Reviewer 1, John Paden

General overview

This paper demonstrates the use of a waveform fitting method for Arctic sea ice satellite radar altimeter data. The work focuses on using the echo shape to determine the mean surface height in the waveform which is shown to vary with surface roughness and correlation length for the model that is assumed. This is a very important step in sea ice radar altimetrysince I am not aware of any work that compares threshold and waveform fitting retrackers for sea ice floes as has been done for land ice. Although there are several assumptions as well as some improvements that can be made, the work will be a solid contribution once a couple issues are resolved

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The two biggest issues with the paper are:

1.Model selection justification especially regarding backscatter in incomplete

2.Discussion of statistics between CR2 and OIB is confusing and poor correlation needs to be explained better.

We thank the reviewer (John) for his insightful comments, particularly with regard to the scattering model which was used and details regarding the radar data which were used. Towards that end, there was indeed an error in the development of the model with regard to the equation relating backscatter dependence on incidence angle (equation 7 in the submitted manuscript). This does not affect the final results. But the model derivation regarding the backscatter has been fixed by usage of the results of Hagfors (1970) which gives the necessary angular dependence of backscatter instead of the power formula which was used in the original version. The conditions for the model validity (found in Ulaby et al., 1986) have also been updated to provide justification that the model assumptions are valid.

We have also updated the discussion on the comparison between the OIB and CryoSat-2 data sets. A more clear description of how the statistics were derived (using gridded data) is now included (this was also requested by other reviewers). Further discussion of the correlations which were found between OIB and CryoSat-2 data sets is also included. As pointed out by another reviewer, part of the reason for better correlation of the data sets in 2013 may be due to the higher sampling of first year ice areas, which is an interesting avenue to explore for future studies.

I think "cross product" should be dropped everywhere in the paper since it refers to the InSAR product (left * conj(right)) and I think that is not what you are using. Even with the InSAR mode data, it sounds like you are just using the mean echo power product.

This is correct, the term “cross product” has been dropped from the text and replaced with mean echo power.

The range resolution for Cryosat-2 is approximately c / (2*BW) where c is the speed of light and BW is the 320 MHz bandwidth. Therefore the range resolution is 0.468 m. The range sampling is 0.234 m (or twice the range resolution) to preserve the Fourier domain after the power envelope is taken of the signal. This allows sin(x)/x interpolation to be applied if you wish to achieve a higher sampling rate, but does not imply that the range resolution is 0.234 m.

The paper should be modified to quote range resolution as 0.468 m. If it would be beneficial to the algorithm applied by the authors to have finer sampling, then regular sinc interpolation (e.g. by zero padding in the Frequency domain) would work

The paper has been modified to quote that the range resolution in vacuo is indeed 0.469 m, a reference to the paper by Jensen, 1999 which describes this effect has now been put in. The word 'effective' has also been removed in the text in reference to the sampling resolution, this is now referred to as simply the sampling resolution.

“The use of Eq. (1) assumes that only surface scattering from the snow-ice interface is present (i.e. the surface is assumed to be perfectly conducting), surface scattering from the snow–air interface and volume scattering from within the snow and ice layers are neglected.”

Looking at snow and Ku-band radar data from Operation IceBridge, this assumption does seem to be violated a lot at 1500 ft altitude. I note this because the two interfaces in Ku band radar data are often of equal strength and the snow-ice interface does not dominate to the point where the snow-air could be neglected. Aqsa Patel has a paper in review (accepted with major revisions) on this.

We have now included a new section which estimates the expected error in our retrieval process when this assumption is violated. We include the case where the surface scattering from the snow and ice layers are near equal strength.

1. I think the definition of d_0 should be moved up to where it is introduced. Right now it is split into these two sentences.

2. What does the _0 mean and why not just use d(x) like Wingham?

3. It may be appropriate to replace FFT with DFT since FFT is an algorithm and DFT is the function. In either case, the acronym should probably be defined. I recommend the following changes to these two sentences: “...through the addition of a synthetic gain term, where d0(cos(zeta) – sin(zeta_k)) is the synthetic beam gain, which is a function of the angle between the direction of a scattering element and the satellite velocity vector, zeta, and the look angle of synthetic beam k from nadir, zeta_k.” ▭
“... through the addition of a synthetic beam gain term. We define this term as d(cos(zeta) – sin(zeta_k)), where d() is a function defined by the discrete Fourier transform of a Hamming window which is the window used when stacking the data. d() is a function of the angle between the direction of a scattering element and the satellite velocity vector, zeta, and the look angle of synthetic beam k from nadir, zeta_k.”

“Similarly, d_0 is the FFT of a Hamming window which is used in the formation of the mean echo cross product and D_0 is the FFT gain of the synthetic aperture minus the Hamming window loss.”

▭

Omit sentence and omit use of D_0 from equation (5). D_0 could be assumed to be included in d_0 so I don't think it is necessary to include.

The sentences have been changed largely as suggested. However, instead of the statement "we define this term as...", we state "The synthetic beam gain which is used in the processor which constructs the Level 1B waveforms is defined as...". The revised version reads much more clearly.

Regarding the discussion on the waveform model selection and backscatter:
I believe that you are using the Physical Optics (Kirchhoff approximation) model for large RMS height deviations. I believe that use of "specular" on page 731, line 18 ("within the specular scattering regime") is misleading since specular implies coherent scattering and this model is completely incoherent. I think saying quasi-specular or locally specular would be better.

