngeo356, 2008 Stearns et al., (2008).

Smith, B. E., Gourmelen, N., Huth, A., and Joughin, I.: Connected subglacial lake drainage beneath Thwaites Glacier,West Antarcticaf, The Cryosphere, 11, 451–467, 2017.

Zwally, H. J., Abdalati, W., Herring, T., Larson, K., Saba, J., & Steffen, K. (2002). Surface melt-induced acceleration of Greenland ice-sheet flow. Science, 297(5579), 218-222.

Specific reply to referee #2

General Comments

This paper combines multiple satellite missions to improve the temporal resolution of ice surface elevation change measurements over 4 previously identified active subglacial lakes in Greenland to provide new constraints on lake volume and evolution. In addition, they find one potential new active lake that might be hydrologically connected to one of the known lakes (although see specific comments). The study is generally well written with some nice figures, and I found the combination of methods to improve the temporal resolution convincing. I did, however, find quite a few minor errors or places which needed further clarification

(see specific comments below), and I agree with the other reviewer that the implications of their findings are currently not clear, and could do with expanding / reworking. For example, could you combine your improved monitoring of recharge rates with your basal melt modelling (expanded to all sites), to make this a more significant component to better explore the role of surface vs basal melt. How do your recharge rates/ drainage rates compare to elsewhere? Can you use your improved timings of drainage to better link to triggers?

As also mentioned in our reply to referee #1 the aim of our study has been to investigate whether CS2 SARIn data and TanDEM-X data can be used to improve monitoring of subglacial lake activity in Greenland. Both referees suggest that the manuscript is reworked to make its aim clearer. We have revised the manuscript to focus on the data and its usefulness in subglacial lake monitoring. We have removed the calculations and discussions on basal melt and subglacial lake volumes from the revised manuscript, and instead improved and expanded the data method section and associated discussion.

Specific Comments

L4 – Antarctic Ice Sheet

We have revised accordingly.

L6 – I think it would be worth mentioning earlier in the abstract that active lakes are typically identified from ice-surface elevation changes to put this point into context.

Agree. We have added the following sentence to the abstract: "Active lakes may be identified by local changes in ice topography caused by drainage or recharge of the lake beneath the ice." (l. 5-6)

L14 – It is odd to mention surface hydrology at the end as this is not discussed in the rest of the abstract.

Agree. We have deleted this sentence from the manuscript (l. 18-19)

L21 – not sure this reference is appropriate here as it focuses on predicting lake locations. Perhaps refer to the Livingstone et al. (2022) study instead.

Agree. This was a mistake. We are now referring to Livingstone et al. (2022) and Fan et al., 2023 instead of Livingstone et al. (2013). L. 26.

L24 – “steeper ice surface slopes”

We have revised accordingly. L 30.

L27 – delete “further”. Your previous points were around different settings not detection.

We have revised accordingly. L. 33.

L30 – the use of e.g. in this sentence does not work that well. Can you combine the first part of this sentence with the second part of the next to provide a more general mechanism for lake drainage?

We have revised the sentence to:

“The lake will eventually drain when filled with enough water to resist the pressure exerted by the overlying glacial load (Chandler et al., 2013), hence a subglacial lake drainage events can be triggered by a prolonged addition of surface meltwater (Livingstone et al., 2022).” L. 36-38.

L33 – I think Palmer look at vertical displacement, but don’t really mention horizontal displacement. It might be better to refer to some of the key velocity studies in Antarctica or Iceland here.

We have changed the reference from Palmer et al., (2015) to Magnusson et al, (2007) and Stearns et al., (2008). L. 40.

L35 – suggest – “Notably, the period of lake recharge following a drainage event provides…”

We have changed this sentence to: "In particular, the period of lake recharge provides information about subglacial water production and conditions at the bed" L. 44.

L49 – should this be InSAR not SARIn?

We made an error here. We have rephrased to: "synthetic aperture radar interferometric (SARIn) mode mode". L. 64-65.

L56 – delete "source of" to avoid repetition of this word.

We have revised accordingly. L. 72.

L59 -this makes it sound like there have just been 4 active lakes identified in Greenland. There are actually 7 – see Livingstone et al. (2019). Worth noting this and rephrasing to justify the four you have chosen.

