Response to Referees on egusphere-2024-3251

We appreciate both the referees and the editor for the support for improving out manuscript. Please find the response to Referee 1 from page 1 to page 16, and the response to Referee 2 from page 17 to page 25.

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. We have added the following theoretical descriptions from Line 31 onwards in the revised manuscript:

"The underlying principle is based on the fact that over firn-covered regions of the ice sheet—primarily at higher elevations—radar pulses at frequencies commonly used in satellite altimetry penetrate into the firn (e.g., Ridley and Partington, 1988). According to Ridley and Partington (1988) and Davis and Zwally (1993), the penetration depth may range from a few centimetres to several metres. Consequently, recorded waveforms contain signals from both surface scattering and volume (or sub-surface) scattering caused by inhomogeneities within the underlying firn layers. Surface scattering dominates the start of the waveform, while volume scattering becomes predominant beyond the inflection point, where the illuminated surface area becomes constant. The rise of the backscattered power from volume scattering depends on firn parameters, including firn density, firn air content (FAC), and grain size (Ridley and Partington, 1988; Vandecrux et al., 2019; Brils et al., 2022). For example, larger grain radii and higher firn densities lead to a faster increase in backscattered power (i.e., a steeper leading edge) (Ridley and Partington, 1988, Figs. 9, 10). Melt and refreezing events alter firn parameters and form refrozen layers, modifying scattering behaviour and waveform shape. Depending on thickness and density, refrozen layers can substantially reduce radar penetration, diminishing volume scattering. While these changes in waveform shape are observable, attributing them to variations in volume scattering—and thus to changes in firn properties—requires distinguishing them from variations in surface scattering, particularly those driven by surface roughness. Indeed, a decrease in surface roughness also results in steeper leading edges (Ridley and Partington, 1988, Fig. 8)."

We also noticed that, directly aiming at "volume scattering" in the title is confusing, as we have not been able to quantitatively distinguish which part of the waveform signal is actually affected by volume scattering and which part by surface scattering. Therefore, we have also changed the title into "Assessing spatio-temporal variability of melt–refreeze patterns in firn over Greenland with CryoSat-2", hoping to be more specific.

To improve readability of the manuscript, we also shifted the Methods and Results sections, to focus on:

- The yearly spatio-temporal variations in LeW, visualised in maps;
- The monthly spatio-temporal variations in LeW, interpreted with the surface roughness datasets;
- The correlation between LeW and laser-radar elevation biases, aiming to link temporal variations in LeW with temporal variations in volume scattering;
- The monthly temporal variations in LeW, interpreted with the help of modelled densities and firn air content (FAC).

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 has been 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 have re-organised the paragraph (and the previous paragraphs) as follows (now it becomes Line 47 onwards):

"Several studies have employed waveform shape parameters to gain insight into the impact of melt and refreezing on Greenland's firn layer, as well as to estimate the bias in radar altimeter-derived elevations caused by radar penetration into the firn. Nilsson et al. (2015), for example, used Ku-band (13.575 GHz) altimeter data acquired by CryoSat-2 to track the formation of ice lenses following melt events. Simonsen and Sørensen (2017) explored the same data to investigate the impact of volume scattering properties on elevation estimates. Both Nilsson et al. (2015) and Simonsen and Sørensen (2017) showed that a large leading edge width (LeW) is indicative of volume scattering of the signal in the upper parts of the firn, while Nilsson et al. (2015) in particular observed the impact of the 2012 Greenland melt event and its subsequent refreezing on waveform-derived parameters, including LeW, trailing edge slope (TeS) and peakiness, backscatter intensity, and height. The Simonsen and Sørensen (2017) study indicated that within the region of the Greenland Ice Sheet covered by Low Resolution Mode (LRM) data (i.e., the LRM zone), LeW could be effectively used to correct for elevation biases caused by volume scattering. In addition to waveform shape parameters, other radar altimeter-derived variables have also been utilised to infer firn properties. For instance, Scanlan et al. (2023) leveraged surface echo powers in Ku-band CryoSat-2 and Ka-band SARAL radar waveforms to derive monthly maps of Greenland Ice Sheet's wavelength-scale surface roughness and density between January 2013 and December 2018. Furthermore, several studies have estimated radar penetration depths by combining radar and laser altimetry data. Michel et al. (2014), for example,

analysed the differences between radar (ENVISAT) and laser (ICESat) altimeter heights over Antarctica to derive Ku-band radar penetration biases into firn and compared these height differences with LeWs. The study provides insights into opportunities for similar approaches to study Greenland's firn.

Despite these advances in using radar altimetry to monitor Greenland's firn properties—particularly in assessing the effects of melt and refreezing—existing studies have largely been confined to a period without extensive melt (e.g., January 2013 to December 2018; Scanlan et al., 2023), or to the short timeframe immediately following the 2012 melt event (e.g., up to 2014; Nilsson et al., 2015). The impact on the long term, especially following the 2019 melt (Tedesco and Fettweis, 2020), remain insufficiently monitored. The availability of more than a decade of CryoSat-2 data presents a valuable opportunity to address this gap."

With this revision, we also hope to show that LeW is the main focus of this study, and the ICESat-2 data are more complementary, following the suggestion of the Referee's comment 1.

