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Discussions

Interactive comment on "Sensitivity of CryoSat-2 Arctic sea-ice volume trends on radar-waveform interpretation" *by* R. Ricker et al.

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Review of "Sensitivity of CryoSat-2 Arctic sea-ice volume trends on radar waveform interpretation" by Ricker et al.

This is an interesting paper describing the processing and validation of CryoSat ice thickness retrievals and application to three years of Arctic data. It includes a sensitivity study of the impact of certain retrieval parameters on derived ice volume, as well as a thorough error analysis and budget. This is an important paper given that the authors are leaders in the processing of CryoSat data and that little has been published about the usage of CryoSat data so far. The authors include the innovative consideration of uncertain penetration depth of radar waves into the snow. Results should definitely be published. However, I am afraid that the paper is overloaded and therefore certain

aspects are not sufficiently presented or hard to understand or confusing respectively. I strongly suggest to consider to split the paper into several manuscripts and to more carefully focus on the key aspects in each of them:

- Study of the impact of different retracker thresholds

- Validation with airborne data

- Error analysis

- - Application to Arctic ice volume trends

There are key concerns with all these four topics which should be carefully addressed. These are outlined below. In addition to these major revisions, there are numerous language issues with the paper. I will send an annotated manuscript directly to the authors where these are marked.

Major concerns:

1. Retracker thresholds

The authors should carefully discuss the relation of their results to the results of Kurtz et al. 2014 in The Cryosphere Discussion. That paper shows that the appropriate threshold may be very close to the waveform maximum and should even be above 80%. Why are the results in the present manuscript different? Other studies have applied different thresholds to measurements over ice and leads, respectively (Laxon, Giles, et al.). Why has this approach not been considered (and possibly rejected) in the present manuscript?

In addition to these concerns, it is unclear why the authors chose to modify the re-trievals with the 40% threshold retracker with estimates of penetration depth and snow thickness. Shouldn't the same assumptions be made for all retrackers such that the results are really comparable?

In addition I find the treatment and interpretation of the 40% threshold very problematic.

The authors seem to confuse penetration issues with the effects of different retrackers. While it is true that these can have the same apparent effects, I don't think that thresholds should be adjusted to compensate for the (unknown) penetration uncertainty. Instead, a physical understanding of the meaning of different thresholds should be developed. Penetration is a physical fact depending on snow properties, and does not change if thresholds are changed. The comparison of the effect of different thresholds is interesting and important on its own, given the spread of thresholds used/suggested by the various existing studies. It does not need to be blurred by modifications based on assumptions on penetration.

2. Validation with airborne laser data.

This is a great data set and it is nice to see that validation has been attempted. However, why does the validation only include the 40% threshold retrievals? Please include the retrievals with the other thresholds as well to better see the impacts and their magnitudes of the different parameters on the agreement with the airborne data. Or do a separate validation study with more extensive comparisons.

3. Error analysis

I commend the authors for their most extensive error analysis. However, I think the study of penetration effects and overall error sensitivity should be significantly expanded and should include explicit sensitivity studies for the individual, novel parameters considered, e.g. penetration depth, level 1 b and SSA uncertainties, similar to what has been done by Giles, Kwok, and others. The authors may also refer to a paper by Tonboe et al (Canadian Journal of Remote Sensing 2010) which includes a description of the penetration issues and the scattering horizon, and sensitivity studies.

In addition I wonder if it is correct to consider the uncertainties due to unknown snow thickness and snow and ice densities as random (P. 1844, L19ff, and Eq. 12). They should rather contribute a constant bias (or vary within the scales of the natural regional and spatial variability), and will therefore not become smaller if averaged over longer

times or over more measurements in a given grid cell?

4. Ice volume trends

With the high uncertainties of the effects of snow and ice densities, snow thickness, and penetration depth, and ice type the conversion of freeboard to thickness adds an additional layer of complexity to the interpretation of the results. I strongly suggest to restrict the sensitivity study to maps if freeboard and its variability over the three year study period. This will show more clearly where anomalous patterns appear and their interpretation will be easier because they haven't been convolved with assumptions about snow thickness variability and ice types. It is trivial to see that higher freeboard in one year should correspond to thicker ice in general.

