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Interactive comment on "Review of Radar Altimetry Techniques over the Arctic Ocean: Recent Progress and Future Opportunities for Sea Level and Sea Ice Research" *by* Graham D. Quartly et al.

Anonymous Referee #2

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This paper presents a broad review of radar altimetry for the Arctic Ocean. While I thought many of the topics were relevant and needed for a review paper, I felt that in general the paper does not demonstrate or present a sufficient depth of understanding of the subject as there are many poorly worded statements and factual errors present throughout. Furthermore, I felt that the manuscript presented an overt subjective bias towards the authors' works rather than presenting a more objective review of the subject in general. Specific comments on these aspects are noted below.

I also agree with much of what was stated in the previous review and would like to see

a significant revision addressing those points as well as the ones raised here prior to publication.

P2 L9-11: This statement about the current year warming of Greenland seems a bit out of context in a review paper like this.

P2 L13: State that this is an expectation of a summer ice-free Arctic Ocean, not year-round.

Figure 1b) Check that the grey circle area is correct, I think the SSM/I pole hole is much smaller than this.

P4 L3: The sampling resolution is different than the footprint size, it would be useful to distinguish that here.

P4 L17 and throughout: I think LRM may have been a term adopted more specifically for CryoSat-2, I would say "pulse limited" here instead.

Table 1: The antenna pattern of CryoSat-2 is non-circular, not the antenna itself.

P5 L2: I'm not sure what is meant by "impulse-like shape". By definition the impulse response of an altimeter takes into account geometric spreading, so I don't think Figure 3 shows this aspect. The SAR processed waveforms do indeed have a much faster decay time than returns from only pulse-limited systems though.

P6 L6: It's not just a smooth curve that is fitted to the return, as described in the cited references it is actually a mathematical function which takes into account the instrument impulse response, point target response, and surface roughness.

P6 L10: I think the use of reflectivity here is incorrect. Do you mean the reflection from the Fresnel coefficient? That is not what leads to the range of different sigma_0 values, rather it is the wide variations in surface roughness.

P6 L10-14: This effectively ignores why the returns from leads are confined to a small number of range bins. It also needs to be made clear that the leads represent a small spatial area which is what leads to the return being much more like that from the point target response.

Figure 4 b): I'm not sure this is a fair comparison between Envisat and AltiKA since the y-axis is in units of instrument counts, unless the conversion between instrument counts and power is the same between both satellites.

P8 L17-18: I don't think it is true that the actual thresholds are unimportant. For example, Armitage and Davidson, 2014 found that the value of pulse peakiness used had a relationship with the bias in retrieved sea surface height.

Figure 6: What is Brownian + pic?

P10 L16: I don't believe the Doppler shift of the echoes are explicitly recorded, but are rather used within the processing loop itself. See Wingham et al., 2006.

P11 L8-9: I don't think this statement is true. Since even small leads have a much higher backscatter than the surrounding ice floes they tend to dominate the echoes from both SAR and traditional pulse limited systems. However, the SAR processing limits the contribution from off-nadir leads in the along track direction.

Figure 7: I'm unsure what this figure is showing. The method used for the threshold on the stack peakiness is unclear as no threshold is specified. Additionally it lacks context, how does it compare to previously published methods?

P12 L5: I think more importantly it is distinguishing between returns from melt ponds and returns from leads, not just recognizing melt ponds.

P 13 L6-7: I'm not sure what is meant here "...have been manually edited for the effects of land and clouds, with no automation of the processing."

P13 second paragraph: I think this paragraph lacks a good description of what is important for radar altimetry discrimination of surface types.

P13 L32-33: Any smooth surface can produce a low backscatter value for a SAR in-

strument, the surface could simply be very smooth first year ice and is not necessarily a lead.

Section 2.3.2: This section focuses much on validation of surface classifications of a few select algorithms, but I am quite surprised it lacks comparison with that used by Laxon et al in various studies as these are widely employed in the standard ESA processing for CryoSat-2 and have a long history of use going back to ERS and Envisat.

P16 L10: I think the written out acronym here is wrong.

P16 L13-14: These studies did not use the imagery data in a comparative sense as stated here.