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.
We understand and appreciate the recommendation. Following the revision of the previous comment, which already indicated the use of roughness dataset, topography and ICESat-2 data, we re-wrote the paragraph 52—69 (now it becomes Line 70 onwards) as:

"The main objective of this paper is to assess the impact of melt and refreezing events on the properties of Greenland's upper firn layer, using LeWs derived from CryoSat-2 radar waveforms acquired between 2010 and 2024. To support the interpretation of the results, we complement the assessment by comparing the LeWs with: (i) the surface roughness dataset derived by Scanlan et al. (2023) to analyse under which circumstances the LeW variation is dominated by surface scattering; (ii) the ArcticDEM standard deviation to assess the impact of macro-scale roughness due to topographic variation on LeW; (iii) penetration depths obtained by differencing ICESat-2 laser altimeter elevations and CryoSat-2 radar altimeter elevations to gain further insights into volume scattering; and (iv) firn densities and FAC from the Modèle Atmosphérique Régionale (MAR) (Fettweis et al., 2011, 2017; Lambin et al., 2022) and the Firn Densification Model from the Institute for Marine and Atmospheric research Utrecht (IMAU-FDM v1.2G; Brils et al., 2022) to analyse how spatial and temporal variations in firn properties affect the LeW and to improve the interpretation of radar altimeter scattering properties for future research."

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.
We appreciate the recommendation. Many of the adverbs have been removed 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. As mentioned in previous discussions, to also make it more specific, we have revised it into *"The main objective of this paper is to assess the impact of melt and refreezing events on the properties of Greenland's upper firn layer..."*.

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. We have briefly added from Line 99 onwards:

"CryoSat-2's primary payload, the SAR Interferometric Radar Altimeter (SIRAL), operates in three measurement modes (Wingham et al., 2006):
1. Low Resolution Mode (LRM): analogous to pulse-limited radar altimetry, this mode is used over relatively flat ice sheet regions and the open ocean.
2. Synthetic Aperture Radar (SAR) Mode: utilising coherent echo processing, this mode provides higher-resolution along-track data from sea ice and sea-ice-contaminated ocean surfaces.
3. SAR Interferometric (SARIn) Mode: combining coherent echo processing with interferometry, SARIn delivers high-resolution along-track data along with across-track echo directions, primarily for ice sheet margins."

7) Line 89, please include the range resolution of CryoSat-2.
(Now Line 114) It should have been made more explicit that the "range resolution", S_r, should be $\frac{1}{2}c \bullet dt$, where c is the speed of light and dt is the waveform sampling interval (3.125ns) This has been improved in the revised manuscript (Eq. 2).

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.
We have realised that the original Fig. 1 has little added value to the overall readability, hence have removed it from the revised manuscript.

9) Line 94, what high-resolution DEM model is used?

(Now Line 85) It should be the 100m resolution ArcticDEM mentioned in Section 2.3. Following the recommendation also from Referee 2, we have introduced the ArcticDEM at the beginning of the Data section (now Section 2.1).

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.
Following comment 8, the original Fig. 1 and the related description have been removed from 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) to 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 have used 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.
(Now Line 140) 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).

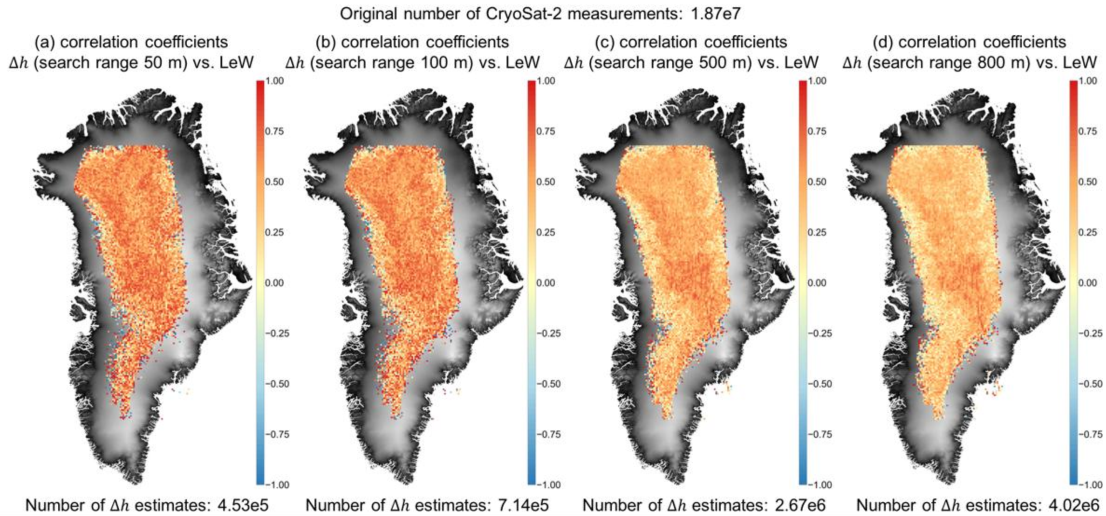

Figure R1. Comparison of correlation coefficients between dh and LeW when using different search range for the corresponding ICESat-2 point for each CryoSat-2 point.

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 has been re-structured in the revised manuscript (Section 3.3).

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. Now we have clarified this in Line 91:

"The model is available at various resolutions, ranging from 2 m to 1 km (Porter et al., 2023). Consistent with Li et al. (2022), we employ the 100 m resolution ArcticDEM for slope-induced error correction in CryoSat-2 elevation estimates, balancing accuracy and computational efficiency compared to the higher-resolution 2 m dataset."