Some of the results, particularly the freeboard increase between March 2013 and November 2013 seem surprising as mentioned by the authors. They hypothesize that this is due to a thicker and wetter snow cover than usual. Although this could be one possible explanation indeed, I wonder how likely such an event is in reality and if there is any other evidence from (in-situ?) observations? Snow thicknesses during the CryoVEx 2014 campaign in the Beaufort Sea and north of Greenland were quite normal, although admittedly regionally very limited.

More minor but important comments and suggestions:

Title: change "volume trends" to "freeboard trends" and limit scope of paper on freeboard which is the most directly retrievable parameter and less error prone than ice thickness and volume?

Abstract: shorten and remove unnecessary introductory parts. L19-21: Unclear without further explanation. Use term waveform. L1-3: better point out this key result. Include validation results/mentioning should you decide to keep the validation section. Last lines: unclear.

Introduction:

Usage of term "scattering horizon" in L11-12 and elsewhere in the paper: Clearly define the scattering horizon, and use the term properly. Clearly distinguish it from the apparent effects of different thresholds. For example, on page 1853, L1-2, retrackers do NOT penetrate, and L12, the main scattering horizon is NOT penetrating - how can it??

P1834, L20: do you mean the RANGE TO the scattering horizon?

P1835 L 6 and elsewhere? It is commonly assumed that CryoSat cannot be used in the warm season. Therefore moist snow is less likely. Mention that stratified, high density snow can cause these effects.

Data and Methodology

P1836, L15: Which data products and versions did you use exactly?

P1837 L 4 and elsewhere: Replace exemplary with typical or example or example of

L 23: mention that surface type "lead" typically does not represent a single, large lead, but a mixed sea ice surface including a few leads within the footprint.

P1838, L9 and elsewhere: What does SSD stand for? Could it be SD? Explain what standard deviation you mean exactly, and of what variable.

q. 2&3: can you show waveform example that explain the differences between Eq 2 and 3? In Fig. 3?

Eq. 8: Although this is only a mathematical exercise, it would be better to use ice freeboard and not snow freeboard for the thickness conversion. Ice thickness depends less critically on uncertainties of snow thickness if ice freeboard is used than if snow freeboard is used. One of the (many?!) advantages of radar altimetry...

P1841 L19: how are these values identical or different from the "unified" ones from Kurtz t al 2014?

P1842 top: Why not used modal thickness instead or in addition to arithmetic mean? May be more representative?

L8-9: good idea to exclude Baffin Bay. Probably there are no in-situ data to support W99 anyways?

L12-14: usage of boxcar average is unclear? Do you do this in 2D, or for radial lines through the North Pole, or what?

2.1.2 Snow layer corrections: This section is critical for a thorough discussion of penetration depth, and needs much better introductions of the issues of wave propagation speed in snow and how this relates to the assumed snow thicknesses and penetration.

A conscious adjustment of thresholds can only be done if penetration depth is really known? Requires knowledge of regional variability as well?

Eq. 6: where does the factor of 0.28 come from and how was it estimated?

P1840, L19: check usage of "those". You mean values outside this interval are discarded.

P1843: explain what speckle noise is. Check numbering of eq. 6 and 8. Reference Kurtz et al for interpolation errors.

Over what spatial range is the mean SSA determined?

P1844: explain that sigma_p is defined through Eq. 6 and 7?

P1846: Snow freeboard from ALS: Should you not use the same leads as for CryoSat to have the same SSA? If you chose them from the ALS data differently the derived SSAs may be different?

P1847 L16-20: refer to CryoVEx data report (Willat and Haas) and include include results from 50 and 80% threshold retrackers.

Results

P1848 top: We know that the OSI-SAF ice type classification is uncertain at the ice type boundaries. Why are the errors along this boundary not larger, as CryoSat may experience some MYI in the FYI classified region, and vice versa?

L13: be careful with roughness effects on backscatter? The impact of volume scattering of MYI on high backscatter are just as high.

P1849 top: use "north of Canada" instead of "Canadian Archipelago"

Why do you assume that the 50% and 80% thresholds represent the same scattering horizon? The waveform amplitudes are clearly different and so must be the retrieved height. The issue as discussed above is that penetration is a physical fact which does NOT depend on a certain retracker in any way.

Discussion – use sub-headings for better structuring contents.

P1851 L 2: it is known that there can be quite deformed ice in this region downstream of the East Siberian Islands. This could cause the same effects on backscatter and freeboard.

P1853: Mention Willat results much earlier when introducing penetration

Last paragraph: How do results compare with other studies?

Conclusions

L10-16: mention some numbers to summarize results. Be more specific about key results in general.