Response to Referees on egusphere-2024-3251

We appreciate the referee and the editor for their support in improving our manuscript. Please find our item-by-item responses below, with our replies highlighted in blue. The suggested changes have been implemented in the revised text.

General Comments

The manuscript "Assessing spatio-temporal variability of melt-refreeze patterns in firn over Greenland with CryoSat-2" by Li et al. presents an interesting study looking at variations in CryoSat-2 LRM waveform leading edge widths (LeWs) and how they might be used to infer conditions in Greenland Ice Sheet (GrIS) firn. The manuscript begins with an Introduction into the intersection of melting and refreezing with firn and their combined influence on GrIS mass balance. This is followed by an overview of how firn conditions have been assessed previously by way of remote sensing. Section 2 presents the various datasets used throughout the study and Section 3 presents how they authors leverage these data to assess GrIS firn conditions. Section 4 presents the main study results, while Sections 5 and 6 discuss the implications of the results and the present the main study conclusions.

Overall, having reviewed an earlier version of this manuscript, I found this version to be more refined and easier to follow. I want to thank the authors for all their hardwork engaging with and considering previous reviewer comments. I believe the authors successfully managed to integrate a more consistent arc into the manuscript and the scope of the study, bringing together all the different datasets, is commendable. At the same time, there has been substantial change to the manuscript and I have some specific comments on this version that I think need to be addressed before the manuscript can be accepted for publication in The Cryosphere. These will be addressed in the enumerated comments presented below. I do not believe any one of them would take very long to address but their totality would improve the manuscript's coherence, clarity and impact. My hope is that the authors find them constructive as they continue revising their manuscript.

We appreciate the comments of the referee. The following comments are also constructive and are really helpful for improving the manuscript.

Specific Comments

1. My one larger comment is that I would recommend the authors more clearly outline what they view as the central implications/impact of their being able to link CryoSat-2 LeW to firn conditions. In the Introduction (Line 71), the authors state the main objective of their study but there is no definitive statement as to why this type of assessment is important (what is gained from linking LeW to firn conditions? How does this study link back to the mass balance considerations introduced at the start of the Introduction?). On line 417 in Section 5, they seem to suggest their LeW results could play a role in refining firn models but do not provide a path for doing this. Are they implying firn modelers simulate CryoSat-2 LeWs as part of their model

validation activities? What would this look like knowing the discretized nature of modeled subsurface structures? I think what I am missing is how the authors see their results contributing to improving the current state- of-the-art. The authors present an interesting and seemingly rich dataset but the discussion about how they see it contributing to the community is underdeveloped. Including more in this direction would strengthen the studies impact and readership in The Cryosphere.

On Line 71, we have added:

"By assessing how melt--refreezing processes affect the CryoSat-2 LeW, we aim to improve the understanding of the stability of the Greenland Ice Sheet and its response to climate change, and to explore the potential of using radar altimeters as a complementary tool for providing a comprehensive observation of Greenland firn properties."

Line 417 onwards (now Line 423) has been elaborated as

"This sensitivity suggests that LeW data could play a crucial role in refining firn models and improving radiative transfer models. For example, currently, radiative transfer modelling has been most successful in understanding firn property variations in Antarctic dry-snow zones (Adodo et al., 2018; Larue et al., 2021). How the refrozen layers in high-elevation zones in Greenland act as a reflective layer and hence affect the radar altimeter signal could be better represented in the modelling. Subsequently, the density and grain size changes following the melt–refreeze events and the potential new-snow deposition could also be derived with the combination of radiative transfer modelling and radar waveform information. Such a method has the potential of improving firn models through data assimilation (Weng, 2007), especially for higher elevations, where existing models may underestimate the impacts of melt events on volume scattering."

2. On line 7, the authors state LeW is the "most" sensitive parameter to changes in volume scattering. I do not think this is supported by what is presented in the manuscript. It has shown to be strongly sensitive, but confirming its primacy compared to other metrics is not established. I recommend the authors consider adjusting this language.

We have changed the sentence from "...the parameter most sensitive..." to "...a parameter strongly sensitive".

3. Line 31 (and line 468), please check the citations and attributions are correct as I don't think the Helm et al., 2014b DEM is actually used in the manuscript anymore.

The reviewer is correct. We removed the references and attributions to Helm et al. (2014b) from the revised manuscript.

