Response to Referee 1 on egusphere-2024-3251

First, we would like to thank the Referee for reviewing and commenting on the manuscript, which will improve the quality of the manuscript. Please find the item-by-item reply below, with the responses in blue. All the suggested changes will be implemented in the revised text that will be uploaded.

General Comments

This manuscript presents an interesting study into using the leading edge width (LeW) of CryoSat-2 measurements between 2011 and 2021 to investigate the long-term characteristics of Greenland firn conditions. The authors begin their manuscript with an introduction to the importance of understanding melt events, their effects across the Greenland Ice Sheet (GrIS) and how they intend to approach the problem using remote sensing data supported by in situ measurements and numerical climate model outputs. In Section 2, the authors present the various datasets considered in their study. Section 3 then outlines how the data are used, combined and the types of analyses the authors perform; the results of which are presented in Section 4. Finally, in Section 5, the authors reflect on the implications of the results and how future studies could expand on them, before the main conclusions are outlined in Section 6.

Overall, the authors have analyzed and presented a substantial amount of data. They take a very thorough approach to assessing the long-term spatial patterns in CryoSat-2 LeW by incorporating another satellite altimeter (i.e., ICESat-2), other derived satellite and airborne datasets (i.e., roughness, radar-laser offsets, topography), in-situ measurements (i.e., densities), and model results (i.e., densities, meltwater content, and firn air content). The scope of bringing all the data together is impressive and I think very exciting direction of work. Relatedly however, presenting so much data requires a very clear narrative and defined structure to help a general and non-specialized reader avoid being lost in all the details. My main comment on the manuscript is that I found this aspect of it to be underdeveloped, which could hamper the impact of the results.

While reading through study and going through all the results, I found it hard at times to get a sense of how all the different pieces fit together. I think the manuscript would benefit from a clearer statement of the central hypothesis and the physical reasoning behind it that would then frame the overall study. In the Introduction, the authors state the importance of melt and refreeze events and postulate that LeW could be used to study long-term patterns. What I think is missing though is how LeW is affected by melting/refreezing events. What is changing in the firn and how does that affect the radar signals? Concepts of surface and volume scattering as well as refrozen layers appear repeatedly later on in the manuscript, but I think explicit descriptions of what the authors mean by these, their linkage to the physical state of the firn and what that means for the CryoSat- 2 would substantially help fortify the overall narrative. This bridging between radar theory and more classical glaciological concepts will also strengthen the impact the manuscript will have by really outlining how all these pieces fit together and what the results mean. Some of the specific comments below will also be in this direction.

We appreciate the general comments. According to the Greenland snow and firn facies defined in Benson (1960), the dry-snow zone is characterised by snow and firn with low density, small grain size and uniform crystals. In lower altitudes, the percolation zone is characterised by ice pipes or ice lenses with large grain size, due to the vertical or horizontal percolation of meltwater and its subsequent refreezing. In the lowest altitudes, the wet-snow zone also has ice pipes or ice lenses, but the densities are higher than those in the percolation zone due to the compaction from a higher temperature. Therefore, without melting, the microwave scattering within Greenland dry-snow zone should be stable (Tran et al., 2008). However, the strong melt events, e.g. the 2012 melt, can cause the formation of ice lenses within the dry-snow zone (Nilsson et al., 2015). These high-density ice lenses reduce the Ku-band radar penetration by approximately 50% (Otosaka et al., 2020). Moreover, the radar altimetry leading edge width (LeW) can be influenced by surface roughness, topography and surface penetration effects of the radar signal. Since the topography of the dry-snow zone is typically flat, we can observe the reduced radar penetration within dry-snow zones by observing the drops in LeW time series. As new snow deposits, the radar penetration hence LeW is expected to recover (rise) again (Nilsson et al., 2015).

