Reviewer 3, Stefan Hendricks

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.

We thank the reviewer (Stefan) for his comments, particularly with the attention to the description of the methods and interpretation of the results. We have now changed the statement to now quote specific numbers from the Willatt et al., (2011) study which showed that 80% of the radar returns from the 2008 CryoVEx field experiment were closer to the snow-ice interface. While for the 2006 CryoVEx field experiment only 25% of the returns were closer to the snow-ice interface. This was done to put a less generalized statement that scattering always occurs from the snow-ice interface. As has been pointed out, tore research into this area is certainly needed, and we have put in the statement that the assumption of the dominant radar return location needs to be considered on a regional and seasonal basis.

To address the concerns about surface and volume scattering from the snow layer, we have now added an additional section which estimates the error in our retrieval process. The section is mainly meant to estimate errors which will occur due to the scattering from the surface and volume of the snow pack. Three cases are presented, one where the backscatter from the ice is much higher than that of the snow (which was assumed in our retrieval of elevation), a case where the backscatter from the snow is comparable to the ice, and an intermediate case.

It is agreed that the original description of how freeboard is derived was not clearly written. Essentially, two 25 km elevation grids are created. One grid containing the floe elevations and another grid containing the lead elevations. Freeboard is calculated by subtracting the lead elevation grid from the floe elevation grid. We have now rewritten the manuscript to make this more clear. The percentage of rejected waveforms is now stated.

Histograms of the freeboard differences have now been added in order to present more detailed information than just the mean and RMS differences. With regard to smoothing of the CryoSat-2 data, this slightly improves correlations and RMS differences by reducing some of the noise in the CryoSat-2 data. Specific numbers as to how this changed the results are in the discussion below.

The dipole feature in March 2013 would indeed look counter-intuitive if the maps were of ice thickness, but it should be noted the maps are of freeboard. If the snow is thicker near the north pole region then it would depress ice freeboard as well as lead to different electromagnetic travel times. In this case a map of ice thickness would not necessarily produce such a strong dipole pattern. Another interesting point to consider is the significantly larger ice coverage at the end of the melt season in September 2013 compared to 2011 and 2012. A thicker ice cover in the Russian sector of the Arctic is consistent with the increased areal coverage of ice during this time period. Overall, we are only reporting what our retrieval algorithm produced for the data. Airborne or other data (if it exists) in this area would indeed be useful to analyze.

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.

Yes, there is indeed a need to better quantify the densities of snow and ice for freeboard to thickness conversions, and this is by far the most important point. This sentence has now been changed to remove the reference to the use of different densities as a bias correction.

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

The quicklook snow depth uncertainty can be estimated since the retrieval method is the same between the quick look and final data. For 2012, the mean difference is small at 0.05 cm, and the standard deviation of differences is 2 cm. For 2013, the mean difference is 1.6 cm and the standard deviation of differences is 2.4 cm.

For the freeboard uncertainty, there was a significant change between the quicklook freeboard retrieval method in 2012 and that in 2013 so that a comparison is not valid. Using available final data which has been processed from 2013, the uncertainty in freeboard is 0.2 +-6.9 cm. The uncertainty in ice thickness is 6.6 +- 67 cm.

The estimated differences for the 2013 campaign have now been added into the text.

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.

Surface and volume scattering can be included in the model, however for simplicity in the development of the retrieval method it was not included in the original manuscript. We have included a new section which addresses model cases demonstrating the errors in the retrieval method for different snow surface and volume backscattering cases. These are meant to be case studies, the inclusion of the model results in the retrieval of surface elevation requires more in depth study.

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

The waveform fitting method as written here assumes the radar footprint contains only one specific type of surface, thus the classification is binary.

One could potentially utilize the method to construct and fit a waveform of consisting of mixed types. The model developed here would have to be modified to account for the fact that the scattering element of each component is not at nadir. It would also require some knowledge or estimation of the backscattering properties within the footprint surface. This is certainly beyond the scope of this study.

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.