4. I recommend the authors be clearer with what they mean by "... beyond the inflection point ..." on line 38 knowing that not everyone who reads the manuscript will be familiar with radar altimetry waveforms or what they look like.

(Now Line 37) This statement is based on the model of satellite radar altimeter waveforms over ice sheets presented by Ridley and Partington (1988). They noted that *"after the area of the surface illuminated by the pulse becomes constant, surface scattering ceases to increase and this point is marked by an inflection point (or 'cusp') in the altimeter return."* To improve clarity, we have revised the sentence from: *"Surface scattering dominates the start of the waveform, while volume scattering becomes predominant beyond the inflection point, where the illuminated surface area becomes constant."* to *"As explained by Ridley and Partington (1988), surface scattering dominates the start of the waveform, while volume scattering becomes predominant beyond the point at which the illuminated surface area becomes constant (regarding the latter, see (Chelton et al., 2001, Sect. 2.4.1))."*

5. I recommend the authors clarify what they mean on line 38 with "... surface area becomes constant." as I am not sure I follow what they mean. Assuming the CryoSat-2 wavefront can be generally thought of as circular once 100's of kilometers from the spacecraft, where the wavefront intersects the surface will continually expand with time.
While it is true that the radar wavefront expands continuously as it propagates, the effective pulse-limited footprint area does not. For a thorough explanation, we refer to Chelton et al. (2001) section 2.4.1. In short, the pulse-limited footprint area grows linearly with time until the trailing edge of the pulse intersects the surface (assumed planar). Thereafter, the footprint becomes an expanding annulus which area remains constant. Indeed, the radii defining the outer and inner perimeters of the annulus continue to grow.

We have added the reference to Chelton et al. (2001), Sect. 2.4.1.

6. Please provide a reference supporting the statement on line 43 "Depending on thickness ...".
(Now Line 44) We have added references to Nilsson et al. (2015) and Otosaka et al. (2020) to the revised manuscript.

7. I recommend the authors provide a version number and/or reference for the ArcticDEM when it is first mentioned on line 74. It comes eventually on line 91 but should appear earlier.
The reference for the ArcticDEM has been added on Line 74 (now Line 77).

8. I recommend the authors clarify the spacing of the 1500m-3000m elevation groups mentioned on line 89. Are they evenly spaced? Evenly according to elevation or ice sheet area?
Yes, they are evenly spaced according to elevation. We have added the specification on Line 89 (now Line 92).

9. In Section 2.1, the authors state that they use two version of the ArcticDEM (100m and 1km). This is fine, though I would recommend the authors provide a quick justification for why the two different versions are used. For example, why is the 100m version appropriate for slope correction

and 1km for macro-scale roughness and not and vice versa? It seems odd to use two versions without presenting a reason for why.
We have switched to using the 100m ArcticDEM throughout the manuscript.

10. In Section 2.2, I'd also recommend the authors include the level of the LRM data products they are using (e.g., Level 1B, Level 2?). I would also ask the authors consider including a simple diagram showing the basics of LeW calculation and OCOG retracking for those reading the manuscript (e.g., firn modelers) who may be unfamiliar with what the radar waveform would look like. As it stands, the authors expect the reader to be well-versed in some nuanced aspects of radar altimetry (e.g., range bins, waveform shape, OCOG threshold) that may limit the reach of the manuscript in The Cryosphere.
We have added the information "L1b" on Line 111 of the revised manuscript.

Regarding the suggestion by the reviewer to include a simple diagram showing the basics of LeW calculation and OCOG retracking, we have added a new Fig. 1 to the revised manuscript.

11. I recommend the authors clarify the discrepancy between the number of LeWs extracted from the LRM dataset and the elevation estimates. How are their more LeWs compared to elevation estimates for, what I assume is, the same number of waveforms? Similarly, in Section 2.3, I'd recommend the authors qualify why there are two orders of magnitude less ICESat-2 elevations compared to CryoSat-2. ICESat-2 has not been flying as long as CryoSat-2 but does have a greater along-track data density and multiple beams, so this order of discrepancy is a little suprising.
We would like to begin by noting that we have updated the numbers in the revised manuscript. The figures in the previous version inadvertently included LRM data points collected over the oceans.

The higher number of elevation estimates compared to LeW estimates is due to an additional data editing step applied during the LeW determination. Specifically, we exclude all waveforms for which the normalized power in the initial part of the waveform (i.e., beyond range bin n_1 , which is set to 10 in this study) exceeds 5% of the OCOG amplitude. In such cases, it is not possible to determine $b_{0.05}$ (see Eq. 1), and thus no LeW is derived. We have added this explanation to the revised manuscript (Line 123).