We have rephrased the sentence (l. 83-84). We have also added the sentence "We have chosen not to include the three subglacial lakes located beneath the highly dynamic Isunnguata Sermia glacier due to the small size of the collapse basins and their location very close to the ice sheet margin (Livingstone et al. 2019)." . L. 87-89.

L66 – this makes it sound like Bowling et al. (2019) identified all these lakes. It would be better to cite the original papers for all these lakes.

We have removed this statement as the relevant papers are cited in each subsection about the specific lakes. L. 83.

L72 – Figure 2a is cited before Figure 1.

We will include a new figure 1 int the revised manuscript, which shows the location of all of the lakes and this is the first figure we refer to. (P. 4)

L83 – is this a subsidence event in 2004? Not clear.

Howat et al, 2015 states that "Drainage occurred in two episodes: a smaller event in 2004 and a larger one in 2011."

We have revised the sentence (l. 97-99) to: "These studies find that Lake 1 has drained both in 2004 (smaller event) and one in 2011 (larger). The 2011 drainage occurred with an unknown rate within two weeks (28 June, 2011 to 12 July, 2011), resulting in the formation of a collapse basin in the ice sheet surface."

L94 – suggest: "… which could indicate some recharge of the subglacial lakes by surface water".

We have revised accordingly. L.117.

L97 – Flade Isblink Ice Cap

We have revised accordingly. L.119.

L105 – clarify whether this was infill of the collapse surface basin or infill of the subglacial lake by surface water causing the ice surface to rise.

We have revised the sentence to:

"The elevation of the collapse basin rapidly increased by 30 meters over the following two years due to inflow of surface water to the subglacial lake, and between August 2012 and April 2013 a topographic bulge appeared in the basin (Willis et al., 2015)." L. 127-129.

L107 – can you quantify the elevation change associated with this event?

Liang et al (2022) observed an elevation change of 10 m. We have added this to the text. L. 129-130.

Section 3 – it would be useful to provide details on the final resolution and vertical/ horizontal errors of the processed datasets.

We have have revised and description of both the TanDEM-X (Sect 3.1) and CryoSat-2 (Sect. 3.3 and 4.1) data in the revised manuscript to hopefully provide the reader with a better understanding of the datasets. This includes addition of four new figures (Fig. 2, 3, 4, 5) of the data itself and the impact of the different processing steps for the CryoSat-2 swath data.

L112 – SARIn has already been defined.

We have revised accordingly. L. 163

L124 – It would be helpful to quantify this change in density.

The density of the swath-processed data compared to the POCA varies and depends on e.g. the thresholds used in the swath processor, and the physical properties and topography of the surface, which affects the waveform. We will add the following sentence to the paragraph (L. 176-178):

"Depending on e.g., the chosen processing thresholds, the physical properties and the topography of the area, the L2 swath processing leads to a 10 to 100 folds increase in elevation measurement compared to conventional L2 processing."

We have included a figure to show the data coverage of POCA and different processing/filtering steps of the CryoSat-2 swath data. (Fig. 4).

L128 – "… thresholds compared to those usually applied …."

We have revised accordingly (L. 191).

L134-138 – Maybe this is because I am a visual person, but a figure showing the raw to processed data points would be really helpful here in allowing the reader to judge the effectiveness of the approach.

Have included a figure (Fig. 5) which shows an example of a CryoSat-2 waveform at Lake 1 together with the derived elevation estimates and color/symbol coded these to show their associated coherence and range bin number.

L143 – not sure why you need the word “apparent”?

This sentence has been revised (L. 240-247)

L145 – “assumed to be representative”

We have revised accordingly. L. 246

L146 – how close in time? This is rather vague and could do with rephrasing.

This sentence is not longer relevant and has been deleted. (L. 247)

148 – would be useful to get a rough estimate – 1%, 10%, 90%? See my comment above, a figure showing the stages in the processing would be helpful here.

We have included a figure (Fig. 4) to illustrate the data coverage and how it is affected by different processing choices and on the filtering. We have further explained in l. 250-253 that it also varies with physical setting and properties of each site.