However, within the CryoSat-2 LRM coverage, the percolation zone is also present, where the high-density firn may limit radar penetration, but the large firn grain size can increase the roughness, hence can increase LeW (Nilsson et al., 2015). This phenomenon causes the limitation of detecting refrozen layers within Greenland firn using LeW time series.

We will better present this concept in the revised manuscript.

I hope the authors find the following comments constructive as they work towards revising their manuscript.

Specific Comments

1) As the manuscript primarily centers on CryoSat-2 LeW results, I would suggest the authors consider revising the title to “Assessing spatio-temporal variability of firn scattering over Greenland with CryoSat-2”. I understand that the inclusion of ICESat-2 data makes a case for multiple altimeters, but my impression is that the ICESat-2 data are more complementary to the main CryoSat-2 dataset. In a similar way to the MAR and IMAU climate model data, ICESat-2 data appear to be used more to help explain trends in the CryoSAt-2 data, not necessarily as the primary data source themselves.
This will be implemented in the revised manuscript.

2) Line 57 “The LeW is adopted as it is sensitive to volume scattering ...” Line 67 “... we have to understand both volume scattering and surface scattering ...” These are two instances where a more explicit statement of what the authors mean by volume and surface scattering could help improve the overall framing of the study. How/why LeW is sensitive to these two concepts and what on the surface and in the firn contributes to them? I think it would broaden the reach of the manuscript by removing the hurdle of needing to be familiar with nuanced radar theory concepts and motivate exactly why the specific model outputs are chosen for comparison with the LeW results in the latter stages of the manuscript.
We agree and, in the revised manuscript, we will clarify this by referring to the modelling results from Lacroix et al. (2008).

3) The authors dedicate Lines 35-51 motivating CryoSat-2 and LeW as a metric for studying firn. I recommend the authors consider expanding more clearly on the motivations for using the other datasets (e.g., ICESat-2, in-situ densities, dz, roughness, topography, and model results) to help explain the LeW results. What aspect of the LeW signal are these datasets being used to interpret? I found Lines 52-69 to be confusing as it was not always clear how these different datasets all supported the LeW analysis.

The use of in situ densities and modelled densities are to demonstrate the formation of high-density firn following melt-refreezing events. Specifically, the in situ data are used to show that these high-density layers are indeed gradually buried due to the new-snow deposition, hence LeW rises. Roughness and topography data are used to delineate the regions where LeW variations may be dominated by surface scattering rather than volume scattering, therefore we cannot effectively interpret the LeW time series in certain areas. The dz dataset was a comparison to show the strengths of our approach: compared to the OIB measurements, the CryoSat-2 LeW not only shows the melt event, but also the firn recovery. It also has much better spatial and temporal continuity. Finally, the use of modelled FAC provides insights into the overall condition of firn, as we may wonder whether the recurrent melt-refreezing patterns are causing a permanent change in firn's health.

This will be explained in better detail in the revised manuscript.

4) I recommend the authors consider reducing the number of adverbs (e.g., furthermore, finally, in addition, therefore, additionally, etc.) used to start sentences to make them more direct and impactful.

This will be improved in the revised manuscript.

5) To be more specific on the types of GrIS changes of interest in this study, I recommend the authors re-phrase Line 70 from "... assess long-term changes over the ..." to "... assess long-term surface changes over the ..."

We appreciate the suggestion of the referee, however surface changes may still be a bit shallow compared to the 1.5m snowpack in our study. Therefore, to make it more specific, we will specify that we are assessing sub-surface changes.

6) In Section 2.1, I recommend the authors include more detail on the nature of the CryoSat-2 LRM data and what differentiate them from other CryoSat-2 data products (e.g., what is unique/different in their acquisition/data processing?).

The CryoSat-2 LRM data are available within the interior of Greenland, SARIn mode is available over the coastal areas, and SAR mode operates over the sea. This will be elaborated in the revised manuscript. However, for the data processing, since SAR and SARIn modes are not available in our region of interest (Greenland interior), it may not be relevant to add everything in detail.

7) Line 89, please include the range resolution of CryoSat-2.

