Referee #1

We thank the referee for their feedback on our manuscript, and suggestions for improvements.
In the following, we will reply in detail to all issues raised by the referee and explain how we will revise our manuscript accordingly if we are invited to submit a revised version of it.
We show the referee's comments in black and our response in blue italic text.

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.
That being said, in those cases where the CS2 data actually provides new information about timing or variability, we have explained this.

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 suggest revise the manuscript to include more information on the data, including uncertainties, quality and methods. We will include more figures of the data, inclusing waveforms from different tiems over a chosen lake, and spatial plots of data coverage for each processing step.

We suggest removing the basal melt calculation and associated discussion.

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 <u>might</u> 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 suggest revising the paragraph in the manuscript to emphasize that some studies show a connection between lake drainage and ice velocity and that one found no connection. We will include the suggested references.

Line 88: "Classified" is not the right verb here. "Asserted" might be better
We agree and will revise accordingly

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 seems ad hoc. In our study, we did try to find a more formalized approach but did not succeed. We find that the threshold is very case-specific and is 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 suggest revising sect 3.1 in the manuscript to explain this more clearly.

Line 140: "highly dynamic" should be "rough"
We will rephrase it to "highly variable", as it is not only defined by its roughness.

Line 141: Is the incoherent component in the processing, or in the radar reflections?
We will rephrase it to "… in a larger incoherence in the data."

Line 145: should be "assumed to be representative"
We will rephrase it to "is assumed to be representative"

Line 145: "were deemed as errors" should be "were assumed to be errors"
Agree. We will revise accordingly.

Line 146: "Across swath tracks close to the basin rim" should be "swath-processed data from tracks close to the basin rim"
Agree. We will revise accordingly.

Line 148: remove commas around "which is removed"
Agree. We will revise accordingly.

Line 183 "vertical alignment" should be "vertical offset"
Agree. We will revise accordingly.

Line 184: delete "found to be"
Agree. We will revise accordingly.

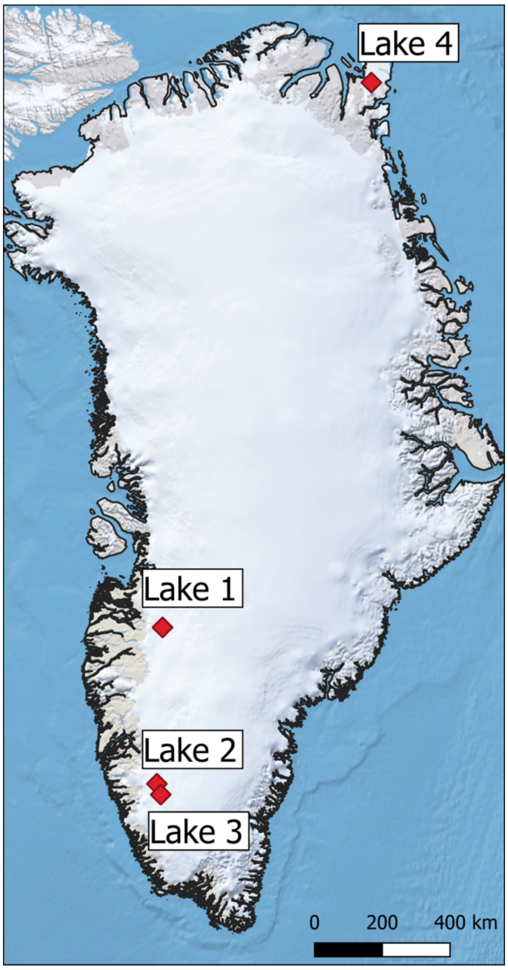
Line 197: “but we see” should be “and we see”
Agree. We will revise accordingly.

Line 201: “such as” -> “including”
Agree. We will revise accordingly.

Line 211: What is the basin shapefile?
Here we refer to the manually delineated basin outline. We will revise to make this clear.

Figure 1 (and all similar figures)
We assume that this comment is actually about figure 2,3,4,5, and 7

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 suggest making one common figure to show all the locations:



Need to provide a color bar for panel c

Agree, we will add this to the new figure (see above).

The yellow lines in panel b are very hard to see

We agree that they are hard to see, but we have tried to plot this in many different ways . We will revise the figure to make the yellow lines more clear.