Regarding the difference between the number of CryoSat-2 elevation estimates and ICESat-2 elevations, we would like to clarify that the ICESat-2 point count mentioned in Section 2.3 refers only to those points that are in close proximity—both spatially (within 50 m) and temporally (within the same month)—to the selected CryoSat-2 measurements. For some months, the number of spatially and temporally coincident ICESat-2 observations is relatively low. For example, in January 2019, only about 3% of the CryoSat-2 points have a corresponding ICESat-2 measurement within 50 m (see Fig. R1). To make this clearer, we revised the sentence from (Line 153): *"The search for ICESat-2 points within a 50 m radius of each CryoSat-2* *measurement yields a total of approximately 4.53 × 10^5 points.”* to “*The search for ICESat-2 points acquired within the same month and within a 50 m radius of each CryoSat-2 measurement yields a total of approximately 4.53 × 10^5 points.”*

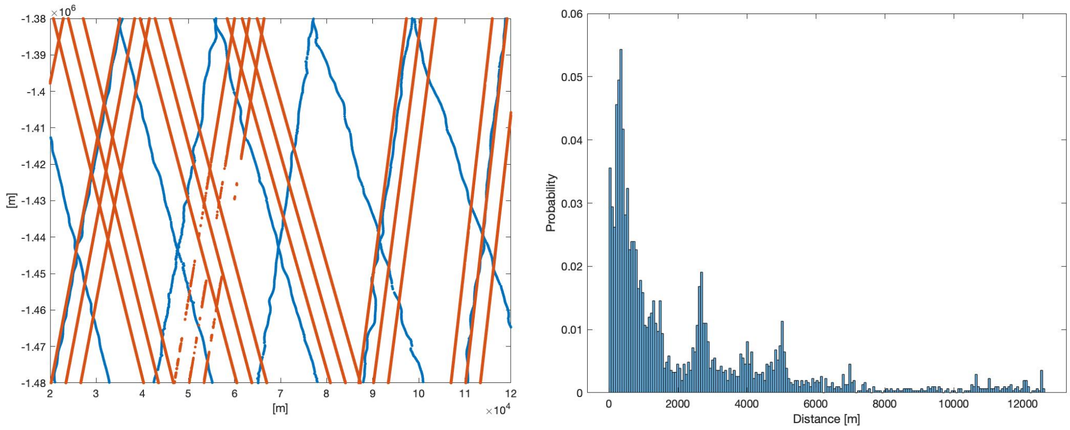

Figure R1. Left: horizontal locations of CryoSat-2 (blue) and ICESat-2 (red) points obtained in January 2019 within a zoomed-in region. Right: histogram of distances between each CryoSat-2 point and the nearest ICESat-2 point.

12. I recommend moving lines 163-165 to the start of the preceding paragraph (i.e., combine with the paragraph starting on line 158).
(Now Line 170) Done.

13. Is there something missing at the end of the sentence starting on line 162 “When MWC>0 ...”? Please check.
(Now Line 173) Correct. The sentence has been revised into “When MWC>0, meltwater is present in the firn layer; thus, the altimeter-derived parameters are primarily influenced by meltwater content rather than firn properties.”

14. Is there a “the” on line 173 in “For consistency with the CryoSat-2 ...”? Please check.
(Now Line 180) The “the” has been added.

15. On line 200, the authors state it is critical that the in situ records they use contain the 2012 melt event. I recommend they state their logic for establishing that the density spikes in the in situ profiles are indeed from 2012 (I assume it is just because these spikes are the largest, but this should still be reflected in the manuscript).
(Now Line 204) It is documented in the Schaller et al. (2016), the Otosaka et al. (2020) and the MacFerrin et al. (2022) studies that the density spikes are from the 2012 melt event. However, it is also true that we used more recent data, i.e. Vandecrux et al. (2023). In this case, our criterion has changed. Line 203 has been therefore revised as

“We include available and published in situ density profiles in our analysis if they meet the following criteria: (i) the acquisition site falls within the CryoSat-2 LRM coverage; (ii) the acquisition time is within the CryoSat-2 operational

time, and (iii) the acquisition is vertically continuous rather than a single measurement at a specific depth."