It should be made more explicit that S_r should be $\frac{1}{2}c \cdot dt$, where c is the speed of light and dt is the waveform sampling interval (3.125ns) This will be improved in the revised manuscript.

8) In Figure 1, I'd ask the authors to consider including the b0.99 and b0.01 values for each waveform as well as map (perhaps as an insert) of where these two locations are in Greenland are.
The indications will be added in the revised manuscript.

9) Line 94, what high-resolution DEM model is used?
It should be the 100m resolution ArcticDEM mentioned in Section 2.3. Following the recommendation, we will mention the ArcticDEM at the beginning of the Data section.

10) In Line 97, the authors state that they used July measurements as indicative of post-melt conditions but there is no way for the reader to assess if melting has ceased at these locations by the time the data were acquired; especially knowing how extreme the melt extents observed in the summer of 2012 were. I would recommend the authors provide further support for this statement or consider using data from later in the year.
This will be changed into the September data in the revised manuscript.

11) I am not sure I fully understand the context for why two different grid resolutions (50x50 and 25x25) are used. I suggest the authors clarify this point.
Originally, we aimed to use the finer resolution (25km x 25km) to calculate long-term (decadal) statistics and the lower resolution (50km x 50km) 50 calculate monthly statistics. However, following the recommendation of Referee 2, we noticed that using 10km x 10km resolution can still ensure sufficient (more than 10) data points per pixel per month, while being consistent with the spatial resolution of the firn models. Therefore, we adopt the recommendations of both referees and will adopt the 10km x 10km resolution throughout the revised manuscript.

12) Line 115. Do all CryoSat-2 measurements in a given month have a corresponding ICESat-2 measurement within 50 m or are their spatial gaps? I'd also recommend the authors provide their reasoning for choosing 50m when the footprint of CryoSat-2 LRM data is much larger.
It is true that not all CryoSat-2 measurements have a corresponding ICESat-2 measurement within the 50 m radius. As shown in Table 1 of Li et al. (2022), such a criterion results in approximately 30 times fewer measurements in year 2019.

We agree that the footprint of CryoSat-2 LRM mode is much larger. The motivation of using a smaller search criterion was that over the undulating terrain, the true footprint of CryoSat-2 LRM should be smaller than the theoretical one, therefore we would choose a corresponding ICESat-2 point as close as possible, yet not largely reducing the number of valid points. However, the selection of 50 m is rather arbitrary, therefore we have conducted a sensitivity analysis, shown in Fig. R1. It can be observed that as the search radius increases, the number of valid dh increases, while the correlation between dh and LeW decreases. Especially, when using 800 m as the search range, which is comparable to the theoretical pulse-limited footprint of CryoSat-2 LRM, the correlation coefficients are overall below 0.5. Using 100 m and using 50 m do not demonstrate distinct differences. Therefore, we prefer to choose a search range as small as possible, which is also similar to the crossover principle proposed by Michel et al. (2014).

