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Discussions

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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.

Anonymous Referee #2

Received and published: 15 February 2017

Review for "Comparison of CryoSat-2 (CS2) and ENVISAT (ES) freeboard height over Arctic sea ice: Toward an improved Envisat freeboard height retrieval" by Guerreiro et al.

General comments:

The study "Comparison of CryoSat-2 (CS2) and ENVISAT (ES) freeboard height over Arctic sea ice: Toward an improved Envisat freeboard height retrieval" compares CryoSat-2 and Envisat freeboard retrievals, certainly for the overlap period 2010-2012. The authors use geo-located CS2 and ES waveforms that are retracked using algorithms and parametrization that can be found in literature. They basically use the same algorithm and parametrization for both data sets (CS2 and ES). They find significant

discrepancies between CS2 and Envisat. In particular, they obtain primarily nega-tive freeboard and winter growth rates for ES, and positive freeboard and winter growth rates for CS2. The authors explain this by an dissimilar impact of ice surface roughness and snow volume scattering on SAR (CS2) and pulse-limited (ES) altimetry. Given this, they use the freeboard height difference between the two datasets as a function of the waveform pulse-peakiness to correct the ES freeboard to be aligned with the CS2 free-board. They show the benefit of this bias correction by comparing CS2, ES and the corrected ES freeboard (ESC) with ice draft measurements from moorings in the Beau-fort Gyre. The comparison reveals a good agreement between the in situ data and the CS2 ice draft, and a significant improvement, using the ESC data instead of the ES data set. Finally, the authors present a sea-ice thickness timeseries from 2002-2015, using ESC and CS2.

The paper gives attention to the problem of comparing sea-ice freeboard/thickness es-timates that are obtained from different sensors. Though, both altimeters use Ku-band, the different footprints/technique (SAR vs. puls-limited) prevent uniform processing and parametrization, in particular with respect to the retracking of the waveforms using empirical threshold retrackers. In order to obtain consistent timeseries, bias correc-tions between different satellite eras are crucial. I think, the approach using the pulse-peakiness for the bias correction deserves publication. However, in the current form, I have some major concerns:

1. In general, I have the feeling that the paper lacks crucial information regarding the methodology, certainly the freeboard processing. Since you indicate using the TFMRA retracker, a very important information, which I could not find, concerns the retracker thresholds. Which values have been used here? Did the authors used the same for CS2 and ES (which I assume)? You refer to the ESA SI-CCI project, but without any reference. The reference Peacock and Laxon (2004) and Laxon et al. (2004) is acknowledged, but just gives a rough idea of the processing. Since you compare free-board, this a key point of the study and needs much more detailed information. Here,

it would be also beneficial to show CS2 and ES waveforms with the corresponding re-tracking points. Also, I would suggest to include an orbit example, showing the along track ice surface elevations, sea surface height and detected leads. This would also highlight the differences between CS2 and ES (ESC).

2. Surely, the Envisat freeboard will be biased when using the same retracking parametrization as for CS2. But still, almost uniformly negative freeboard seems strange to me. But with the few details about processing given in the paper, it is hard to guess the reason.

3. I find the motivation and structure of the paper misleading as well as some terms that are used misleadingly ("negative freeboard", "surface diffusion"). As I understand, you process CS2 and ES freeboard using the same retracking algorithm and parametriza-tion. Then, you compare CS2 and ES, finding negative freeboard and winter growth rates for ES. For the reader, it seems that, a priori, you assume that you would get comparable results when applying the same method for ES as for CS2. Furthermore, CS2 freeboard might be biased as well, though less than ES, as the comparison with the in situ data indicates. Due to the different mode/footprint (SAR/pulse-limited), the effects of surface roughness and volume scattering are represented differently in the CS2 and ES radar echoes. Therefore, it seems evident that using the same threshold parametrization will lead to a more or less substantial bias in both data sets. I suggest to avoid using "negative freeboard" and "negative growth rates", since here, it is not a physical effect as in the Antarctic (flooded sea ice causes negative freeboard), but a bias due to the retracking parametrization. I would also recommend to revise the structure: Make clear that your motivation is to produce a consistent data set. Then, produce CS2/ES freeboard, using the same parametrization, but clarifying that differ-ences are expected. Then, only show the difference plots (CS2-ES), not the absolute freeboard necessarily (move Fig 2c to Fig 3 and discard Fig2 a/b). Afterwards, you can introduce the correction function. You could add a figure then showing the abso-lute freeboard of CS2 and ESC (similar to former Fig 2 a/b) and the difference between

CS2 and ESC.

4. While I agree that Fig.7 is convincing and showing the entire time series is attracting, I think this also needs a more in-deep analysis and information. Over which area have you averaged? How did you deal with the pole holes? Also, separation between FYI and MYI would be interesting. And finally, uncertainty estimates are missing. I would consider discarding/changing this part and rather focus on the overlap years. I would like to see the sea-ice thickness distribution (monthly histograms) for CS2 and ESC for 2010-2012 and corresponding statistics.

In addition, I find sentences sometimes misleading and a bit unclear or too unspecific. I think this can be improved (see in the specific comments). As stated above, in my opinion, major revisions are needed before the paper can be considered for publication.