We also added another explanation regarding the density spikes on Line 211:

"Among the adopted in situ density profiles, the inclusion of the 2012 melt layer is particularly important, as it offers strong evidence that the extreme melt event in that year produced a distinct high-density layer which can also be identified in modelled firn densities (Schaller et al., 2016a; Otosaka et al., 2020; MacFerrin et al., 2022). This layer becomes progressively buried in subsequent years, enabling the observed recovery in LeW. Similarly, recent melt events can appear in the recently acquired in situ density profiles (Vandecrux et al., 2023), as they show similar spikes (approximately 25 % higher than the average density over the top 5 m) as the 2012 melt event."

16. I recommend the authors include references to the original implementations of RSR technique on line 212 (e.g., Grima et al., 2012 https://doi.org/10.1016/j.icarus.2012.04.017 and Grima et al., 2014 https://doi.org/10.1016/j.pss.2014.07.018).
(Now Line 221) The references have been added.

17. I would recommend the authors include dates for each of the in situ measurements presented in Figure 1 as this is important context for what is discussed on lines 303 to 306.
(Now Fig. 2) The dates of the acquisitions have been included.

18. I would recommend the authors be careful with the use of "validation" on line 217. I do not believe the Scanlan et al. (2023) study went as far as to fully validate their results.
(Now Line 226) Agreed. We have changed "validation" into "assessment".

19. Line 240 is where it is most evident, but it also applies to Figures 4, 6, 7, and 8 along with the surrounding discussions. The authors use a few different definitions for what is considered an "anomaly" in the different datasets they consider. On line 240 it is difference from a long-term mean, in Figure 8 it is a difference relative to Winter 2010/2011, but they are all just called "anomalies" and presented without qualification. This is confusing as it isn't always clear if what is being presented/discussed is immediately comparable. I recommend the authors either 1) define all "anomalies" the same way or 2) be specific and explicit with how the data are being represented. For the latter, a part of this could be something like replacing "anomaly" in Figures 6, 7, and 8 with "difference relative to October 2010-April 2011 mean". This would make things much clearer and understandable for the reader.
Thanks for pointing this out. We have adopted the suggestion (option 2) by the reviewer. Now in the captions and related descriptions of Figs. 8, 9, and 10, we have replaced "anomaly" with "difference relative to October 2010-April 2011 mean".

20. Please clarify if summer months are also removed from the annual means introduced on line 273.
(Now Line 282) Summer months are included. We have added "including summer months" to the revised manuscript.

21. I recommend the authors be explicit on line 284 in that the increase is ~0.5m per year (I assume this is what they meant) and not a cumulative increase (which would be trickier to immediately get out of Figure 3a).
(Now Line 293) We have clarified that the increase is ~0.5m per year.

22. On line 289, is it possible to provide support (e.g., a reference) for the "... strong melt events between June and December 2018 ..." statement similar to the Tedesco and Fettweis (2020) reference for 2019?
(Now Line 299) We have added a reference to Houtz et al. (2021).

23. For Figure 3b, much of the discussion surrounding the results is done by contrasting to 2012. With this in mind, I would recommend the authors consider if there'd be something to gain by referencing LeW changes to 2012 and not 2010. I think this would simplify the interpretation of the LeW timeseries by contrasting all changes relative to the state where they interpret there to be the least amount of volume scattering.
We appreciate the reviewer's thoughtful suggestion. While referencing the 2012 LeW as a baseline could indeed simplify interpretation related to the observed minimum in volume scattering, we chose to reference the 2010 LeW in order to highlight two key aspects: (i) the partial recovery of LeW (and thus the firn layer) following the 2012 melt event, and (ii) the fact that this recovery does not return to pre-2012 levels, as discussed on Line 298. Using 2010 as the reference year thus allows us to present a more comprehensive picture of both the impact and the long-term persistence of the 2012 melt-induced changes. For completeness, we have included the alternative version of the figure as Fig. R2.