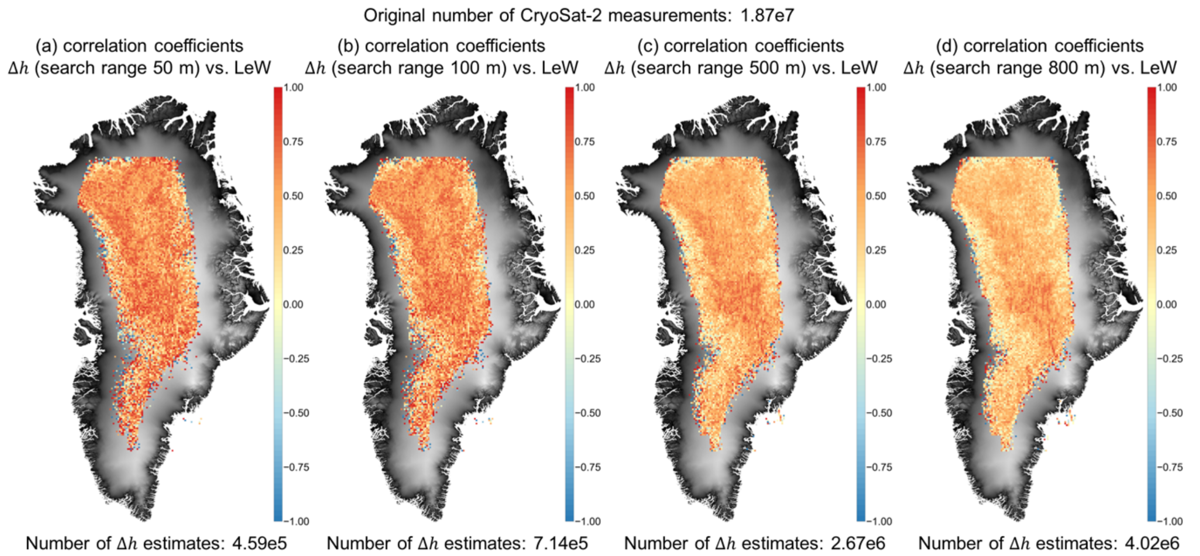

Figure R1. Comparison of correlation coefficients between dh and LeW when using different search range for the corresponding ICESat-2 point for each CryoSat-2 point.

13) With how Sections 2, 3 and 4 are structured, the CryoSat-2/ICESat-2 results from Figure 2 and Lines 122-128 seem to be more suited to Section 4 than Section 2. I understand they are used again in Section 2.4, but could Section 2.4 be treated more abstractly by referring to a subsurface depth extent to be determined later? The current placement seems to interrupt the flow of describing all the individual datasets considered.

We appreciate the suggestion of the referee and this will be re-structured in the revised manuscript.

14) Lines 134-136. I recommend the authors elaborate a bit more on how “computational efficiency” necessitates using both the 100m and 1km ArcticDEMs in these two instances. What about these specific applications makes the use of two different DEMs more efficient?

The use of the 100m ArcticDEM is inherited from Li et al. (2022): in principle, ArcticDEM can be available with the resolution of 2m (Porter et al., 2018). However, since our computation is performed in MATLAB, it is difficult to load the large ArcticDEM Geotiff files. Therefore, the finest resolution considered in our studies is 100m.

We admit that the “computationtal efficiency” that comes with the 1km ArcticDEM can be confusing: originally, we divide the interior of Greenland into a 50km-by-50km grid, so that the topography for each pixel is represented by the standard deviation of the 50-by-50 ArcticDEM height values within the pixel. However, following both referees’ comments, we will reconsider the resolution of the grid over the interior Greenland, and use the 100m ArcticDEM to compute the topography in the revised manuscript.

15) Figures 2, 3, 4, 6, 7, and 9. I recommend the authors elaborate why they used a DEM from Helm et al. (2014a, b) as their basemap instead of one of the ArcticDEMs they use in their analysis. Also, I’d recommend including a colorbar for the elevations the first time it is used.

The only reason is that the Helm et al. (2014) DEM focuses on Greenland, while ArcticDEM covers the entire Arctic including the ocean surrounding Greenland and is difficult to crop. We will edit the ArcticDEM in the revised manuscript for better consistency. The colorbar will also be added.

16) Line 146. I recommend the authors clarify how the weights are determined in their weighted average densities.
The weights are defined as the thickness of each layer. This will be elaborated in the revised manuscript.

17) I recommend the authors consider better motivating the inclusion of the IMAU FAC. FAC is a column-integrated measurement (Line 168) whereas LeW derived from CryoSat-2 is seemingly only sensitive to the upper few meters (Figure 2). Why would these two datasets derived over different depth ranges be considered comparable?
We agree with the concern of the referee. In general, we aim to use the density dataset at upper 1.5m to prove that CryoSat-2 is indeed sensitive to the changes that happen within the 1.5m firn layer. On the other hand, as we also try to learn about the overall condition of the firn (beyond this 1.5m threshold), the FAC over the entire snowpack is used as additional information to indicate whether Greenland firn experiences a continuous decrease in pore spaces. We will better motivate it in the revised manuscript.

