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Interactive comment on "Comparison of CryoSat-2 and ENVISAT freeboard height over Arctic sea ice: Toward an improved Envisat freeboard height retrieval" *by* Kevin Guerreiro et al.

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The paper presents a new record of sea ice freeboard and thickness from CS2 and Envisat. A new method for levelling Envisat freeboard against CS2 based on PP is presented and represents an interesting and novel approach that could be very valuable for constructing longer time series' of freeboard/thickness. The authors evaluate their data against ice draft from the BGEP moorings and find good agreement, which lends credibility to their processing methodology. In particular, the agreement seen between Envisat and the moorings prior to the CS2 period is encouraging.

The paper is in general well-presented and the subject matter is certainly relevant for publication in The Cryosphere, however I believe some major revisions are required.

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My concerns are broadly in line with the previous two reviewers; I believe that the authors' treatment of the Envisat data is not up to the standard of the current state of the art for radar altimeter sea ice processing. Whilst I have some strong criticisms of the methodology/interpretation, I have tried to provide a comprehensive review as I believe this paper deserves publication.

Major comments

1. My major concern with this paper is the interpretation that the difference between the Envisat and CS2 freeboard is due to a "dissimilar impact of ice roughness and snow volume scattering" (in the abstract, and throughout the manuscript). I prefer the interpretation that the difference (presented in figure 2a&b) is caused by the high sensitivity of the pulse-limited Envisat data to off nadir ranging as a result of the footprint size compared to CS2. Figure 3 shows that the high PP and highly biased Envisat freeboard is in areas where we might expect higher lead fractions, and that the PP is particularly high in November when there is rapid ice formation and open water areas. The assertion that the lower PP areas correspond to areas of MYI is not backed up by Figure 3b at all, in fact it shows high PP corresponding to the MIZ and polynya areas. In my opinion, the highly negative freeboard shown in Figure 2b (which cannot be published as is) is a direct result of the fact that the authors make use of waveforms with intermediate PP values. These waveforms will be highly contaminated by off nadir scattering, which causes the low sea ice elevation estimates, and hence negative freeboard when differenced with the local sea level. The authors need to improve their treatment of the Envisat data before it can be considered 'state of the art' and is suitable for publication. (See my specific comments below).

2. Related to this is the waveform interpretation. The authors assert that waveforms with intermediate PP values originate from thin level ice, however these waveforms are conventionally interpreted as showing 'mixed' scattering behavior. The 'conventional' interpretation is backed up by publications which compare altimeter returns with coincident imagery [e.g., Peacock & Laxon (2004), Armitage & Davison (2014)]. As

well as this, it is known that sea ice is rarely homogeneous at the scale of altimeter footprints (even SAR footprints), so you would almost always expect mixed scattering behavior to be present in echoes over sea ice. I believe that the waveforms presented in Figure 4 also show mixed scattering behavior – they all have a diffuse scattering component corresponding to the sea ice, and each one has a specular part superimposed on top, presumably corresponding to leads or thin, freshly formed ice. You should plot the absolute power of the waveforms – is the diffuse scattering part of the waveforms remaining at a similar level, with different amount of specular scattering? I would require much more convincing, including detailed comparison with imagery, and possibly scatterometry (to show roughness), to be convinced by the interpretation that the intermediate waveforms correspond to thin, level ice.

3. The reference to "ice surface diffusion" and "surface diffusion variability" throughout the manuscript is confusing, and I do not know what the authors are actually referring to. I don't think I have come across this terminology in any other publications on satellite altimetry. You need to clarify, or adopt more conventional terminology. In some parts, it seems that you are implying that the different footprint shape/size changes the surface/volume scattering components of the ice (e.g., page 3, line 14-16). As far as I am aware, the surface/volume scattering depends on the frequency, the angle of incidence, and surface properties like grain size and water/salt content. I don't see how footprint size or shape can affect these properties?

4. I think it should be made clear throughout the manuscript that you are actually comparing the "radar freeboard" rather than "sea ice freeboard" e.g., page 1, line 5. This is particularly important when you're comparing the two instruments. For example, you say that the Envisat freeboard decreases during the season whilst CS2 increases – in actual fact the freeboard is independent of the altimeter (it is a geophysical quantity), but the radar freeboard that is retrieved by the altimeter can be different with different instruments. This distinction has been made in other publications (e.g., Ricker et al, 2014, Armitage and Ridout (2015)) and accounts for the fact that the altimeter free-

board may not correspond directly to the ice-snow interface.

5. Finally, I would consider splitting this paper into two. The first would concern the technical aspects of making a consistent sea ice thickness time series from two different altimeters, and evaluation of the data against in situ and airborne data. The second would use the decade+ long time series to do some science! The scientific value of this dataset is large, and it is wasted here – section 3.5 is just two paragraphs. If you retain the 'scientific' part of this manuscript, you should provide some interpretation – what is driving the inter-annual and long term changes of ice thickness? You should also provide maps of the sea ice thickness through the period, for example autumn (Oct&Nov) and spring (Feb&Mar) average thickness.

Specific comments:

Throughout the manuscript: the authors consistently refer to "freeboard height" - it is a personal preference but I think that you just need to say "freeboard", and not "freeboard height".