We admit that the "computationtal efficiency" that comes with the 1km ArcticDEM can be confusing. Following both referees' comments, we have used a 10km-by-10km grid over the interior Greenland in the revised manuscript. When we used the 100m-resolution ArcticDEM to compute the standard deviation of each grid cell, it again resulted in an overload of computational power, therefore we had to use the 1km-resolution ArcticDEM.

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 have edited the ArcticDEM in the revised manuscript for better consistency. The colorbar is also shown in Figs. 1 and 2.

16) Line 146. I recommend the authors clarify how the weights are determined in their weighted average densities.
(Now Line 56) The weights are defined as the thickness of each layer. This has been elaborated in the revised manuscript as:

"We use the time series of modelled firn density profiles to compute the weighted average density of the upper firn column, from the surface to the max Ku-band radar penetration depth. Thickness is used as a weighting factor to account for the model's uneven vertical resolution."

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 have added in Line 181:

"The FAC represents the vertically integrated porosity of the firn (Kuipers Munneke et al., 2015), expressed in metres. It is computed over the entire firn column and serves as a measure of total firn porosity, indicating the firn's capacity to retain meltwater (Vandecrux et al., 2019). Although CryoSat-2 signals primarily penetrate the upper firn layers, we leverage the modelled FAC time series to assess whether the observed melt–refreeze patterns notably influence broader firn conditions."

However, from our experience, variability in FAC is mainly a result of changes in the upper meters of firn.

18) Line 168. The "but" in "... 1.5 m but the FAC ..." can be removed.
(Now Line 181) This has been rewritten as above 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 have included this dataset in the revised manuscript (the Schaller et al., 2016 and Otosaka et al., 2020 datasets are already included in the SUMup database).

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. This motivation has been added to Line 196 of the revised manuscript.

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 has been moved to Line 87, Section 2.1 (together with the introduction to ArcticDEM) of 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.

(Now Line 227) We made a wrong estimation of the lowest elevation within the CryoSat-2 LRM data coverage. This have been 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 sentence has been removed in the revised manuscript (and now we use the 10x10 km grid everywhere).

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 has been changed to the full non-melt season (Oct.-Apr.) 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. We have added *"Within the coverage of CryoSat-2 LRM data"* in contents such as Line 251 of 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?
Figure 4 has been removed from 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 Ronan et al. (2024), the melt events result in the formation of subsurface ice lenses, which reduces the radar penetration hence volume scattering; the LeW in turn reduces. Therefore, it is correct that the reduced volume scattering implies a reduced LeW. We agree that the comparison against the Rutishauser et al. (2024) study causes confusion and have removed it from 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 has been added in the revised manuscript (Fig. 5c), where the point density distribution is estimated using Gaussian kernel estimation (Węglarczyk, 2018).

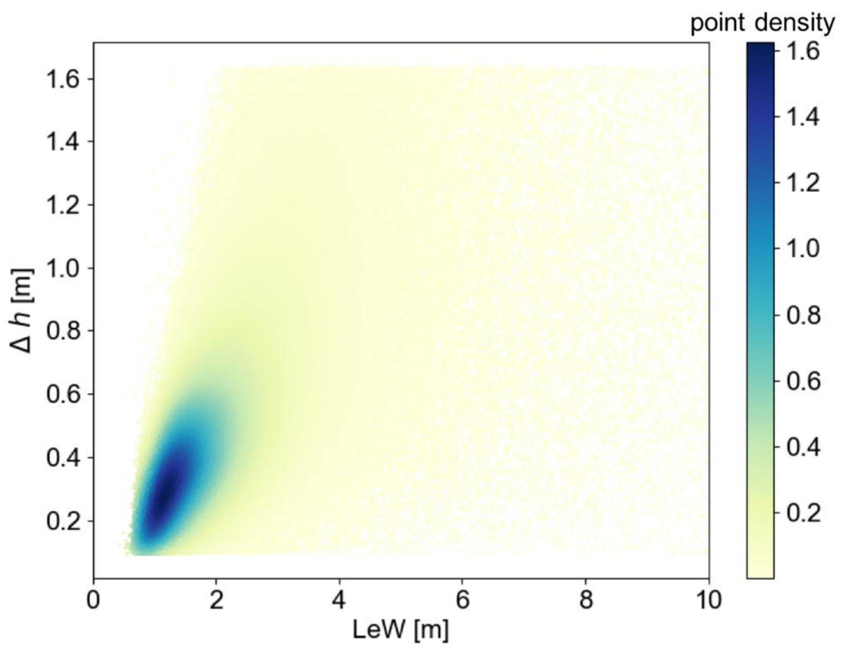

Figure R2. Scatterplot between LeW and Δh. The point density distribution is shown in colours.

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 0.6.

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.