18) Line 168. The "but" in "... 1.5 m but the FAC ..." can be removed.
This will be changed from
"While the density is calculated over the first 1.5 m but the FAC is calculated for the entire firn column."
to
"While the density is calculated over the first 1.5 m, the FAC is calculated for the entire firn column."
in the revised manuscript.

19) I recommend the authors expand on why these particular in-situ firn density measurements are used instead of the more comprehensive SUMup dataset (i.e., Vandecrux et al. 2023)? Furthermore, why is it necessary for firn density profiles to contain the 2012 melt year (Line 185)?
Vandecrux, B. et al. The SUMup collaborative database: Surface mass balance, subsurface temperature and density measurements from the Greenland and Antarctic ice sheets (1912-2023). Arctic Data Center https://doi.org/10.18739/A2M61BR5M (2023).
We appreciate the referee for the suggestion and will include this dataset in the revised manuscript.

The main reason to contain the 2012 melt year is to provide more sound evidence that the 2012 melt results in a visible density increase, which can also be observed in the modelled firn densities. This high-density layer is buried in the subsequent years, therefore the recovery in LeW can be eventually observed.

20) Line 212. These 10 DEM elevation groups have not been mentioned yet, so I do not follow how they can be "aforementioned". I recommend the authors clarify this statement.
It should be 8 groups equally divided between 1500m and 3000m, 1 group below 1500m and 1 group above 3000m. This will be improved in the revised manuscript.

21) Line 230-231. These seem to be the elevation bands mentioned in Line 212. I recommend the authors clarify why they include elevation bins down to 100 m elevation. It is my understanding that the study only considers CryoSat-2 LRM data which cover the high-elevation interior portion of the GrIS.
We made a wrong estimation of the lowest elevation within the CryoSat-2 LRM data coverage. This will be improved (as mentioned above) in the revised manuscript.

22) Line 243. I recommend the authors clarify the "Following ..." used to start this section. The previous two analyses described in Section 3.1 and 3.2 use to 25x25 km grid. The adoption of the 50x50 km grid here seems to be a marked departure from what has occurred previously as opposed to following/continuing.
The analyses before were performed to understand which regions are dominated by surface scattering and which regions by volume scattering, so that the following time series can be better interpreted. However, we agree that the logic of this sentence is weak. This will be removed in the revised manuscript.

23) Section 3.3. I recommend the authors clarify which months are included in their analysis of long-term variations. As it reads, it seems as though June-December LeW data are not represented (average is derived between January and May, Lines 243-244). What motivates this choice and why are Fall/early winter data not considered? If the goal is to avoid melt being present in the snow, would focusing on the full non-melt season (e.g., Oct.-Apr.) be more appropriate as opposed to following calendar years?
The goal is indeed to avoid melt being present in the snow, therefore the analysis will be changed to the full non-melt season in the revised manuscript.

24) Lines 261-263. I recommend the authors be more specific on where on the GrIS they are referring to. Are the number they state representative of the ice sheet as a whole or only a portion of it?
It is true that the observation represents only a portion of the GrIS. This will be better clarified in the revised manuscript.

25) Figure 4. I suggest the authors be specific with the LeW time periods behind the data presented here. Do they match the time periods shown at the top of the plot or are they those outlined in Section 3.3?
They match the time periods at the top of each column. This will be clarified in the revised manuscript.