The three assumptions of this model should be spelled out [Ulaby, Moore, and Fung, Microwave Remote Sensing Volume 2, 1986 does this clearly... Hagfors paper is not as concise in detailing these and only "assumption 2 below" is mathematically described by him when he states that the "phase modulation is taken to be deep"]:

1. Surface features are larger than a wavelength so that ...

2. Surface RMS height is large with respect to wavelength...

3. Radius of curvature for the surface is large with respect to wavelength...

Can you show that these assumptions are valid? One paper that discusses surface roughness:

Maria Belmonte Rivas, James A. Maslanik, John G. Sonntag, and Penina Axelrad, Sea Ice Roughness From Airborne LIDAR Profiles, IEEE TRANSACTIONS ON GEOSCIENCE AND REMOTE SENSING, VOL. 44, NO. 11, NOVEMBER 2006

The Helmholtz-Kirchoff diffraction formula is indeed used in the development of the scattering model, and thus the more explicitly spelled out conditions of Ulaby et al., 1986 are applicable. The assumptions and reference have been added to the text. The Rivas et al., 2006 paper provides the necessary information to show that the assumptions are generally valid over sea ice, this has also been added to the text. The term "specular" has also been removed and the discussion is stated to be for a smoothly undulating surface to be consistent with Hagfors, 1964.

Also, have you considered using DMS and ATM photogrammetry products to help choose a surface roughness model? Why is an exponential correlation function assumed rather than a Gaussian correlation function?

Since the dominant backscattering surface is assumed to be from the ice surface, one would need to use more than just the DMS and ATM data sets. The combined high resolution data from the ATM and snow/Ku band radar data on IceBridge could be used to provide information to guide the use of different surface roughness and correlation models with some effort. In this study, we have opted to state where simple assumptions have been made to attain a reasonably tractable solution, these are definitely research areas that could be used to improve the retrievals in future studies.

I don't like the way the scattering is made to look like a reflection coefficient which is then renamed to a backscatter component. I think you should just introduce the backscatter term and avoid the confusion caused by discussing a reflection coefficient (since this implies coherent scattering).

This portion was incorrect as it was originally written. The relation between scattering incidence angle and backscatter is provided in Hagfors (1970) which is now used in the text. In this way, the model is kept physically consistent by keeping the development strictly in terms of the backscatter rather than a reflection coefficient.

Each element of the summation in equation (12) is more than one since max(Pr) >= Pr(i). Therefore this threshold or equation (12) is incorrect. Also equation (12) is not actually given in Laxon (2013) and appears to be given most recently in Peacock and Laxon (2004) and that equation does not quite match the one given here either. The PP threshold is given as 0.18 (here), 1.8 (2004 ref), and 18 (2013 ref)... Perhaps these inconsistencies could be addressed in the paper in addition to the equation/threshold being corrected.

There was indeed a typo in equation 12 as written, it has been fixed. It is also correct that the equation for pulse peakiness was not defined in Laxon et al., 2013. The equation in this paper is actually from Armitage and Davidson, 2014, and this has also been corrected in the text. Defining the pulse peakiness in this way allows for the off-nadir ranging bias to be estimated in a consistent manner with the results of Armitage and Davidson, 2014.

It is not clear how the EGM08 geoid is used. Are you suggesting that the lead tracker in ELTF is not used? Perhaps the use of EGM08 could be explained better?

The use of EGM08 was not clearly explained in the original manuscript. We first bilinearly interpolate the EGM08 geoid and subtract it from each elevation measurement. This statement has been added to Section 2 where the description of the geophysical corrections occurs.

For the description on Page 743, this is stated again that in the ELTF method the EGM08 geoid is used, whereas Laxon et al., 2013 used a mean sea surface height built from a year of CryoSat-2 observations of ocean elevation.

The values for correlation given in Table 2 seemed very low (e.g. 0.02 in 2011). I think the standard deviation of the correlation coefficients should be given in any case.

Equation (18) does not seem to have much physical basis and either needs a better explanation for why it is justified or it should be dropped.

Laxon 2013 reports fairly high correlation coefficients and I think this should be discussed (especially since this paper also givens a 2011 CR2 and OIB comparison).

Discussion on Monte Carlo simulation can probably be changed to an analytical discussion which discusses the accuracy of the uncertainties. In other words, if you know your uncertainties, then you can precisely estimate the correlation coefficient assuming the uncertainties are independent.

The values of the correlation coefficients in Table 2 are indeed low, and should be further investigated but we feel this is beyond the scope of the present study. The difference between the correlations presented in Table 2 and those of Laxon et al., 2013 is now discussed in the manuscript. The reason for the large difference is not clear at this time, but we wish to state that the ELTF method is not an exact reproduction of the data set of Laxon et al., (2013), also the correlations in Laxon et al., (2013) were for ice thickness not ice freeboard.

Equation 18 follows from standard error propagation which assumes the uncertainties between the IceBridge and CryoSat-2 data sets are uncorrelated and thus the observed standard deviation of differences between the two measurements is due to the combined uncertainty of the individual components. That is, sigma_diff^2 = sigma_IceBridge^2 + sigma_cs2-fb^2. This has now been described in the text.

The discussion on the Monte Carlo simulations indeed shows that if the uncertainties are known, then the correlation coefficient can be accurately estimated. We have added in the sentence that the discrepancy between the Monte Carlo results and the computed correlations can also be due to the fact that the IceBridge and/or CryoSat-2 freeboard uncertainties are underestimated.

Technical corrections

All technical corrections have been amended as suggested.