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 altimetry since 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. 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

The conclusions of this work are:

1. That the 50% retracking method for sea ice floes performs worse than waveform fitting for absolute height estimates because the tracking point varies due to surface roughness and correlation length.
2. Waveform fitting can be used to recover surface roughness and correlation length.
3. A positive comparison between satellite radar and airborne lidar measurements suggest that these two records can be better reconciled with this new method than with the previous 50% retracker method. This conclusion is problematic with the results that were given. Specifically, the low correlation value of 0.02 for 2011 reported in this paper seems to be in conflict with the 0.608 value reported by Laxon 2013 using the threshold retracker.

1. Does the paper address relevant scientific questions within the scope of TC?
   Yes.
2. Does the paper present novel concepts, ideas, tools, or data?
   Yes.
3. Are substantial conclusions reached?
   Yes.
4. Are the scientific methods and assumptions valid and clearly outlined?
   Yes, except a couple points outlined in the review.
5. Are the results sufficient to support the interpretations and conclusions?
   Yes, except a couple points outlined in the review.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
   Yes, except use of EGM08 and peakiness factor are not clear.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
   Yes.
8. Does the title clearly reflect the contents of the paper?
   Yes.
9. Does the abstract provide a concise and complete summary?
   Yes.
10. Is the overall presentation well structured and clear?
    Yes.
11. Is the language fluent and precise?
    Yes.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
    Yes with several minor corrections and suggestions in the review below.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
    No, other than what is detailed in the review.
14. Are the number and quality of references appropriate?
    Yes.
15. Is the amount and quality of supplementary material appropriate?
    NA.

Page 722 line 2, Page 728 line 11, Page 731 lines 8-9:

"We develop an empirical model capable of simulating the mean echo power **cross product** of CryoSat-2 SAR and SARIn mode waveforms…"

"Here we provide the theoretical basis for modeling the mean echo power **cross product** from CryoSat-2 SAR…"

"Similarly, d0 is the FFT of a Hamming window which is used in the formation of the mean echo **cross product** and…"

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.

Page 726 line 10:

"The power detected echoes contain 128 range bins in SAR mode and 10 512 range bins in SARIn mode, each range bin is sampled at **1.563 ns** (0.234m range resolution in vacuo)."

Would there be some benefit in having an over sampled data product (i.e. sin(x)/x interpolation to get finer sampling)? This sin(x)/x interpolation should be done on voltages and therefore before any kind of incoherent detection. This would require a change or addition to the Cryosat-2 data products I think. If you agree that this would be worthwhile, is this something that could be commented on in the paper?

Page 729 line 5-6:

"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.

Aqsa Patel, John Paden, Carl Leuschen, Ron Kwok, Daniel Gomez-Garcia, Ben Panzer, Malcolm W. J. Davidson and Sivaprasad Gogineni, "Fine-Resolution Radar Altimeter Measurements on Land and Sea Ice," submitted to IEEE Transactions on Geoscience and Remote Sensing on Dec 3, 2013.

Aqsa Patel (aqsa@ku.edu)

Page 730 lines 2 and Page 731 line 8:

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.

Page 731 line 18 through Page 733 line 3:

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 $l > \lambda$ where l is the correlation length and λ is the wavelength in air.
2. Surface RMS height is large with respect to wavelength, $2kh > 10$ where $k = 2\pi/\lambda$ is the wavenumber in air and h is the surface RMS height.
3. Radius of curvature for the surface is large with respect to wavelength $l^2/2h\sqrt{\pi/6} > \lambda$.

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

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?

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).

Page 738 lines 8:

"PP > 0.1"

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.

Page 743 lines 26

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?

Page 746-747:

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.

Page 731 lines 15 (Technical Correction):

Start new sentence

"with incidence angle, this will be shown"

→

"with incidence **angle. This** will be shown"

Page 731 lines 20 (Technical Correction):

Omit comma

"Hagfors, (1964)"

→

"Hagfors (1964)"

Page 731 lines 21 (Technical Correction):

Insert comma

"height features the theoretical"

→

"height features**,** the theoretical"

Page 734 lines 1 (Technical Correction):

"elliptical antenna pattern, it is taken from Wingham and Wallis"

→

"elliptical antenna pattern **which is** taken from Wingham and Wallis"

Page 738 lines 23 (Technical Correction):

Start a new sentence

"off-nadir look angles i.e. it is determined"

→

"off-nadir look angles**. In other words**, it is determined" or "off-nadir look angles**. I.e.**, it is determined"

Page 739 lines 25 (Technical Correction):

Remove parenthesis in “8a)” since it is redundant

“(Fig. 8a) where sigma = 0.05 m)”

→

“(Fig. 8a where sigma = 0.05 m)”

Page 744 lines 6 (Technical Correction):

Omit second “difference” on this line.