26) Line 266. I have a hard time following the logic behind this statement because there isn't a really clear statement of how/why LeW is sensitive to volume scattering. Is increased volume scattering expected to increase or decrease LeW? Figure 1 would imply a positive correlation but, to me, here it seems to imply the opposite (reduced scattering (implying reduced LeW) due to subsurface high-density layers).
Following Nilsson et al. (2016) and Fig. 1, the melt events result in the formation of subsurface ice lenses, which reduces the radar penetration hence volume scattering; the LeW in turn reduces (e.g. the LeW at Pixel A reduces from 6.05 m to 3.21 m, according to Fig. 1). Therefore, it is correct that the reduced volume scattering implies a reduced LeW. This will be better clarified in the revised manuscript.

27) Figure 6. I recommend the authors consider including select representative 2D histograms directly comparing dh and LeW in addition to the correlation coefficient maps. I think this would give a sense on if the data are clustered or the range over which they co-vary against one another.

The following figure will be added in the revised manuscript, where the point density distribution is estimated using Gaussian kernel estimation (Węglarczyk, 2018).

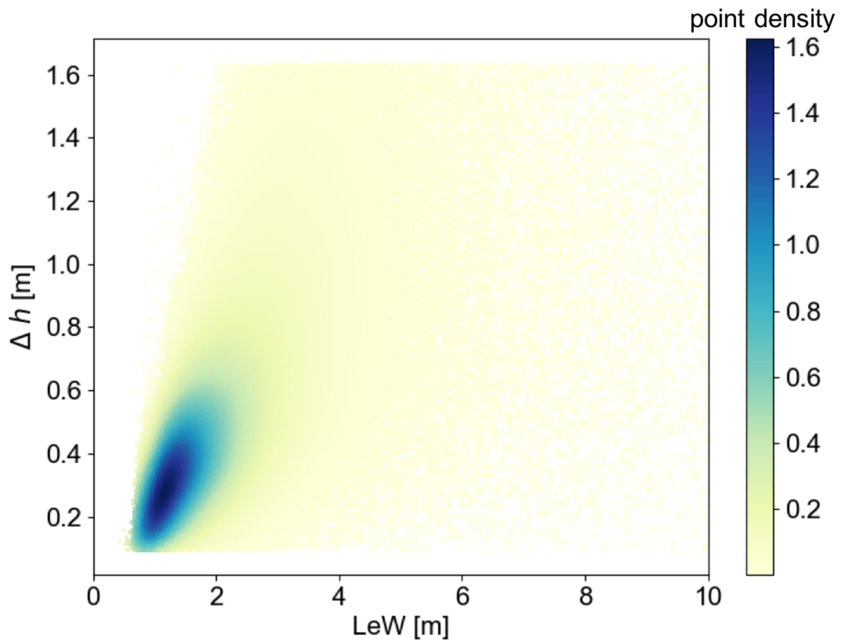

Figure R2. Scatterplot between LeW and Δh. The point density distribution is shown in colours.

28) Line 291. Could the authors expand on this point and elaborate on how surface scattering effects the LeW/dh correlation? Is it because the OCOG retracker becomes less sensitive in rough areas?

As Referee 2 also pointed out, our original method to compute LeW was not sufficiently robust, as it directly searches for the peak of the normalised waveforms. We have improved the method to use the OCOG amplitude as the maximum amplitude, and define the bins between 0.05 and 0.95 thresholds as the LeW. By improving the method, the overall correlation increased from on average 0.3 to on average 0.7.

Regarding the specific regions where the correlation coefficients are generally lower than 0.5, they are typically characterised by more undulating topography close to the coastal line of Greenland or the southern regions with more recurrent melting. We present an example of the time series of LeW versus dh in Fig. R2. Two pixels in the 10km x 10km resolution are chosen for the visualisation. The pixel in the north shows a matching trend between LeW and dh, while the pixel in the south only shows a match partly, with a large standard deviation of both LeW and dh values.