Specific Comments:

Title: no fullstop.

page 1:

l1: sea-ice . . . I suggest to use hyphenation here and in general, improves readability, though not used uniformly in literature.

l3: "as free of instrumental error as possible". . . this sounds a bit odd. And also, as stated above, I think the goal should rather be to produce consistent time series. Of course, reducing uncertainties is important as well, but doing this individually for both datasets does not guarantee a consistent time series. Any assumptions we have to make for the parametrization may introduce a bias in one of the data sets.

l4: . . . height(s)

l4-8: As mentioned in the general comments, the authors should avoid using "negative freeboard" and "negative winter growth rates". In particular for the abstract, this is very misleading.

l9-10: "Following. . ." In my opinion, this is the key message of the paper.

page 2:

l15-19: "While the. . .": As you mentioned, the SI-CCI product is a prototype product, which has not been published in a journal yet. I suggest to delete these two sentences as they do not really add value to the introduction.

l23: I have the feeling that the authors associate "bias" with "accuracy". While I agree that one can obtain more accurate freeboard and thickness estimates with CS2 (thanks to SAR altimetry), you seem to refer to the bias in the ES data. As mentioned above, this is a bias, which can be corrected (to some point, same as for CS2). It does not necessarily tell us something about the actual accuracy. And also, you argue that the bias in the Envisat ice thickness is driven by the freeboard and not by the freeboard-to-thickness conversion. Why should it be driven by the freeboard-to-thickness? Only, if you use different snow depth parametrization and other density values. Why should you?. I suggest to rephrase the paragraph and rather focus on the consistency between CS2 and Envisat.

page 3

l29: "than" = as l32: What does the CTOH netcdfs contain? geo-located waveforms? l1b elevations? What kind of data are you using? Please, be more specific here.

Section 2: Please be more specific: Which retracker thresholds have been used? It is true that the TFMRA is described already in Helm et al. (2014) (over land ice) and Ricker et al. (2014) (over sea ice). But a short description of the main processing steps is missing here from my point of view.

page 4

l12: "than" = as

l14: Which sea-level corrections do you mean here? DTU15? Tides?

l19-20: You refer to the SI-CCI project but without a reference. This is not very helpful for readers who are not involved in this project.

l21: In general, i suggest reducing the usage of "indeed".

l29-31: I agree that discarding these waveforms might lead to a bias. On the other hand, these waveforms can also result from off-nadir leads (mixed lead-ice waveform), similar shape as thin smooth nadir FYI, introducing a range bias.

l26: WF represents the echo power distribution, no?

page 5

l1: [upper] PP . . .. [lower] PP . . .

l10: "In Laxon . . ." . . . Are you sure? Didn't they use a Gaussian plus exponential model fit for lead waveforms?

l19: "the TFMRA retracker is parametrized identically" . . . Given that, it is seems clear that there will be a bias.

l21-22: Ricker et al. (2016): "The Impact of Geophysical Corrections on Sea-Ice Freeboard Retrieved from Satellite Altimetry" shows that for major parts of the Arctic, the geo-corrections (tides, wet/dry tropospheric Correction, etc.) do not really matter on basin scale. It is mostly the MSS playing a crucial role for the sea-level interpolation.

l24-25: Can the authors provide an along track plot for an orbit? With freeboard, ice/sea surface elevations, detected leads, and also including the filtered retrievals.

l24-26: Why do the authors use a 12.5 km grid (instead of 25 km for example)? Because in the following, you use a 100 km radius for the smoothing? Why such a large radius? I think you will loose lots of details in the spatial thickness/freeboard distribution, also the SARIN box seems to be "interpolated".

page 6

l11: "every" = any

l26: Which density are you using then? I cannot find a number.

l30: "An another" . . . typo

page7

l17: "The parameter . . ." I think it would be better to name it here already and then refer to section 3.3.

l17-18: Again, I find the spatial smoothing too coarse and certainly the SARIN box should be masked when not using the SARIN data.

l18-21: I do not really understand why the authors obtain such a freeboard (-13 cm in average). Even if you use the same threshold as for CS2, I would assume the freeboard to be mostly positive, see Schwegmann et al. (2016). It means that your lead elevations are significantly higher than those from the ice surface. Though I acknowledge that, in contrast to Schwegmann et al. (2016), the authors us the same retracker for both ES leads and ES sea-ice waveforms. Did you check for off-nadir leads? This could also be an issue. Again, I think more information about the freeboard processing are necessary here, for example showing lead fractions and an example for the along track processing.

page 8

section 3.2: I find this section misleading and not well understandable. What do you mean with "surface diffusion"? The Impact of surface roughness?

l32: "As suggested by the visual observation": rephrase, for example: "As suggested (indicated) by Fig.3"

l24-25: ". . .and/or melted snow" . . . Melted snow in November? I am not sure about that, at least not on basin scale. Moreover, this would mean that your observed freeboard is likely not ice freeboard anymore.

page 9

l3, Last paragraph: I do not really understand the point here. Do you mean the impact of surface roughness? Surely, this has an impact when using CS2 SAR altimetry on the one hand and ES pulse limited altimetry on the other hand. But again, I would argue that this is rather a retracking calibration/parametrization issue, when using a threshold retracker.

l15-17: The bias is also a question of how well the thresholds are calibrated. This counts for both CS2 and ES.

page 10

l3: "Looking at" -> "Considering"

section 3.4: So you first tune your ice thickness retrieval? Why are you using different densities here? Why not the same as for the freeboard-to-thickness conversion? This should be consistent. Moreover, you first tune your ice thickness and then you conclude that there is a good agreement with the mooring ice draft data. This is not surprising.

page 11

section 3.5: As suggested above, I think a more in-deep analysis is needed here if you want to keep this part. I would rather focus on the comparison during the overlap years.

page 12

The discussion is very short and overlaps with the conclusion section. Actually, the authors mixed "Results" and "Discussion" in the "Results" section. I suggest, either remove the "Discussion" section and call it (Results and Discussion) or separate them explicitly (which I would prefer).

Figure 3: Color tables: I find the usage of "polar" color tables confusing when they are not centered. May be, consider using a non-polar table, especially for PP, which is not a divergent data set.

Figure 4: I suggest to add CS2 waveforms and corresponding retracking points.

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Discussion paper