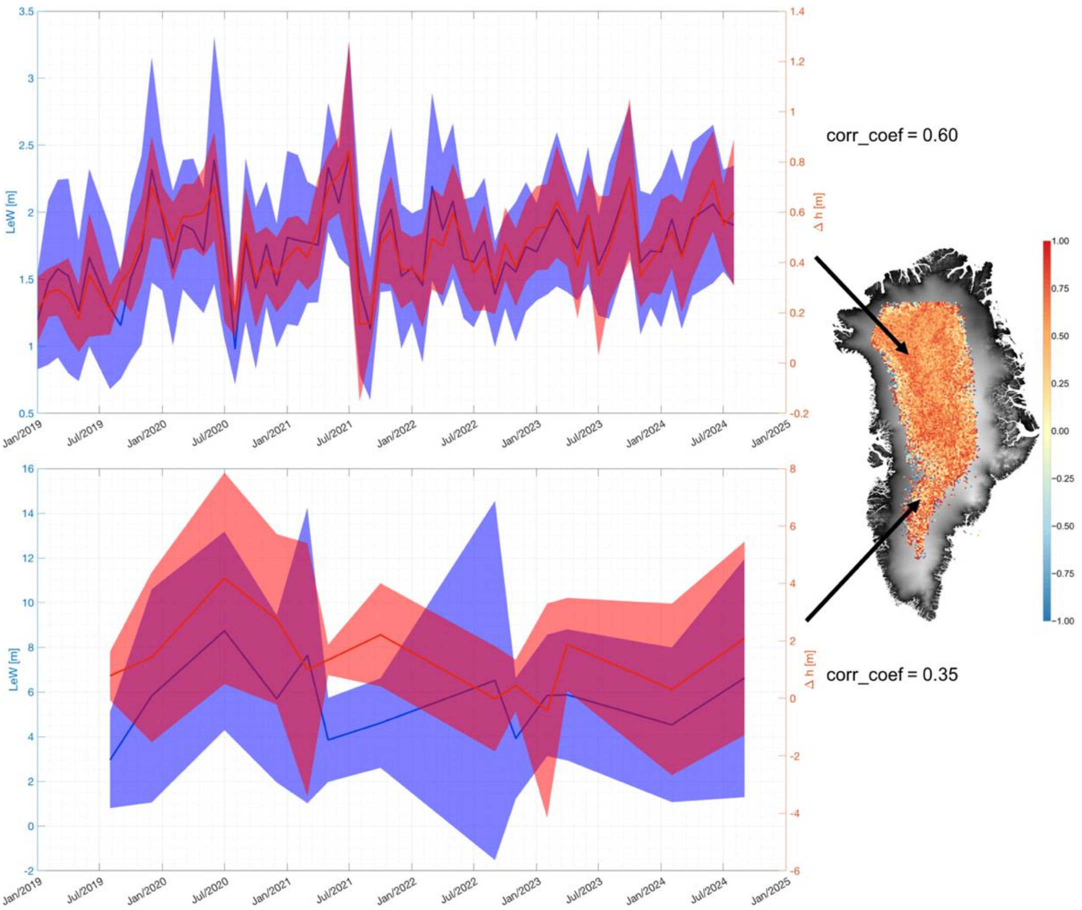

Figure R3. Example time series of LeW (blue) and dh (red) for two locations. Shaded areas show the standard deviation of the inspected parameters within the pixel.

The figure shows that towards the coast of Greenland, the distribution of LeW and dh is not as uniform as in the interior, exhibiting large uncertainties. This can be due to the uncertainties in LeW, in ICESat-2 height measurements, as well as in CryoSat-2 height estimates. We have added the following explanation in Line 330:

"The lower correlation and significance towards the margins can likely be attributed to the compromised performance of ICESat-2 elevation measurements due to the large slopes, rough surfaces (Smith et al., 2023c) and scattering biases in the low-elevation regions (Smith et al., 2018). These biases can be propagated to the derived Δh, which may not properly indicate volume scattering variations."

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”?
(Now Line 334) We agree. The concept of “penetration depths” has been replaced with “laser-radar height offsets” (because laser height measurements are higher) in or “a proxy for Ku-band radar penetration depth” 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 has been removed from the revised manuscript (shown in the response to comment 26).

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 have been differed and the colorbars are labelled in the maps in the revised manuscript except for Figs. 5a and 5b, where the names of the variables are too long and are therefore added to the titles.

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 (now Lines 347-351) 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 have added this explanation 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?
OIB MCoRDS does indeed operate in a much lower frequency, but for our study, the main difference lies primarily in how the radar waveforms are analysed. 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.

Now we realised that this comparison can really cause confusion, especially because the main conclusion from this comparison is that "CryoSat-2 has a better spatial and temporal continuity than OIB". Therefore, all the comparison with dz has been removed from 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 have elaborated on this point in the revised manuscript (Line 429 onwards):

"According to Lacroix et al. (2008) who compared waveform parameters from S-band and Ku-band radar altimeters, the impact of surface scattering as well as from snow grain size decreases with an increasing radar frequency. According to Scanlan et al. (2023) who derived firn properties using both Ku-band and Ka-band radar altimeters, radar altimeters operating in a lower frequency are sensitive to firn densities at a larger depth. For future dual-frequency radar altimeters, e.g. the Copernicus Polar Ice and Snow Topography Altimeter (CRISTAL) mission which operates in both Ku- and Ka-bands, the different penetration abilities and sensitivities to firn properties offer the potential of a multi-layered analysis approach. For a higher frequency such as Ka-band, the penetration depth is smaller, hence we expect a quicker recovery of LeW after a melt event than that of Ku-band. This different recovery rate can help future studies to locate the subsurface refrozen layers and derive accumulation rate."

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?
(Now Fig. 8) 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 have removed the curves from 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".

(Now Line 461) This has been corrected in the revised manuscript as “subsurface”.

36) Line 441. “stratigraphy” is misspelt in the Code and data availability section.
This comparison has been removed from the revised manuscript.

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Response to Referee 2 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.

Referee comments

The study provides valuable insights into firn properties using altimetry data from CryoSat-2 (CS2) and ICESat-2 (IS2), but there are several areas that need clarification and refinement to validate the conclusions. Before any major insight or conclusion can be drawn I find that there are several aspect of the methodology that needs more validation or attention to ensure the accuracy of the results. Except that I find that its a very interesting approach that can yield some good scientific insight into this area.