L160 – “data takes located”? Is there an error here, I didn’t follow this part of the sentence?

The TanDEM section (Sect. 3.1) has been revised (l. 144)

L181 – “the vertical bias.”

We have revised accordingly. L. 285.

L192 – given you correct using local ice flow, it would be useful to know here whether this 100 m in 10 years equates to a local ice flow in this region of ~10 m/yr.

We are not sure what the referee is asking for here.

L198 – “One reason for this is that the…” I don’t really get the point around the size of the lake as surely it is the lake edge that you are tracking.

Correct. We have revised the sentence to: “One reason for this can be that the subglacial lake drains again in the observational period, which could make the potential ice flow less evident since the collapse basin is re-formed over the stationary location of the subglacial lake.” L. 303-305.

Section 4.2 – are these calculations still based on the local difference compared to the basin rim? If not, could these not be influenced by the different penetration depths etc? I don’t really follow the approach to calculating the deepest depths (why take a mean if looking for deepest point) or the use of standard deviation. Is this not just a measure of roughness of the floor of the basin? This needs clarifying.

These calculations are the absolute elevations of the aligned datasets. Since the datasets all are aligned at the rim (section 4.1) they will not be affected by different penetration depths. L. 280-284

We agree that the mean and standard deviation is also a measure of the roughness of the floor of the basin, but these will inherently have an impact on the accuracy of the measurements.

L226 – In other papers error is calculated by multiplying the internal error of the DEM by the lake area both before and after drainage and adding together in

quadrature. What is the basis for your approach, especially as you state that 2 standard deviations is not a true measure of their accuracy?

In the revised manuscript we have deleted the section on Subglacial Lake Volumes.

L260 – does this actually show uplift? Could you run regression analysis over the two periods to calculate the recharge rates more accurately.

No, not a significant uplift. We will revise the sentence to:

"The subglacial lake recharge can be divided into a fast basin uplift of ~13 m/yr in the period 2011-2015, and a period with no significant uplift from 2015-2019." L. 367-369.

L276 – Flade Isblink Ice Cap

We have revised accordingly. L. 385.

Section 5.1 – Some of the text in this section would I think be better incorporated into the figure captions e.g., "To maintain a visually clear plot not all data sets are shown in Figure 5b." This would help the flow of this section while making the figures standalone.

We have chosen to keep the specific sentence in the section 5 but have made some further revisions of the text. We have revised the figure captions in this section (Fig 8-11),

L297 – Although this is the maximum volume measured, it is the minimum possible volume (given you might have missed the period of maximum collapse (i.e. it might have collapsed and then recharged between data points).

We have chosen to delete the section on Subglacial Lake Volumes in the revised manuscript.

L310 – can you quantify this – re. number of data points over X years?

This is very site specific.

L313-315 – There does seem to be some signal of the final collapse and then beginning of the recharge period though during the winter 2011/2012 period. To better test this it would be better to split these components and calculate the recharge for the upwards tick as a rate vs. the summer after.

What we meant was that we do not see that there is a recharge in late 2011 when taking error bars into account. We will rephrase to make it more clear: "Notably, the addition of CS2 observations during 2011/2012 allows us to conclude that no significant recharge of the subglacial lake occurred in the second half of 2011, while recharge is observed throughout 2012." 427-429.

L316 – How do you know it is bedrock? Suggest change to "bed"

We have revised accordingly. L. 431.

L318 – Ok, but could this not be associated with a decrease in filling over time as the area increases (i.e. for a given melt input the rate will decreases because of the basin shape?). I think it is fairly common for recharge rates to slow over time.

Good point. We have rephrased the sentences to: "We further hypothesize that the infilling of the collapse basin after 2014/2015 is likely primarily caused by snowfall and ice flow, and less recharging of the subglacial lake due to the fact that the center of the collapse basin moves away from the subglacial lake as a result of local ice flow. The filling rate will however usually slow over time due to the geometry of the lake." L. 431-437.

L320 – "model estimates of basal melt rates" – ok, based on what data? Need a supporting reference and to quantify. I don't quite see how this point fits with the idea of ice flow/ snowfall. What is the surface mass balance change?