Page 1, line 3: "..free of instrumental error as possible". This is a rather trivial statement (of course you wish to minimize instrumental error) however it also misses the point that sea ice thickness uncertainty is dominated by snow loading error, not instrumental error.

Page 1, line 4: It's more accurate to say that you compared freeboard during the 2010/11 and 2011/12 sea ice growth seasons.

Page 1, line 10-12: It isn't valid to present a comparison of the EnvisatPP data with CS2 as a significant result because you are using CS2 to calibrate the EnvisatPP data – so the 'improvement' is by construction! The BGEP comparison is more significant.

Page 1, line 18-19. It would be interesting to test exactly how much ice volume Envisat is missing in the 'pole hole', by comparison with CS2 and ICESat. The 'circumpolar' claim (here and elsewhere in the manuscript) is arguable, due to the size of the Envisat pole hole.

Page 2, line 9: “For *more* than a decade,...” or “Since 2003,. . .”

Page 2, line 14 and page 3, line 3-19: “LRM” – you should refer to the Envisat data as “pulse-limited” rather than “LRM”. Low resolution mode is specific to CS2 and is just conventional pulse limited operation.

Page 2, line 22: Some references are missing: Ricker et al. (2014), Kurtz et al. (2014), Tilling et al. (2015).

Page 2, line 23-page 3, line 2: The “important question” discussed here is not a question at all: CS2 provides better estimates of ice thickness than Envisat because it was designed to! In the late 90s, the question was asked, how can we improve altimeter design to better capture interannual and seasonal sea ice thickness variability? The answer was CS2 – a SAR altimeter, with very high inclination orbit.

Page 2, line 25-26: The freeboard to thickness conversion uncertainty affects both Envisat and CS2 in the same way, so would not result in a bias in Envisat.

Page 4, line 12: the bandwidth (receive) of SIRAL is the same as Envisat, not similar.

Page 4 line 27-page 5, line 7: This relates to my major comment above. You need to provide substantial evidence that intermediate PP waveforms “likely result from thin and relatively flat sea ice”, as this would be contrary to the current understanding as presented in the literature. You say that filtering these data may bias the sea ice thickness high, however there is no evidence of this in other publications presenting comparisons with in situ data (e.g., Tilling et al (2015)). In fact, including these waveforms produces the extreme negative freeboard maps present in Figure 2b. For me, you would have to develop and demonstrate an extremely robust retracker to make use of intermediate PP Envisat waveforms.

Section 2.3: It is surely not valid to use the exact same processing for Envisat and CS2 (PP thresholds, retracker parameters) given the fundamental difference between the

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instruments??

Page 5, line 10-12: Two different retrackers are used in Laxon et al (2013), hence the need for the bias correction. As a point of reference, the SICCI ATBD is actually based on the CPOM processing presented by Laxon et al (2013).

Page 5, line 16-19: Has this retracker been demonstrated for Envisat, or just CS2? If not, then you need to do a proper assessment on the Envisat data.

Page 5, line 21-27: Sea level interpolation causes errors because of lack of lead tie points, snagging, or use of a poor geoid/MSS model. Geophysical corrections have a much smaller effect, as I think another reviewer pointed out. Your method for treating sea level interpolation is new and needs to be demonstrated more robustly against current algorithms.

Page 6, line 4-6: I believe it was Laxon et al. (2013) who first used the "Warren/50% on FYI" methodology, not Kwok & Cunningham (2015).

Page 6/Figure 1: monthly snow depth – wouldn't it be better to use daily ice type masks and match to individual altimeter orbits? The location/size of the MYI area can vary quite a lot over the course of a month.

Page 7, line 17, Figure 2c: You should introduce figure 2c here or move it – perhaps move it to Figure 3.

Figure 3: I find the colourbar used for Figure 3 misleading – normally the red-blue "polar" colourbar is centred on zero, to show positive/negative values. It also makes it appear as though the PP is zero in large areas.

Page 8, line 4-5: Here is an example of misleading use of "thicker freeboard". The radar freeboards are different, the ice freeboard stays the same.

Section 3.2: This section will need considerable revision based on my major comments.

Page 9, lines 11-18: Is the first part of this paragraph necessary? Consider cutting.

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Section 3.3 is good, the most interesting/important development of the paper.

Page 10, line 4-5, Figure 18a,b,j,k. I think it's worth noting that the CS2/EnvisatPP are so similar *by construction*. Currently the paper makes is appear like the agreement between EnvisatPP and CS2 is a significant result in itself, but it is simply a consequence of levelling the CS2 against the Envisat data. This doesn't detract from low RMSE or the good agreement seen with the BGEP moorings, but is an important point.

Section 3.4: I wonder if you could do your evaluation with any other datasets? E.g., Fram Strait moorings have been in place for a long time, Operation IceBridge goes back to 2009, EM-bird data.

Section 3.5: I think this section should be greatly expanded, or else written up as a separate paper. What is driving interannual to decadal thickness variability? This can be done by comparison with ice drift, temperature records, climate indices (e.g., AO). You should compare the Envisat thickness with ICESat. You should present seasonal maps of ice thickness for the entire time period. Are changes in basin mean thickness reflected in changes in volume? What are the implication for heat/freshwater storage?

Page 11, line 23: The references should be in chronological

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