Below are detailed comments and suggestions to help improve the study with a focus on the main methodology for the altimetry components and firn models.

General Comments

LeW Computation:

In Figure 1 (related to L87), it is clear that using thresholds at 0.01 and 0.99 may result in unrealistic LeW values unrelated to the volume/surface scattering ratio. How exactly is LeW computed? Is a peak finder algorithm employed? I strongly suggest either smoothing the waveform for better LeW extraction or using the Offset Center of Gravity (OCOG) method to compute the width after identifying the leading edge. Alternatively, the overall OCOG amplitude could serve as the max. The critical objective is to minimize jitter in the LeW estimation. A specific example is pixel C, where the algorithm identifies a maximum beyond the true leading edge, likely near bin 40–45, which aligns with observations for pixel A.

We directly normalised each waveform using the maximum power and searched for the first bin (except for the initial noisy bins) that exceeds 0.01 and the last bin that exceeds 0.99 of the normalised waveform. We agree with the recommendations of the referee, and have improved the method in the revised manuscript. From here onwards, we compute the OCOG amplitude to serve as the maximum power, and following another one of the referee's comments below, we define the beginning and the end of the leading edge using thresholds at 0.05 and 0.95.

In addition, in response to one of the comments below, we adopted Baseline E data, and the results shown in this document will be based on Baseline E instead of Baseline D. Accordingly, the time series of our study have been extended from 2011—2021 to 2010—2024. Everything has been implemented in the revised manuscript.

DEM (Section 2.3)

The REMA description should be moved to the beginning of the data description section. Introducing it first provides essential context, as the DEM is referenced throughout both the CS2 and IS2 sections.
This has been implemented in the revised manuscript (now becomes Section 2.1).

FDM (Section 2.4)

Given the availability of multiple firn models such as GSFC and GEMB, have you compared their results against the IMAU-FDM model? Previous analyses have shown substantial spatial and temporal differences among these models, which I think is crucial when evaluating penetration depth from laser and radar measurements. At a minimum, a discussion on the potential impact of model differences is necessary to gauge the validity of the results.

Furthermore, models like GSFC and GEMB have been updated to include data through the end of 2024, which presents a valuable opportunity to extend your CS2 and IS2 time series analysis. Incorporating these more recent datasets will enhance the robustness of your study and help provide more insight into how melt events affect the LeW and elevation relationships.
While we agree with the reviewer that comparing the altimetry results with more firn models would strengthen our analysis, we do not do this for the following reasons:

1) We believe that adding more firn models to our analysis has little added benefit. We already make use of two different models, driven by two different RCMs (IMAU-FDM and MAR's firn module). While the models differ in their exact density, temperature and water content, they qualitatively agree in their response to 2012's melt season, the subsequent drier years, and increased melt after 2018. The focus in this paper is on the latter and different firn models would show similar trends. This makes sense, as these trends are largely driven by the climate and not the firn physics. We explicitly refrain from making quantitative statements that would not be supported by a single firn model.
2) A comparison of the performance of different firn models is out of the scope of this work. IMAU-FDM's capabilities have already been compared to other models under idealised non-melt conditions (Lundin et al., 2017), melting conditions (Vandecrux et al., 2020) and runoff capabilities (Machguth et al., under review).
3) Finally, there is a simple practical reason for not including GEMB and GSFC in our analysis: the mean density of the uppermost 1.5 m of firn, which is used extensively in our analysis, is not publicly available online for either model, whereas IMAU-FDM's and MAR's data was already available to the authors. While we could have asked the developers of GEMB and GSFC to also provide us with these data sets, we decided against this given the reasons above and include the potential of GEMB and GSFC in Discussion.

In the revised manuscript (Line 443), we have added:

"Although not presented in this study, the up-to-date Goddard Space Flight Center (GSFC) firn model (Medley et al., 2022) and Glacier Energy and Mass Balance (GEMB) firn model (Gardner et al., 2023) can also be incorporated in the satellite

time series analysis by both qualitatively indicating the presence of subsurface ice lenses and by quantitatively deriving firn properties with radiative transfer models."

Resolution (Sections 3.1, 3.2, and 3.3):

The current 50x50 km binning resolution seems excessively coarse and likely introduces decorrelation, especially for the "dz" variable but also to elevation as you are mixing a lot of different elevations regions. Increasing the spatial resolution would likely improve both spatial and temporal patterns and correlations.
The increased spatial resolution of 10kmx10km has been implemented in the revised manuscript.

Correlation (Section 4.3):

The low correlation values (~0.3) are surprising, especially when using a 50% threshold, which should generally yield higher correlations due to its more sensitive to volume change. I remember seeing much larger correlations in both Antarctica and Greenland using the same methodology you have provided. A few factors may contribute to this, including the coarse resolution and the LEPTA slope correction method. LEPTA may inadvertently remove signal by basing its correction on leading-edge range information that varies over time. Testing a more traditional slope correction method, as suggested you explained in Li et al. (2022), would help to better understand this. Additionally, localized analyses are likely to reveal higher correlations, as elevation usually de-correlates a lot more over larger distance while LeW might have larger spatial cohesion.
We appreciate the referee for pointing out the problem. First of all, we realised that the problem lies indeed in our original intuitive LeW estimation, where we directly used 0.01 and 0.99 thresholds to cut the normalised waveform. After following the referee's suggestions to use the OCOG amplitude as the maximum amplitude and using 0.05 and 0.95 thresholds, the correlation between LeW and dh improved to approximately 0.6 (Fig. R4 in blue).

