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Interactive comment on "Retrieval of snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 returns" *by* Steven W. Fons and Nathan T. Kurtz

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In their paper "Retrieval of snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 returns", the authors develop and apply an algorithm to obtain snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 data. The waveform fitting is based on a forward model from earlier work of one of the authors and the main work here has been the to include backscatter from multiple interfaces (air/snow and snow/ice) in combination with snow volume backscatter for the application of sea ice in the southern hemisphere with its higher and more complex snow load. The authors compare the results with airborne validation data and earlier ICESat laser altimeter re-

sults and conclude that their algorithm can be used to obtain snow freeboard with the CryoSat-2 during the maximum austral sea ice extent in October. They also investigate the potential to retrieve snow depth from CryoSat-2 waveforms, but do not find realistic results except for an area in the East Antarctic sector.

I have been part of the team that produced freeboard maps of Antarctic sea ice from Envisat & CryoSat-2 data in the Climate Change Initiative. We used an empirical retracking scheme, which made it difficult to include the contribution of snow backscatter in the freeboard algorithm without prior knowledge of its impact on waveform shape. We are acutely aware of this deficiency in the CCI sea ice thicknesses for the southern hemisphere, and it is commendable that the authors attempt to overcome this issue. Therefore this study is for me a very welcome and novel contribution to the field of remote sensing of Antarctic sea ice thickness. The concept is generally sound, but there are a number points that need to be addressed before publication. I have detailed my concerns in the general and specific comments below:

General Comments:

1) It is understandable that maps of snow freeboard is the main objective of this work, but the authors show very little in terms verification of the different algorithm steps. The waveform fitting is based on nine free parameters, but it is not clear to me what the sensitivities are for resolving the snow backscatter properties. E.g. do snow backscatter/depth and surface roughness have an ambiguous impact on waveform shape and thus range? In general, the potential impact of surface roughness changes receives very little attention in the evaluation of the results. The fact that the snow depth resulting from the waveform fitting is unrealistic in wide parts of the study regions shows that there are issues with fully resolving the backscatter processes. A sensitivity study of the waveform forward model would help greatly to assess the skill of the algorithm for different snow conditions. Regional information on the average waveform fit quality would also be of interest to the reader as it might help to identify issues of the algorithm. I find this especially important as the direct validation with the Operation IceBridge ATM

and snow radar data is very limited compared to the spatial and temporal extent of the CryoSat-2 data and the otherwise indirect comparison to ICESat.

2) The conversion of surface elevations into snow freeboard is not state-of-the-art, in parts problematic and the authors risk to undermine the value of their retracker development work. For example, the authors use a surface type classification scheme that was originally designed for Arctic sea ice and an earlier version (baseline-b) of the CryoSat-2 Level-1 data. For the ESA CCI data set we had to define separate waveform parameter threshold for Arctic and Antarctic sea ice. The authors need to show that there is no preferential sampling of surfaces that may introduce a freeboard bias, which can be easily done by providing information on detection rates for lead and floe surfaces and compare those the surface type fractions of the ESA CCI dataset (Paul et al. TC 2018). I would also strongly suggest to compute freeboard per orbit and not by subtracting monthly mean elevations and sea surface height. In our experience the geophysical range corrections in the CryoSat-2 Level-1 product files are not good enough for this approach. Using the instantaneous sea surface height during the orbit will be more reliable and also yield better options for filtering incorrect retrievals.

3) Several earlier studies and datasets that are highly relevant for this topic are not mentioned and the manuscript gives the wrong impression that the work of the authors is the first application of CryoSat-2 for Antarctic sea ice. The authors also state that the impact of snow backscatter on ranging with Ku-Band frequencies is "often ignored" which I certainly do not agree with. It might be a matter of wording, since most operational CryoSat-2 products use the assumption that the freeboard is the average ice freeboard within the footprint. But this is due to the challenges of parametrizing snow backscatter and its temporal and regional variability, not the lack of awareness in the scientific literature. I have suggested references in the specific comments below. The lack of reference to existing publications that specifically deal with CryoSat-2 freeboard retrieval in the southern hemisphere (Schwegmann et al, 2016, Paul et al, 2018) is also unfortunate, as these are a good motivation for this study. There would be added value

if the authors compare their snow freeboards to the freeboard information in the ESA CCI CryoSat-2 data set (see data availability in Paul et al. TC 2018) and demonstrate the improvements and limitations of using waveform fitting.

Specific Comments:

P2L7: I guess you mean "active" in the sense of active microwave sensors respective altimeters in general? I would recommend to use the term "satellite altimeters" instead of "active platforms" throughout the document. (Typo: remove -> remote)

P2L27: Typo: "of off"

P2L28: Beaven et al. 1995 states that the snow/ice interface is the dominated backscatter source. To my knowledge Beaven itself does not imply that cold snow under laboratory conditions is completely transparent for Ku-Band. This is a fine distinction but relevant for this paper.