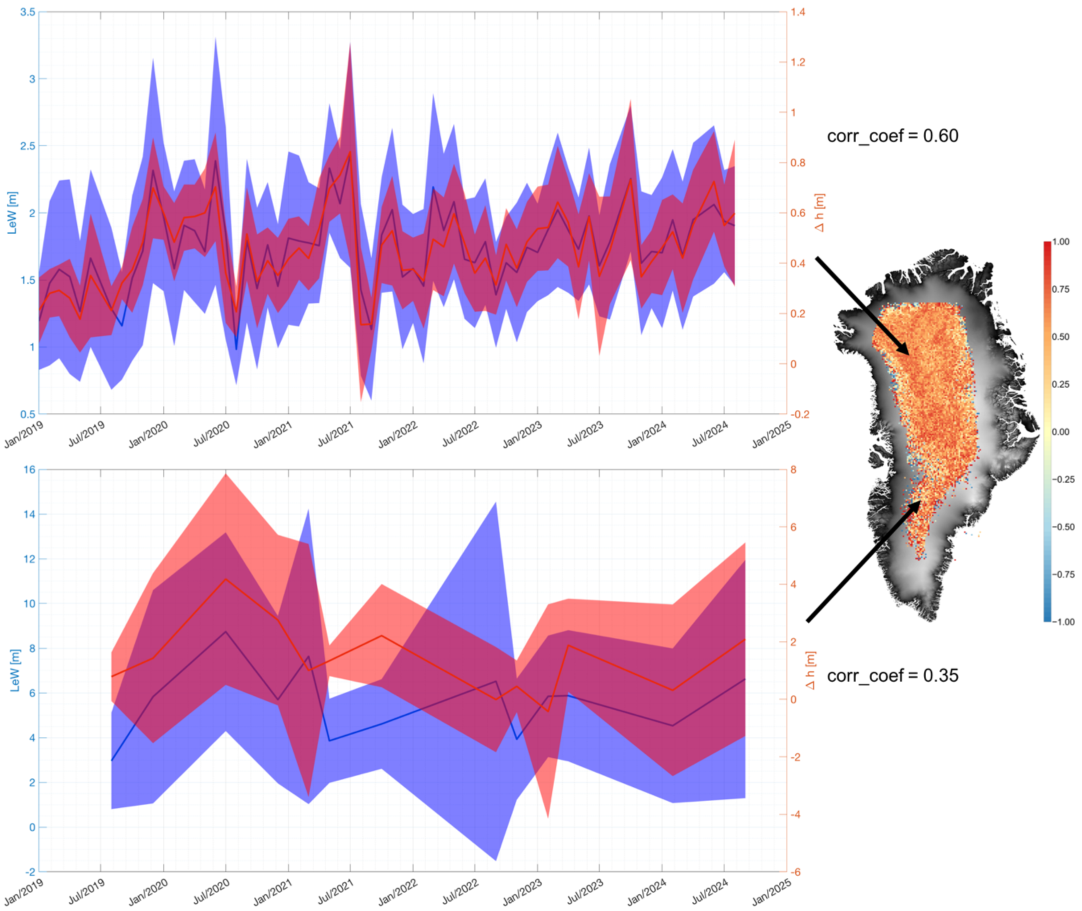

Figure R3. Example time series of LeW (blue) and dh (red) for two locations. Shaded areas show the standard deviation of the inspected parameters within the pixel.

29) Line 293. I'd recommend the authors be very careful with the statement that penetration depth increases because LeW increases. LeW is an interpretation of an observed signal. If there was no volume scattering in the subsurface, the signal would penetrate as just deeply but no reflected power would exist at that point in the waveform, so LeW would only be a function of surface roughness. The depth to which it is possible for a radar to say something about the subsurface is a function of both how radar is designed and operated (e.g., transmit power, noise levels, data processing) as well as the structure and makeup of the surface and subsurface. All of these would affect at what point the SNR of a reflection from the subsurface would reach 0 dB. In light of this, would it be a more appropriate/accurate option to use "radar-laser height offsets" as opposed to "penetration depths"?

We agree. The concept of "penetration depths" will be replaced with "radar-laser height offsets" in the revised manuscript.