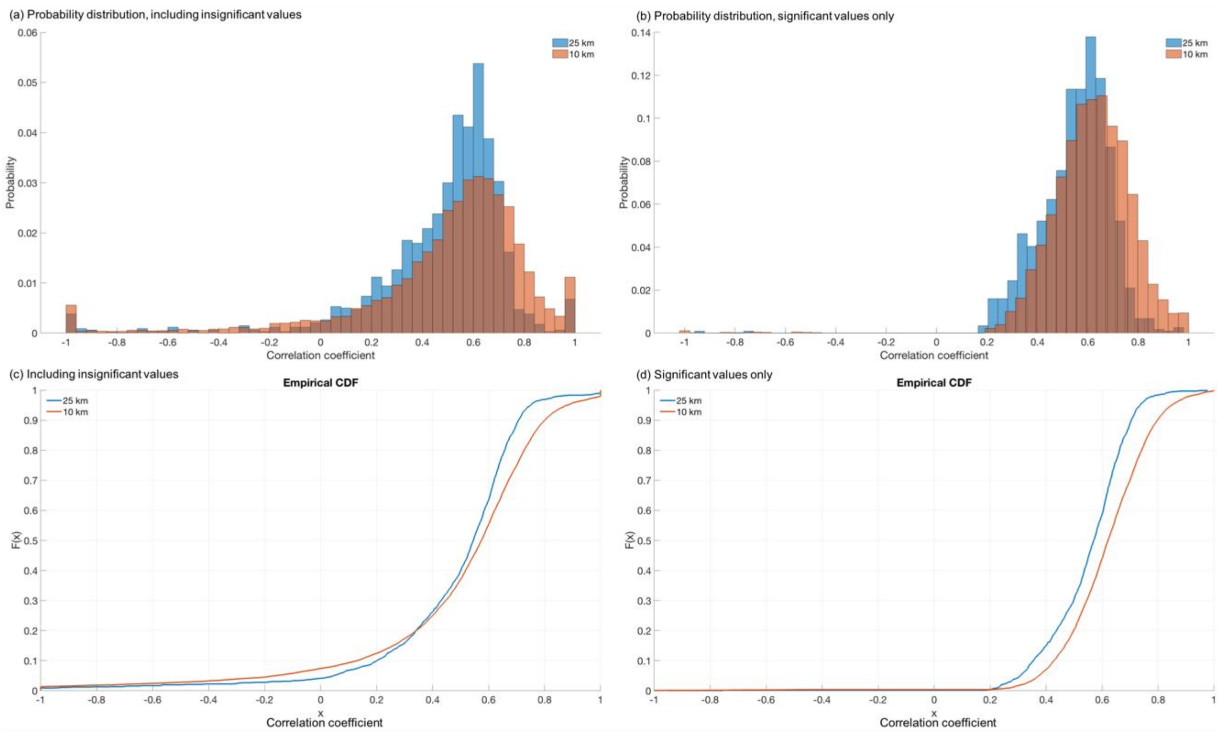

Figure R4. (a)—(b) Probability distribution histogram and, (c)—(d) cumulative distribution function of correlation coefficients when using the improved LeW estimation and different grid resolutions. (a) and (c) include all correlation coefficients. (b) and (d) only use the correlation coefficients with p-values not higher than 0.05.

To inspect the effect of different resolutions, we used a 10km x 10km grid to derive the correlation coefficients and analysed the results. In order to provide a more straightforward assessment, we compare the histogram of correlation coefficient using different resolutions, as shown in Fig. R4. It is true that according to the probability distribution, the number of pixels with correlation coefficients higher than 0.75 increased, compared to the 25km x 25km case. In addition, both resolutions result in insignificant (p-value > 0.05) correlation coefficients towards the Greenland coastal regions. Therefore, we remove the insignificant values and evaluate the probability and cumulative distribution function (CDF), as shown in Fig. R4b and d. After removing the insignificant values, it is more apparent that the correlation coefficients derived using the 10km x 10km resolution is more concentrated above 0.7, while those derived using the 25km x 25km resolution is only concentrated between 0.5 and 0.7.

Finally, we also inspect whether using the traditional slope method (Bamber, 1994) and the point-based method (Roemer et al., 2007) can further improve the correlation. For this comparison, we show the map of correlation coefficients in Fig. R5, using the 10km x 10km grid determined above. The figure shows that the slope method only results in high correlation between LeW and dh in the Greenland interior with little topography, and the point-based method results in slightly lower (~0.5) correlation coefficients than LEPTA. Our explanation is as follows. The elevation estimates from LEPTA method best represents the elevation of the subsurface, which in turn indicates the volume scattering effects. This can be also reflected by LeW, which varies due to the variation in volume scattering. The slope method, on the contrary, does include the topography signal that theoretically also has an impact on LeW. However, its implication of the laser-radar height offsets can be compromised, as it introduces the uncertainties caused by the simple assumption that the topography within the radar pulse-limit footprint can be represented by a slope. Similarly, the point-based method may also suffer from the simplication of a fixed footprint size, resulting a slightly less ideal derived dh, as shown in Li et al. (2022).