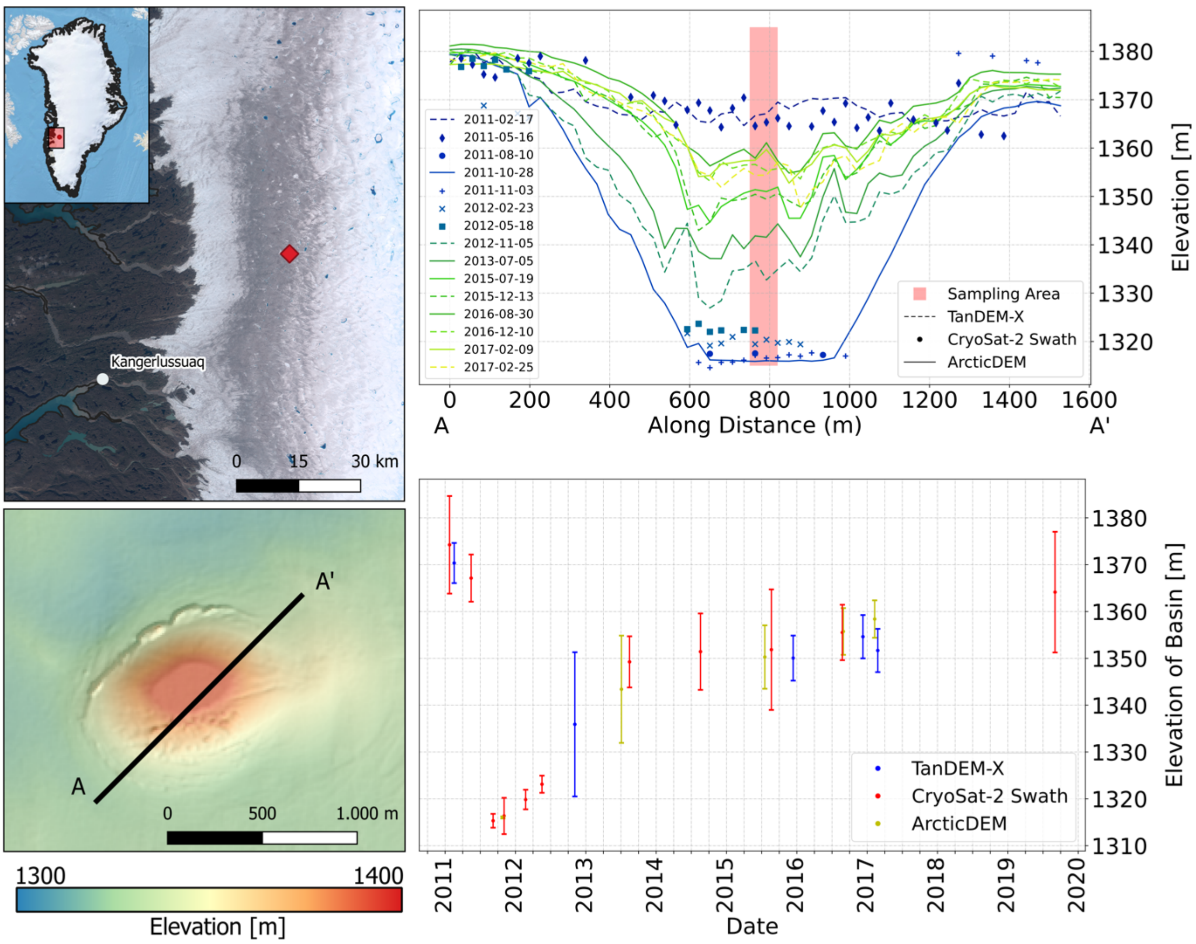
The range of contrast in the colors in panel b does not really allow the distinction between different CS2 dates. Different symbols should be used to denote different dates.

We will revise the figure to include different symbols

The legend should explain the blue shaded bar

We will revise accordingly

Suggested figure revisions:



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?
We agree that it is indeed an approximation to use the median height along the rim, and that it will have an impact on the absolute volumes. A more accurate method would be to reference surface (plane) based on the rim elevations.
We will investigate if this would change the results and revise the methods/manuscript accordingly.

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.
We will rewrite the paragraph "line 226" to:
"To estimate the error of the DEM volumes, we compute a new set of volumes at each grid point, with the uncertainty from the depth estimation for the used DEM added to the extracted surface elevations. We then subtract the previous set of volumes, and sum the discrepancies, to get the total volume error at each DEM."

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?
Yes, the depths here are the deepest points. We see that clarification is needed and we will revise this section.

Line 231 / equation 1.   How does the derivation of R and V take the error bars into account? More detail is needed.
We agree that a further explanation of how the uncertainty impacts the derivation of R and V is needed.
We have changed this paragraph:
"Since the collapse basin shape and form changes over time, the shape factor R changes over time, and we fit a smoothed function,˜R(t), through all available R(ti) values, taking their error bars into account."

To:

" Since the collapse basin shape and form changes over time, the shape factor R changes over time, and we fit a smoothed function,˜R(t), through all available R(ti) values, using a least squares method.

Because of the uncertainty of the depth and volume estimates, there is not one unique solution, making this an ill-posed problem. To account for the uncertainty, we introduce a regularizarion parameter "α" that penilazes the cost function in the least squares solution, to prevent overfitting to the ill-posed problem."