30) Line 314. I am confused by the statement here of a notable recovery in firn conditions and what is on Line 266 where the authors state firn recovery is not reflected. I recommend the authors clarify the distinction/difference between these two seemingly conflicting results.

Line 266 was an imprecise phrasing. We meant to say that dz was showing an abrupt recovery between 2013 and 2014, while the LeW recovery was more gradual. This will be improved in the revised manuscript.

31) A general comment on the Figures, but I'd ask the authors to consider using different colormaps for different variables. The same red-to-blue colormap is used in Figures 4, 6, 7, 8, 9, and 10 even though the variable being plotted changes; sometimes an absolute value is shown and sometimes a difference. I would also recommend that when presenting data on a map, the authors label their colorbars to make it explicit what variable is being shown.
The colormaps will be differed and the colorbars will be labelled in the revised manuscript.

32) Lines 338-339. I recommend the authors provide more explanation regarding why regular, annual melt-refreeze cycles are less impactful on volume scattering compared to more intermittent events.
Lines 338-339 particularly discusses the LeW variations in the southern part of Greenland. Here, the snow deposition rate is higher than the other regions of Greenland, as shown in Fig. A1 of the original manuscript. We will better explain this in the revised manuscript.

33) In the Discussion section, the authors devote the first paragraph to contrasting their results against those of Rutishauser et al. (2024). The authors compare the results in terms of their spatial patterns, but I would also suggest the authors consider the nature of the underlying radar measurements as well. The OIB MCoRDS radar operates in a much different frequency range compared to the CryoSat-2 SIRAL altimeter. What affect will that have on the resulting data and interpretations that could be assumed to be responding to more or less the same near-surface stratigraphy?
The CryoSat-2 SIRAL altimeter operates in a different manner from the OIB MCoRDS radar. While we used dh and LeW to indicate the terrain and part of the firn layer that have an impact on radar altimeter's waveforms, the Rutishauser et al. (2024) study tracks the peak of the the reflected radar signal. Therefore, in the Rutishauser et al. (2024) study, a perfectly dry-snow condition results in dz=0, indicating the radar reflection from the air-firn interface, while in our study, dry snow results in dh>0, indicating the height offset between laser and radar due to radar penetration. With the formation of an ice lens, dz from the Rutishauser et al. (2024) study increases, as another strong sub-surface reflector is detected, while in our study, dh and LeW immediately drop due to the reduction of Ku-band penetration ability. We will elaborate it better in the revised manuscript.

34) Also in the Discussion section, I would also suggest the authors be more specific with what they expect can be gained from integrating radar measurements at other frequencies (Lines 413- 415)? MCoRDS data are substantially different from CryoSat-2 but, as outlined in the previous comment, frequency-dependent impacts are not discussed. How can improved results in complex surface and volume scattering areas be improved by adopting more frequencies? At the same time, I'd ask the authors to consider what this means for future dual-frequency radar altimeters such as ESA CRISTAL which will operate Ku- and Ka-band altimeters simultaneously.
We appreciate the detailed recommendations of the referee, and will elaborate on this point in the revised manuscript.

35) Figure 10. I recommend the authors elaborate more on the specific elevation intervals presented in 10b, 10d, and 10e. Why are these specific intervals chosen and what additional information do they add? Thes subplots and the specific elevation intervals, not only deviate

from every plot that has been shown so far but they are not even mentioned in the main text. What overall purpose do they serve?
They were originally randomly sampled among the 10 elevation groups with the aim to show separate time series in terms of curves with corresponding standard deviations. However, we understand that this can cause confusion, and will improve the illustration in the revised manuscript.

36) Line 433. Here “sub-surface” appears with a hyphen, while through the rest of the manuscript it is written as “subsurface”.
This will be corrected in the revised manuscript as “subsurface”.

36) Line 441. “stratigraphy” is misspelt in the Code and data availability section.
This will be corrected in the revised manuscript.

Reference

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