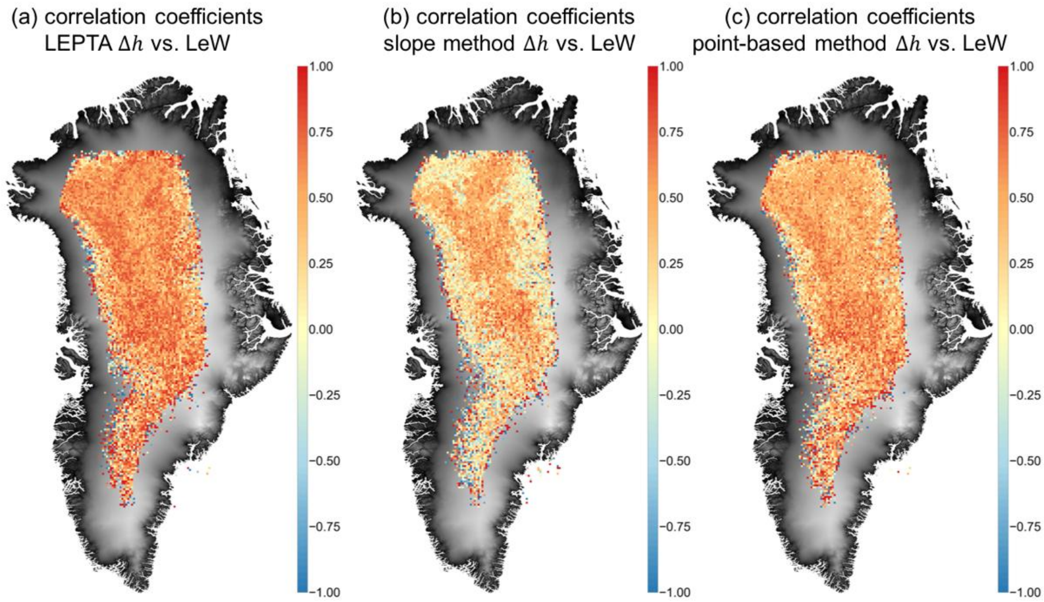

Figure R5. Comparison of correlation coefficients between LeW and dh derived using (a) LEPTA, (b) slope method (Bamber, 1994; Li et al., 2022), and point-based method (Roemer et al., 2007; Li et al., 2022).

Therefore, we believe that 0.6 is a sufficiently high correlation coefficient between dh and LeW, as it on the one hand indicates that LeW increases simultaneously with the laser-radar height offset, indicating an increased volume scattering. On the other hand, it is also consistent with the observation of Nilsson et al. (2015), where the extreme melt event has a more prolonged effect on LeW than other parameters derived by a satellite radar altimeter.

Specific Comments

L54: The Nilsson et al. (2015) study was not limited to NEEM; it covered the entire LRM region, although the time series presented was from NEEM.

(Now L64) This has been changed from

"Despite the advances in using altimetry to monitor Greenland's firn, the evaluation of firn properties has been limited to either to periods without extensive melt (e.g. January 2013 to January 2019; Scanlan et al., 2023) or small regions (e.g., NEEM site; Nilsson et al., 2015)."

to

"Despite these advances in using radar altimetry to monitor Greenland's firn properties—particularly in assessing the effects of melt and refreezing—existing

studies have largely been confined to a period without extensive melt (e.g., January 2013 to December 2018; Scanlan et al., 2023), or to the short timeframe immediately following the 2012 melt event (e.g., up to 2014; Nilsson et al., 2015). The impact on the long term, especially following the 2019 melt (Tedesco and Fettweis, 2020), remain insufficiently monitored."
in the revised manuscript.

L59: Provide a theoretical penetration depth for Ku-band frequencies. For Ku-band over the Greenland Ice Sheet (GrIS), penetration depth is typically 1-2 meters. Additionally, mention that the bias is retracker-dependent.
We have added
"According to Ridley and Partington (1988) and Davis and Zwally (1993), the penetration depth may range from a few centimetres to several metres, depending on the firn status ((e.g. dry, wet, refrozen; Slater et al., 2019)) and the retracker (Michel et al., 2014; Simonsen and Sørensen, 2017; Li et al., 2022)."
in Line 33 of the revised manuscript.

L85: Consider updating to Baseline-E, as it includes significant improvements in waveform processing compared to Baseline-D.
Throughout this document, the results are generated using Baseline E data (as mentioned in the comments above). Due to the higher data availability of Baseline E, the time series have been extended as well. These updated results have been presented in the revised manuscript.

L87: The 0.01 threshold for LeW seems too low; most studies use thresholds between 0.05 and 0.15 to account for noise. What is the impact of changing these values to 0.05 and 0.95? A more robust approach would be to compute LeW using OCOG parameters, which are less sensitive to noise.
(Now Line 110 onwards) When we aimed to observe the temporal variation of LeW, changing the 0.01 threshold to 0.05 did not result in essential changes. However, following the general comment and this specific comment of the referee, we have implemented the more conventional and robust approaches in the revised manuscript (using OCOG amplitude as the maximum and using 0.05 and 0.95 thresholds).

L91: The 50% threshold is appropriate for focusing on volume scattering rather than surface scattering. However, the LeW extraction algorithm needs to be redefined or better explained. OCOG-based methods would offer greater robustness.
This has been updated in the revised manuscript (as mentioned in the comments above).

L98: Include a map figure or inset to indicate pixel locations, as their current placement is unclear to the reader.
The pixel locations should be the same one as in Fig. 3 (Fig. 1 in the revised manuscript), which did not appear in L98 yet. However, we realised that the original Fig. 1 did not have added value in the overall readability, hence have removed it from the revised manuscript.

