egusphere-2024-3251

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.

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.

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.

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.

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.

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.

6. Please provide a reference supporting the statement on line 43 "Depending on thickness …".

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.

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?

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.

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.

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.

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

13. Is there something missing at the end of the sentence starting on line 162 "When MWC>0 …"? Please check.

14. Is there a "the" on line 173 in "For consistency with *the* CryoSat-2 …"? Please check.

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).

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).

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.

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.

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.

20. Please clarify if summer months are also removed from the annual means introduced on line 273.

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).

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?

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.

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.

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.

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?

27. Should “annually” in line 376 be “annual”? Please check.

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”.

29. Is “revolution” on line 387 a typo? Please check.

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.

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?