P16 L18-25: This paragraph is poorly written and carries a negative tone towards the cited studies. The King reference is lacking a year, and I don't think the Connor et al., 2009 citation accurately represents what was shown in the study.

P16 L31-32: This is a poorly written sentence.

P17 L9-11: This statement is not true, the returns from sea ice are quite a bit different than open ocean returns most prominently they tend to have a much shorter trailing edge, but this is not due to snow volume scattering.

P17 L17-19: The Giles et al., 2007 paper did not use an OCOG retracker. I think in general this section is missing much of the historical work done by Seymour Laxon in retracking of floes.

P18 L1-7: This section clearly misrepresents the historical work done towards development of CryoSat-2 retracking. The widely cited Laxon et al., 2013 study utilized a threshold tracker for sea ice floes and did not use an OCOG tracker. This threshold tracker superseded the TFMRA tracker which is quite similar on a conceptual level.

P18 L 9-11: I'm not sure what this has to do with how snow impacts the results?

P18 L13-14: I think it is quite a subjective statement on the author's part that TFMRA is the most widely used tracker for SAR data and should be removed. I think the statement also does a disservice to the historical development of sea ice retrackers in general by framing this in such a way, and conveys a bias towards the authors' own work.

Section 3.1.3: I find this section to be poorly written. The second paragraph inappropriately mixes in a discussion of models for ice and open ocean returns even though it has been quite clearly shown in prior studies that the models for open ocean returns do not work for sea ice regions. The section also ignores what physical modeling of ice returns have shown with describing variations in the radar returns as elaborated on in previous sections.

P18 L30: The Rivas et al., 2006 paper did not compare sea ice and open ocean roughness so this sentence needs some revision.

P19 L9-10: How is the waveform "severely undersampled" for sea ice leads? The range resolution sampling is set by the bandwidth of the altimeter. So it would seem to me more appropriate to discuss this in the context of the need for high accuracy or precision in the data which is limited by the bandwidth of the satellite altimeters.

P19 L11-15: Here I think it is missed that the work of Laxon et al., 2013 used the tracker from Giles et al., 2007 and this is used in the CryoSat-2 sea ice product. Again, this paragraph conveys a bias towards the authors' own work.

P19 L20-26: This paragraph ignores the fact that the range resolution is determined by the received bandwidth, zero padding of the waveform does not actually confer a real increase to the range resolution, and associating this to the calibration functions used to obtain mm level path delays is highly inappropriate. Zero padding can be useful in certain algorithm approaches or in visualizing the data, but this needs to be more clearly written.

P19 L29-30: Here I think the authors miss an important need for zero padding of SAR

waveforms as demonstrated by the cited Jenson, 1999 study.

Section 3.3: The cited study by Dinardo et al., 2017 did not demonstrate retrievals over sea ice or leads, but was focused on the coasts of German Bight and West Baltic Sea. So it seems not credible to put the claim in this review paper that the methodology was successfully demonstrated to retrieve sea ice freeboard from CryoSat-2 SAR data.

I think here the authors also miss discussing the Kurtz et al., 2014 study which showed a unified physical tracker for both sea ice leads and floes from SAR waveforms.

Again, this seems to show clear biases towards the authors' own work.

Section 3.4.1: Here the authors miss the opportunity to more thoroughly describe the capability of CryoSat-2 to determine phase and thus estimate the range bias present over sea ice as shown in Armitage and Davidson.

P23 L11: This sentence is not accurate: "This is due to the along-track beam-limited resolution, which also reduces the backscatter from across-track points." The SAR processing does not impact the backscatter from across-track points in this way.

P25 L4: The western Greenland Sea is quite often ice-covered.

Section 4.1.1: Here it would be useful to state the magnitude and variability of the corrections.

P29 L8-9: Not all studies use a 50% reduction for snow climatology, the Kwok and Cunningham study used a different value.

Section 4.2.2: I think here the important factor of snow depth and density in leading to a lower propagation speed of light in the snow is missed.

P32 L18, P34 L4-5: From the text it seems unclear what is measured by Operation IceBridge, in my reading it seems to imply only laser altimeter freeboard. But it should be noted that snow depth measurements from radar altimetry are also used to determine snow.

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