We have removed the basal melt estimates and discussion from the manuscript as part of shifting the focus towards the data and methods.

L321 – Appendix not Append.

This paragraph is no longer relevant and has been deleted (L. 436)

L322 – capitalise vatnajokull ice cap

We have revised accordingly. L. 438.

L330 – A more positive spin would be to give the time span over which the drainage could have happened.

We have chosen to keep the original wording here.

L331 – “drains”

Since we are here discussing two lakes we believe that it should be “drain” and not “drains”? L. 447.

L332 – “spring”

We have revised accordingly. L. 448

L334 – In which case, how do you know whether Lake 2 is actually a supraglacial lake that is filling and draining? It would be useful to confirm whether the 2011 data is associated with surface water or not.

Figure 8(b) shows that the surface over Lake 2 was not flat in 2011, which we assume would be the case if it was a supraglacial lake. We have indicated with light red boxes in Figures 8(d) and 9(d) times where we identified surface water in optical imagery. This explanation has been added to the caption text of these figures.

L347 – please state the infilling rate from this calculation.

We have stated the infilling rate (L. 463-464)

L352 – It would be useful to incorporate the results of Liang et al. (2022) into this discussion as they look at seasonal recharge (i.e. impact of warmer summers on recharge rate, with rates of up to 49 m/yr).

We have added a paragraph about the Liang et al. (2022) results in Sect. 6.3. L. 471-479.

L357-358 – "which shortly affected the local horizontal ice velocity" – needs rephrasing.

We have rephrased the sentence to: "The surface lowering in 2019 is also documented from ICESat-2 data by Liang et al. (2022), who also identified it as a drainage event. They also document that this event caused the ice velocity downstream from the lake to abruptly but shortly increase. " L. 476-478.

L368 – "basins are flat"

We have revised accordingly. L. 488.

L378 – You use SARIn elsewhere.

Yes, but this is correct. For CryoSat-2 the data is called SARIn.

L386 – Ok, but others have shown this in their data, so should acknowledge this.

We have deleted this sentence (L. 507-511).

L387-388 – this needs clarifying? Where is this low pressure region?

We have deleted this sentence (L. 507-511).

Section 6.5 – this seems rather tagged on and is not explained in the methods, and very briefly here. I would suggest introducing earlier in the method section and showing in the results, or deleting.

We agree and the Section on Basal Melt Flux is not part of the revised manuscript.

L402 – There is also a new paper in TCD by Fan et al. (2022) with lots of new active subglacial lakes. Is this one of the lakes in their inventory? https://tc.copernicus.org/preprints/tc-2022-122/

We have included the Fan et al. (2023) as reference. (L. 26, 59-60, 513-515, 552-554).

Section 6.6 – given that this period coincided with a large melt event could these both be responding independently to large volumes of water accessing the bed in this region? More information is needed here to better test this hypothesis.

This is true. Here, we simply want to point to the fact that the timing of the events could imply that they are connected. We do not foresee to do any detailed analysis in this work to support this hypothesis. But we have revised the Section (Sect 6.5) to better support it.

L422 – Greenland Ice Sheet.

We have revised accordingly.

L423 – “investigated elevation changes”
We have revised accordingly.

Figures/ Tables

Given that you correct based on the elevation outside of the collapse basins, I was surprised to see many of the plots with a y-axis of elevation rather than elevation anomaly (relative to the tie points). It would be useful to clarify whether you use similar time periods as cross-over points or how you are able to show these.

Our reasoning for using the absolute elevations rather that relative ones is it could be relevant for the reader to know the actual elevation of the lake site.

Figure 1 – what is the blue shaded bar? (also in figures 2 and 3)

We have revised the figures have added colour bars (Figs. 7(c), 8(c), 9(c), 10(c), 11(c)).

Figures 2 and 3 – what are the red bars? (also true for Figure 7)

This highlights those measurements that are likely connected to the occurrence of surface water. We have added the explanation in the figure captions. (Figs. 8(d), 9(d), 11(d)).

Figure 6 – what is the grey-blue bar around the points? Need to clarify. Why do only (a) and (d) have red lines?

This figure is not included in the revised manuscript.

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