In total, about 60% of the CryoSat-2 waveforms are used for elevation retrievals during the March campaigns, this rate is largely determined by the use of the pulse peakiness and stack standard deviation requirements. For floes, about 90% of the waveforms which meet the pulse peakiness and stack standard deviation requirements are fit. This has now been added to the text. The rejection rates look to be higher in first-year ice areas due to the higher prevalence of leads which create mixed returns.

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.

The purpose of the paper is to show that having consistent freeboard retrievals from laser and radar altimeters is necessary to reconcile the sea ice thickness record from multiple data sources. The comparison with thickness provides this link, and we feel it is essential to leave it in the manuscript.

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.

The paper has been updated to clarify this point. We have changed this statement to now quote specific numbers from the Willatt et al., (2011) study which showed that 80% of the radar returns from the 2008 CryoVEx field experiment were closer to the snow-ice interface. While for the 2006 CryoVEx field experiment only 25% of the returns were closer to the snow-ice interface. This was done to put a less generalized statement. More research into this area is certainly needed.

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

The sea surface height corrections (e.g. tides, dry troposphere, inverse barometer) are described in Section 2, thus this is correct.

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

Since the sea surface height corrections are applicable at a different resolution than the gridded product, they are applied to each individual data point. This has now been put in to Section 2.

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

The smoothing of the data was done to reduce noise in the CryoSat-2 gridded data. It slightly improves the comparison to the OIB data, but not in all cases. The mean differences are largely the same. The RMS difference improves by 0.023 m, 0.041 m, and 0.028 m for 2011-2013, respectively. The correlation changes by -0.06 (a decrease), 0.15, and 0.15 for 2011-2013, respectively.

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).

The dipole feature in March 2013 may be due to the fact that the maps are of freeboard, not ice thickness. If the snow is thicker near the north pole region then it would depress ice freeboard as well as lead to different electromagnetic travel times. In this case a map of ice thickness would not necessarily produce such a strong dipole pattern. Overall, we are only reporting what our retrieval algorithm produced for the data. Airborne or other data in this area would indeed be useful to analyze to determine whether this is an artifact in the data.

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.

The data gaps in Figure 9 are due to the requirement that each gridded data point contain at least five floe and five lead measurements. In most cases for the central Arctic, the gaps are due to the lack of lead measurements. The maps in Figure 10 are of surface roughness where only sea ice floe measurements are used.

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?

The sea surface height corrections are now better described in Section 2. As an initial step, to each individual CryoSat-2 data point we apply the elevation corrections found in the CryoSat-2 data file (e.g. Tides, inverse barometer, dry troposphere). We also apply a correction to the elevation for the EGM08 geoid.

For the ELTF method, we then gridded the sea surface height elevations which were retracked using the empirical fit function described in Giles et al., (2007).

For the waveform fitting method, we then gridded the sea surface height elevations which were retracked using the method described in the paper.

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

Yes, this is what is meant. It has been changed in the text.

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.

Histograms of the freeboard differences have now been added.

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?

No, it has not been smoothed in this way. The reason for this is that the OIB data represent a small flight track through the data grid, smoothing by +-2 grid cells will usually only incorporate two nearby grid points.

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

It is variable, in areas with many leads the freeboard uncertainty will be low and thus the uncertainty in the snow radar will be the dominant driver.

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.

This could indeed be a sampling issue as there was definitely a different proportion of first year and multi-year ice sampled in the three different campaigns. This has now been added to the text.

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 free-board. How is this in line with the fact that the 2013 OIB data is "only" the quick look product?

Preliminary estimates of the 2013 OIB quick look data show that the uncertainties in the data are not largely different than the final product (see earlier comment) so this is likely not the main factor. It could be a sampling issue which was brought up in the previous comment, this has now been added in. It could also be an ice motion issue as we have not accounted for that in our analysis. At this time, we do not have a good explanation for the significantly better agreement with the 2013 data, we are only reporting the results of the analysis.

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.

It has now been added that the large thickness differences shown here are seen when consistent estimates of snow depth and sea ice density are used. We note that the thickness values reported by Laxon et al., (2013) are in much better agreement when different parameterizations are used.

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

This is true, but in terms of an electromagnetic scattering model it is best to mention factors which are parameterized in the model.