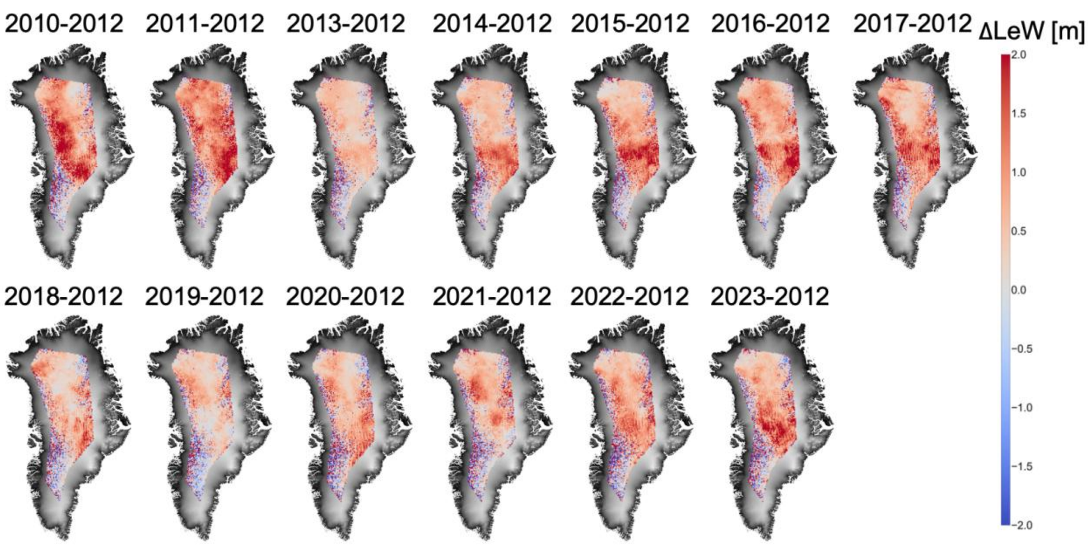

Figure R2. Changes in non-melt season average LeWs for the seasons 2010–2011 and 2013–2023 relative to the 2012 non-melt season average. Years refer to the start of each non-melt season.

24. In Section 4.2, I'd recommend the authors consider restructuring to align with Figure 4 (or re- structure Figure 4). It is odd to me to discuss the bottom half of the figure (e.g., Figs. 4e and 4g on line 308) before the top. Also, I'd recommend the authors consider splitting Figure 4 between transect and elevation similar to Figures 6 and 7.
(Now Figs. 5 and 6) The orders of the subplots are restructured and the original figures are split.

25. On line 338, the authors state correlation coefficients do not exceed 0.9 but this seems to be contradicted by what is shown in Figure 5d. Are the authors referring to an average correlation coefficient? Please clarify.
(Now Line 352) We have changed it into *"most correlation coefficients remain below 0.9"* as the number of pixels that have correlation coefficients larger than 0.9 is smaller than the numbers of pixels with correlation coefficients between 0.3 and 0.8.

26. I do not completely follow the authors logic on lines 373-375. How would they explain an increase in model density if not caused by melt/refreezing? What other process are they envisioning that would operate in the models with such an annual timescale?
(Now Line 380) This was a wrong observation from our side. The annual cycle of LeW decrease does exist concurrently with the annual cycle of densities, although not as pronounced as the abrupt anomalies due to melt--refreezing. We have removed these statements from the manuscript.

27. Should "annually" in line 376 be "annual"? Please check.
(Now Line 382) This has been corrected.

28. The sentence on lines 378-379 is confusing to me and I think it is the multiple uses of "past". I recommend the authors be more specific with the exact years they mean for "the past decade" and "in the past".
(Now Line 384) This sentence has been revised into

"Since 2012, the firn density of the upper 1.5m has been notably higher---by up to 50 kg m^{-3}*---compared to the period prior to 2012.*

29. Is "revolution" on line 387 a typo? Please check.
(Now Line 393) Indeed. Changed into "evolution".

30. I recommend the authors replace "... ArcticDEM data." on line 397 with "... ArcticDEM mosaics." to make it clear what specific date product they are referring to.

(Now Line 403) This has been changed from "ArcticDEM data" into "ArcticDEM mosaics".

31. I recommend the authors expand a bit what they mean with "... up-to-date ..." models on line 444. Are they referring to older versions used in some other uncited study?
(Now Line 456) We intended to highlight that the GSFC-FDM is also available up to year 2023 (similar to the availability of MAR data). However, we agree that the phrase "up-to-date" may have been unclear in this context, so we have removed it from the manuscript to avoid confusion.

Reference
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Chelton, D.B., Ries, J.C., Haines, B.J., Fu, L.L., Callahan, P.S., 2001. Chapter 1 satellite altimetry. In: International Geophysics, https://doi.org/10.1016/S0074-6142(01) 80146-7.

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