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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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This is an interesting and important paper describing a new retracking method for CryoSat radar altimeter sea ice freeboard retrievals, and comparing derived Arctic-wide freeboard estimates with results from previously used retrackers. The paper then attempts to convert the freeboard estimates into ice thickness estimates using consistent assumptions about snow and ice density and snow thickness. Comparisons with freeboard and thickness estimates from airborne laser altimetry show that on average retrievals with the new method agree much better with the laser altimeter results than ice thickness retrievals with the old method.

The new retracking method is called waveform fitting and is based on least-squares fitting of model curves to actual waveform measurements. The model is physically based and takes roughness and backscattering properties of the ice surface as well as radar antenna characteristics into account, including the novel synthetic aperture radar altimeter concept which CryoSat employs. A sensitivity study is presented showing how ice roughness and incident-angle dependent backscatter affect the waveform width and peak delay time. The old method is the well-established threshold method which has been empirically optimized and applies different thresholds for ice and water. The waveform method yields consistently lower freeboard estimates than the threshold method which can be explained with the results of the waveform model sensitivity study. As a result, waveform-model derived freeboard estimates are much smaller than threshold-method derived ones. Good agreement between both methods and the same laser altimeter data can only be achieved if different assumptions about snow thickness and snow and ice density are made.

The authors did a great job describing the nature of CryoSat waveforms and the resultant model and retracking procedure. However, I am not able to verify the detailed correctness of the model. It may also be worth to even more extensively describe the consequences of the applied simplifications and assumptions, e.g. if generally level ice was assumed, or what the consequences of mixed ice/lead surfaces are. The comparison with the threshold-derived freeboard estimates is solid and identified differences between the methods and their results are well explained. However, comparison of results with airborne laser altimetry is questionable particularly due to assumptions about the accuracy of snow thickness retrievals from airborne snow radar measurements and the assumption that CryoSat radar returns originate from the snow/ice interface only. With the expected uncertainties resulting from these two error sources the good (average?) agreement between waveform-retracking and laser altimetry is almost unbelievable. In fact, the discussion of poorer correlation coefficients in the end of the manuscript raises some questions about what conclusions can be drawn from the good average agreement.

Although I recommend to publish the paper with minor revisions, I am very concerned about the quantitative comparisons of thickness results. I would strongly encourage the authors to focus on freeboard retrievals only, and to omit any quantitative discussion of ice thickness results. It is clear that larger freeboard resulting from one data set would result in larger thicknesses if the same assumptions would be made otherwise. I think it would be sufficient to leave it at that, although the author's attempt to point out the importance of using the same assumptions about snow and ice properties is of course commendable. It is also clear that clever assumptions of ice and snow densities and thickness can be made to yield better agreement between freeboard and thickness retrievals from different data sources. But it is somewhat a different topic and problem than the description of the new method. And many other authors have published sensitivity studies of the importance of various snow and ice properties on freeboard-to-thickness conversions.

Several other aspects should be clarified as outlined below.

Specific comments Title and abstract: Please consider to remove "thickness" from the title and abstract. The abstract should be rewritten to be clearer on the used or developed methods and to better represent the results. E.g. there is confusion about empirical versus numerical model, although this becomes clear in the text, where additionally physical model is used. I don't think the comments about consistent assumptions in the abstract are easily comprehensible for non-experts. And I could not find any reference in the text to the exact values of differences between CryoSat and OIB stated in the abstract? How were the values in the abstract obtained?

P 725 top: this discussion is important but could be kept much more general, as the individual values aren't supported by references and show relatively high variability in nature. Many other authors have already presented sensibility studies in this regard.

P 725 L13 & 21: Clearly define and consistently use freeboard or surface elevation, or snow freeboard or ice freeboard etc. . . from the outset.

P 726 top and L 8: briefly explain SAR, and say if stated footprint is for pulse-limited radar? Similarly, the discussion of SARIn seems to complicate the flow of the paper. Why not ignore SARIn here and just discuss it in the discussion or conclusions?

P726 bottom: What are the errors of these corrections, or do they just not matter because the same corrections are applied to the data before the different trackers are applied?