L103: Justify the use of a 50x50 km binning resolution, as it appears excessively coarse. Correlation length analysis could support this choice, or consider aligning the resolution with firn models, which typically have a 10 km resolution. If empty pixels result from a 10 km grid, they can be filled using gentle interpolation.
(Now Line 227) This choice was originally used in Li et al. (2022) to ensure that every inspected pixel should have sufficient (more than 10) data points to compute the reliable statistics, i.e. mean, median and standard deviation. We admit that this was an intuitive choice, therefore we have implemented different pixel sizes (50km x 50km, 25km x 25km, and 10km x 10km) to generate LeW time series, and show the results in Fig. R6. For each sub-plot, y-axis shows the distance along the north-south transect. The overall spatio-temporal patterns of using different resolutions are similar, while the 25km x 25km and 10km x 10km time series indeed show better details.

Finally, due to the higher correlation coefficients between dh and LeW using the 10km x 10km resolution (as assessed above) and the consistency with firn models, we have now adopted the 10km x 10km resolution in the revised manuscript.

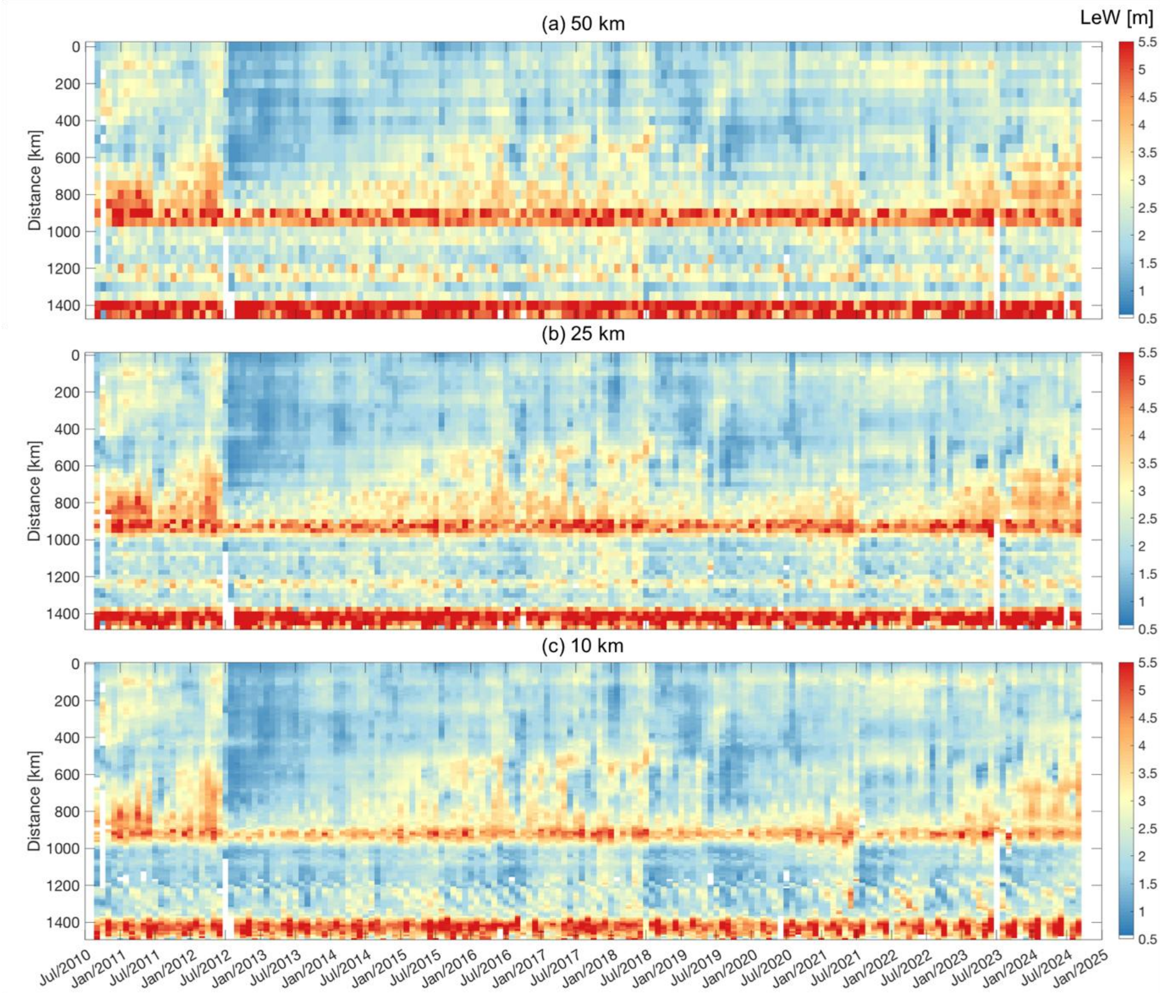

Figure R6. Comparison of LeW time series along the north-south transect when different resolutions are adopted.

L117: The DEM resolution (100 m) and search radius (50 m) may not be optimal. Wouldn't this setup yield identical DEM values for adjacent locations? A higher-resolution DEM (e.g., 10 m from REMA) would likely provide more accurate results, particularly in areas with complex topography.

We appreciate the suggestion. However, from Li et al. (2022), we found that loading the ArcticDEM with a resolution higher than 100 m was not feasible in MATLAB. One solution would be to crop the DEM, but this was not convenient for the processing chain, therefore we adopted 100 m as the finest resolution in our sensitivity analysis.

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