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Interactive comment on "An improved CryoSat-2 sea ice freeboard and thickness retrieval algorithm through the use of waveform fitting" *by* N. T. Kurtz et al.

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The authors present a method of using a numerical physical model to fit CryoSat-2 radar waveforms and to derive freeboard and surface roughness of Arctic sea ice in March 2011, 2012 and 2013. This approach is novel compared to existing publications which used a constant threshold on the leading edge of the waveform to estimate freeboard only. The authors compare the result of their waveform fitting method and a threshold based method that is similar, but not identical, to the method of Laxon et al. with airborne validation data from Operation IceBridge flights in the same month. They find a significantly better agreement of the freeboard estimates between CryoSat-2 and

the airborne data for the waveform fitting method than the threshold retracker.

The paper discusses a very important topic: The correct interpretation of CryoSat-2 waveforms over Arctic sea ice. It has been a debate for some time whether Ku-Band SAR altimetry (airborne and satellite platforms) is able to sense the freeboard of the snow/ice interface or whether the main scattering surface is located in the snow layer or the air/snow interface for some boundary conditions. This paper offers a new view of the topic, present the methodology in creditable detail and retrieves a novel parameter, surface roughness from the CryoSat-2 data. I agree with the other that the physical interpretation of the CryoSat-2 echoes is an improvement compared to threshold methods. I would like to see this study published, but I also think the paper needs a better description and interpretation of the freeboard results. The details of my concerns are laid out below as general and specific comments.

One key assumption of the waveform fitting is that only surface scattering from the ice/snow interface has to be considered. The authors mention that volume scattering from within the snow layer may be present for certain snow conditions but the uncertainty which arises from that is not described. If volume scattering cannot be easily included in the waveform forward model, I would like to see the author's view of how the volume scattering might affect freeboard retrieval. The authors cite work of Willatt et al., 2011 to support their claim, that the dominant backscatter in Arctic sea ice is the ice freeboard, but in the work of Willatt et al., this is not explicitly stated. The paper would benefit from a more detailed discussion of this issue, since neglecting volume scattering may introduce a bias in multi-year sea ice regions with more complex snow stratigraphy.

While the waveform fitting method is described very detailed, there are some open questions how the freeboard maps were derived. I have not fully understood how the actual sea surface height was used to obtain the gridded freeboard data. There also some visual difference in the data coverage between the two freeboard retrieval methods. The authors should state how much waveforms were rejected in the waveform

fitting process.

The comparison of the CryoSat-2 and Operation IceBridge derived freeboard products could be more detailed. The mean differences between both methods show very different results, which is to some part obvious based on the methodology. But the author could support their claim much better, if they show the comparison in more detail (e.g. the histograms or scatter plots). It is also not clear to me if the comparison between satellite and airborne data is affected by smoothing of the CrySat-2 maps, with has not been mentioned for the airborne data.

The freeboard maps of both methods show some sort of dipole feature in March 2013. This looks a bit counter-intuitive to me. Could the authors comment in that? Unfortunately, there is no airborne data in this region.

As a general comment: The maps could be of better quality.

Specific Points:

P725L16: There is still the need to know the correct snow and ice density for the freeboard to thickness conversion. And from my understanding different snow and ice densities in various studies are not the product of a bias correction, but reflect how less firm the mean values for first-year and multi-year sea ice are.

P727L7 Can the uncertainty of the 2013 quicklooks be estimated by comparisons between quicklooks and final data products from earlier years?

P729 The authors use a model which does not take potential volume scattering in the snow layer into account. Is this done only for considerations of the physical snow properties or would that require a significant change the waveform model? If the latter is not the case, it would be very beneficial to show the effect of volume scattering on the waveform shape and retracking point. Internal layers and density contrasts in the snow can be found quite frequently on multi-year sea ice (personal experience) and it would be beneficial to get more reliable CryoSat-2 data in late spring were the snow

might start to get wet.

P735L17 Can the waveform fitting method identify mixed echoes from leads and ice floes? Or is the classification binary?

P739L10 Here it would be good to know how many waveforms are discarded and if there is a significant difference in the waveform rejection in first-year and multi-year sea ice regions.

P740 Bottom: I would suggest to the authors to concentrate on freeboard and roughness only. Comparison of thickness does not give any additional information, since it is directly and identically derived from the two freeboard data sources.

P741L5 The publication of Willatt et al., 2011 does not show this very well. There are abundant echoes were the main scattering peak is well above the snow-ice interface. The authors in this study argue that this findings are affected by warm snow temperatures and the peak shifted towards the snow-interface for lower temperatures (-8C instead of -4C). But whether this can be extrapolated for March conditions and radar freeboard being ice freeboard is speculation at this point.

P741L10 Sect 2 is “Data Sets”, Reference error?

P741L10 It the sea surface height correction done for individual orbits or the gridded product?

P741L15 What is the reason for the low pass filtering of the 25 km grid? Does this affect the comparison to the OIB data?

P741L21 I can see the typical freeboard pattern only in 2011 and 2012 in Fig 9. In 2013 there is some sort of a dipole pattern with higher freeboard in the Lincoln Sea and the Russian shelves. Do the authors have an idea whether this feature is real or an artefact? General comment: The figures could be of better quality (e.g. larger and the histograms of the waveform fitting and empirical threshold grouped for better comparability).

P741 Why are there data gaps visible in Fig 9 (e.g. Eastern Beaufort Sea in 2012 and 2013) but not in Fig 10? This comes back to my question how many waveforms are discarded for the waveform fitting.

P743L24 The usage of identical mean sea surface height for both freeboard estimations is definitely correct. But I may have missed that, but how is the sea surface height correction applied specifically? It is only the subtraction of a common mean sea surface height?

P744L15 Minimum thickness is a confusing term in this context. I guess the author mean the theoretical thickness of bare (snow-free) ice.

P745 General comment: It would be good to see more than the mean differences which theoretically could be right for the wrong reasons. I suggest to the authors that they support their claim by a scatter plot or histograms.

P745L3 The OIB data has been gridded to a 25 km grid, but I guess it has not been smoothed by +/-2 grid cells like the CryoSat-2 data?

P746L8 Is the uncertainty of the snow radar the main driver for these values?

P746L18 It is intriguing that the uncertainties are apparently decreasing by that amount. Can this be a sampling issue of the OIB data? From figure 2 it looks like that the sampling in first-year ice regions has increased in the three years.

P747L5ff I have not fully understood the discussion. The 2013 data shows the lowest RMS difference and a better correlation between airborne and satellite derived freeboard. How is this in line with the fact that the 2013 OIB data is “only” the quick look product?

P747L23 I think it should be noted here that these large differences of 1.11 – 1.43 m were in general not found in the Laxon et al. paper in comparisons with independent sea-ice thickness data. The conclusion sections states correctly that the ELTF is only similar to the method by Seymour and the differences have been well explained in the

previous sections, but the vicinity of the citation and the given biases may give a wrong impression on Seymours thickness results.

P748L28ff Point 5 may also be a potential bias between first-year and multi-year sea ice.

Interactive comment on The Cryosphere Discuss., 8, 721, 2014.