P2L39: Please rephrase the term "often ignored" as it does not properly reflect the state of the scientific literature. The issue is not lacking awareness of the importance of snow interface or volume backscatter for Ku-Band freeboard retrieval (e.g. Armitage and Ridout GRL 2015, Ricker et al. GRL 2015; Nandan et al. GRL 2017), it is the challenge of getting the temporal and spatial variability from the available waveforms. A better description would be "often not included freeboard retrieval algorithms, especially those depending on an empirical waveform evaluation".

P3L30: The inverse barometric correction is included in the dynamic atmosphere correction. Both corrections should not be applied in combination.

P4L18: See comment above. There are several publications that investigate the impact of snow on CryoSat-2 freeboards and are not cited here.

P7L7ff: At this point it would have been good if the authors had included a sensitivity study using their forward model. It is difficult to assess the skill of the waveform fitting if e.g. the impact surface roughness changes and snow backscatter on the waveform shape could be ambiguous.

P7L22ff: The authors should provide information on the detection rates for lead and ice surfaces. In Paul et al 2018 (TC) we needed to introduce different waveform parameter thresholds for Arctic and Antarctic sea ice. There is a risk of introducing freeboard biases if the surface type classification is not performing well (Schwegmann et al. TC 2016).

P7L30: The values for PP in Laxon et al. 2013 are: PP < 18 (not 0.18) and SSD < 4. I assume that the value of 0.18 is only a typo in the manuscript. But the issue that these thresholds are valid for an older version (algorithm baseline-b) of the CryoSat-2 Level-1 waveform. The authors must use baseline-c data for the later years of their data record and waveform oversampling introduced in this version changes pulse peakiness values. The authors should therefore verify their lead and ice detection rates (see above).

P8L8: Same PP threshold magnitude inconsistency. Laxon et al. 2013 reports PP < 9 and SSD > 4 as criteria for sea ice surface returns.

P8L33ff: Does this mean that the authors have removed the DAC and tides from the CryoSat-2 derived elevations and then applied a consistent DAC, tide and mss correction for both ATM and CryoSat-2? What about the inverse barometric correction (see comment above)? This approach however still leaves the ionospheric and tropospheric corrections as an uncertainty factor, which may a dynamic range of 10 cm or more (Ricker et al Remote Sensing, 2016) thus a non-negligible magnitude. A second validation in form of along-track freeboard might help to improve the initial validation.

P9L19ff: Filtering waveform is standard practice. The question is how many are filtered out?

P9L24f: The practice of computing freeboard by subtracting monthly means of sea surface heights and surface elevation is definitely not state-of-the-art. It puts a lot of

trust in the range and tidal corrections that to my experience is not justified. I strongly suggest to estimate freeboard orbit-wise, which also gives a better handle to identify sea surface height estimation issues.

P9L29: The description is a bit confusing, as the earlier sentences reads as snow freeboard (per grid cell) = mean elevation (per grid cell) - mean ssh (per grid cell). From this sentence I get the impression that the authors compute a mean sea surface height, remove that from the all elevations, filter anomalous elevations and then compute the snow freeboard from the remaining values. Please revise.

P9L30: The authors seem to filter negative freeboards within a grid box. I assume this are then from individual waveforms? The filter should be lower than 0 meter or else the negative part of the range noise distribution will be filtered out for thinner ice and thus cause the freeboard to be biased high. In the ESA CCI dataset, the filter range for CryoSat-2 along-track freeboard is -0.25 to 2.25 meters.

P9L34: Is the 125km filter applied to reduce noise, or to interpolate between gaps?

P10L6: The impact of surfaces waves into the ice pack also needs to be considered for CryoSat-2 data.

P10L17ff: For this comparison it would be helpful to have corresponding maps of surface type (lead/ice) detection rates as well as average resnorm values to look into these differences and verify that the sub steps of the CryoSat-2 snow freeboard algorithm are working as intended.

P10L16: Is the ICESat data also filtered to an effective resolution of 125 km?

P10L26ff: It is good that the authors looked into the other output parameters of the waveform fitting. Unfortunately in this shorted form, more question are raised than answered. In essence, the retracking is based on a backscatter model of snow that when applied to CryoSat-2 waveforms, does not result in realistic snow conditions. Again, additional information such as regional differences in surface type detection rates or waveform fit quality parameters would greatly help to identify potential issues.

P11L34: This is an overstatement given that all existing studies that used CryoSat-2 for Antarctic sea ice are not referenced in this paper.

P19Fig6: axis annotations and ticks are difficult to read on a printed version

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