P727 middle: Why use 2013 quick look data if they are less accurate? Why not focus on 2011 and 2012 only? And why is the comparison of 2013 quick look data with CryoSat retrievals not worse than 2011 and 2012?

General comment: It is clear that the OIB data are well referenced to SSH and that accuracies depend on e.g. number of SSH tie points. However, what did you actually do with the CryoSat retrievals? I did not find a clear description about how SSH was reconstructed from the lead waveforms? What is the spacing of tie points in the CryoSat data?

P. 728 top: This model is a good first step towards better understanding and application of CryoSat waveforms. However, it is clear from the outset that the sea ice surface is not Gaussian (hence, e.g. the possibility to use open water tie points). Can you add a sentence at the outset that this is so (e.g. use a Wadhams reference). Of course you could well examine surface roughness and autocorrelation with the ATM data and use those results in future improvements of the model. What else is known about the shape of surface elevation autocorrelation functions?

P. 729, L5: conducting is not a good word here; it is rather the dielectric properties. Why not say more generally that the ice is assumed to be opaque?

P729, L25: is sigma the rms roughness, more explicitly?

P729, L27: I think it is known that the form is rather log-normal or exponential; but you have to start with the most simple assumptions.

P 732 bottom and 733 top: This is confusing. Does the discussion so far assume rather level ice? (where the Gaussian distribution may be more realistic?). There will almost always be ridges within the footprint, except maybe on fast ice e.g. in McMurdo Sound.

P. 734, L11: does the model only consider purely lead or ice surfaces within the footprint? Does this mean that only the largest leads could be identified?

P 734 L18-22: how does this differ between pulse-limited and SAR waveforms?

Section 3.2.1, 3.2.2, and following: The use of terms Sea Ice Leads and Sea Ice Floes sounds kind of wrong. A floe is something rather confined and limited although they can be vast. I suggest to consistently use the terms Leads or Ice Cover or Sea Ice Cover or Ice Surface?

P735 L 15: unclear: all amplitudes seem to be equal in Figure 3

P737 Fitting routine: What do you do with waveforms which possess multiple peaks, or where the highest peak is not the first peak, respectively?

P739 L15 ff: Is this based on the fact that sea ice returns are wider than lead returns?

P740 L8-22: More generally? What is the rejection rate of waveforms that cannot be fitted? Later on you mention that there may only be very few points in each grid cell. What is the average number, or how does it vary?

P741 L5 ff: I would argue that Willat et al show that penetration is actually quite variable and that complete snow penetration cannot be generally assumed. This is a key point for all following discussions in the manuscript, and therefore I would suggest to not go into too much detail. More research needed. . . lets go CryoVEx. . .

P741 L9ff: This discussion is very short and I would be afraid that these steps introduce high uncertainties. Is your assumption that they affect waveform and threshold retrievals equally? I think this might be a fair assumption but should be stated?

P743 L20 ff: The discussion of different thresholds for leads and ice, and the usage

of certain geoid models creates significant uncertainties. I think it is important to point these out, but Section 5.2.1. becomes quite confusing. Can these issues be explained a little better? What geoid model was used in the present study?

P744 L1-2: Important sentence, state earlier on!

P744 L20-L23: This is also very important and should be stated at the outset of the paragraph. Does the paragraph first assume that no snow is present at all?

P745 L5: does A valid measurement mean ONE valid measurements? As mentioned above, it would be good to know more about the rejection rates of invalid versus valid measurements.

P746 L8: In what context is SSH mentioned here? How is SSH determined for CryoSat (see above)?

P746 L19 ff? Correlation: What has been correlated with what, and on what scales? Is this now a point-to-point comparison along the same tracks? Or grid point by grid point?

P748 L5: How does 0.57 relate to the values stated on P747 L5?

P749 L8-11: But there will still be uncertainty from dynamic SSH variations?

Technical comments P723 L 5: impact ON climate P723 L23: clarify limited regional data P724 L24: FROM the use. . . P 728 L 14: add ERS P741 L1: What do you mean by frequency range? P742 L7: BE added. . .

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