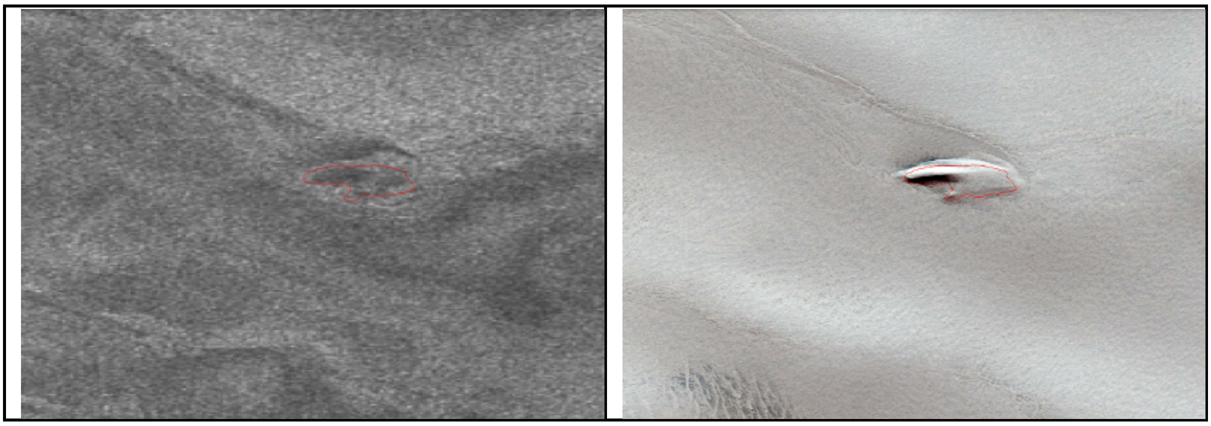
Line 236: It would be useful to demonstrate how R~(t) varies in time based on the available DEM data.
We do not see how such a figure would improve the manuscript, but we could provide such a figure in an Appendix.

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?
We agree that it does make sense to add a basic "roughness" uncertainty to the CS2 uncertainties. We will do so and update figures.

Line 246: add comma after "coverage"
We will revise accordingly

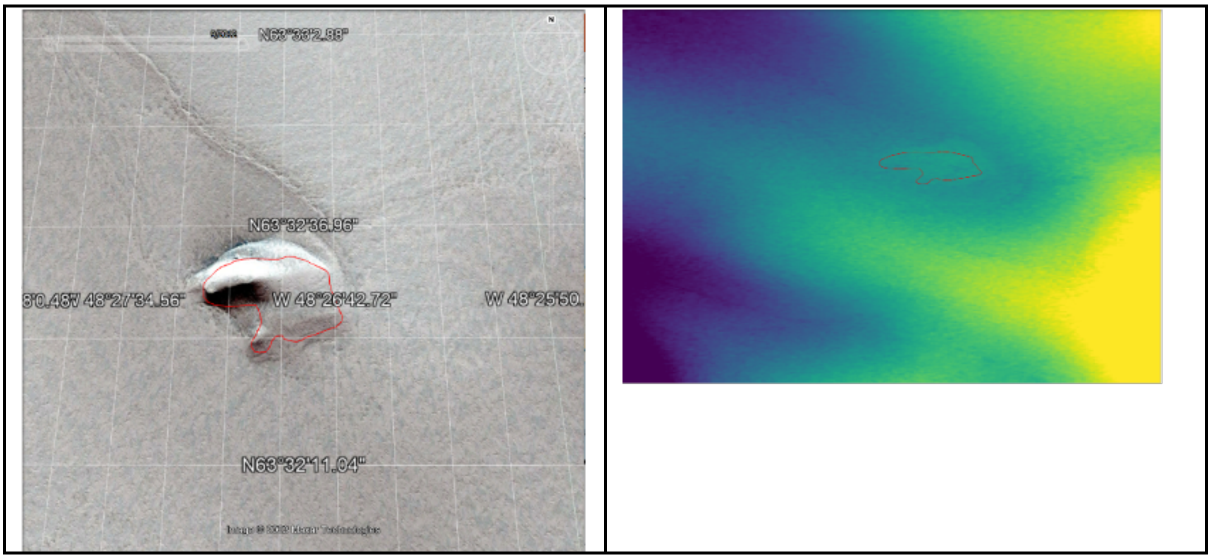
Line 264: It would be useful to explore why CS2 did not provide data over lakes 2 and 3. Were there no footprints that intersected the lake boundary?   Was the coherence too low?
We agree that it would be useful to further explain why that is the case.
The preliminary reason for the lack of data is that the surface depressions were too small, it was therefore 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 suggest to add figure(s) of some selected radar waveforms to clarify.

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.



TanDEM-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.



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 will revise accordingly

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).

358: "shortly" should be "briefly"
We will revise accordingly

373: “off-nadir” should be “off nadir”
We will revise accordingly

376-384: this repeats material found in the methods section.
We agree and suggest deleting the sentence from line 376-380, but keeping the last part (380-384) that emphasizes that we do not take the associated error into account.

378: delete “parameters”
See comment above

387: is “highly active” all that can be determined here? This doesn’t seem like a lot has been learned.
As the focus of a revised manuscript will be more on data and methods and less on the geophysical interpretations, we suggest to delete this sentence.

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 can be improved by expanding on the information and discussion. We will do so.

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 suggest removing the basal melt plot and associated discussion.

Appendix B:
Figure B1: Indicate the location of this lake relative to lake 2. 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 will revise Appendix B to clarify

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 and information needed in the revised manuscript.

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.