Responce to RC2

Response to the Interactive comment on “Comparison of CryoSat-2 and Envisat freeboard height retrieval” by Kévin Guerreiro et al.

First of all, we would like to thank all three reviewers as well as the Editor for their constructive comments and advices that truly helped to improve the first version of our manuscript.

The response to the reviewers is developed as follows:

-The first section provides general comments on the changes and reviews.
-The second part is a detailed answer to each reviewer.
-The last part is a summary of all changes operated in the new version.

I-General comments and modifications:

+ About the freeboard height retrieval

The freeboard height methodology is now further detailed in the new version of the manuscript. In particular, a new section with an along-track analysis is now provided and the retrieval steps are further discussed. We also combine optical imagery with radar altimeter measurement to improve the flow/lead detection and we make the appropriate changes in the freeboard height retrievals.

+ About the Envisat freeboard estimates

First of all, we would like to remind the reviewers that this manuscript would potentially be the first study showing Envisat circumpolar Arctic freeboard maps. In previous published studies, only ice thickness maps were presented and we therefore have no other published study on this topic to rely on.

Regarding the negative Envisat freeboard estimates: as this effect was already described and corrected in sea-ice studies (Giles et al., 2008, Laxon et al., 2013) and ocean studies (Giles et al., 2012, Armitage et al., 2017) we thought that it was not necessary to spend too much time on this topic. Considering the reviewers comments, we now give more insights and explanations on this phenomenon. In particular, the along-track analysis section should truly helps to understand the negative freeboard estimates obtained with Envisat.

Regarding the spatial variability of the native Envisat freeboard estimates: the 2010-2012 period is unfortunately not a good period to observe a high variability of radar freeboard height as the MYI fraction is very low. Having said that, if you look at our estimates for let's say March 2007 (see bellow) you will see that the native Envisat freeboard estimates still capture some coherent spatial variability despite the negative freeboard estimates.

+ About the structure of the manuscript

Following reviewers comments, the structure of the manuscript was modified in order to highlight more clearly the goal of the study: improving Envisat freeboard retrievals in the aim of producing accurate Arctic ice thickness estimates.
In addition to the extra section concerning the along-track analysis, we decided to follow the reviewers comments and to remove the time-series section. These results will be further developed in a new study.

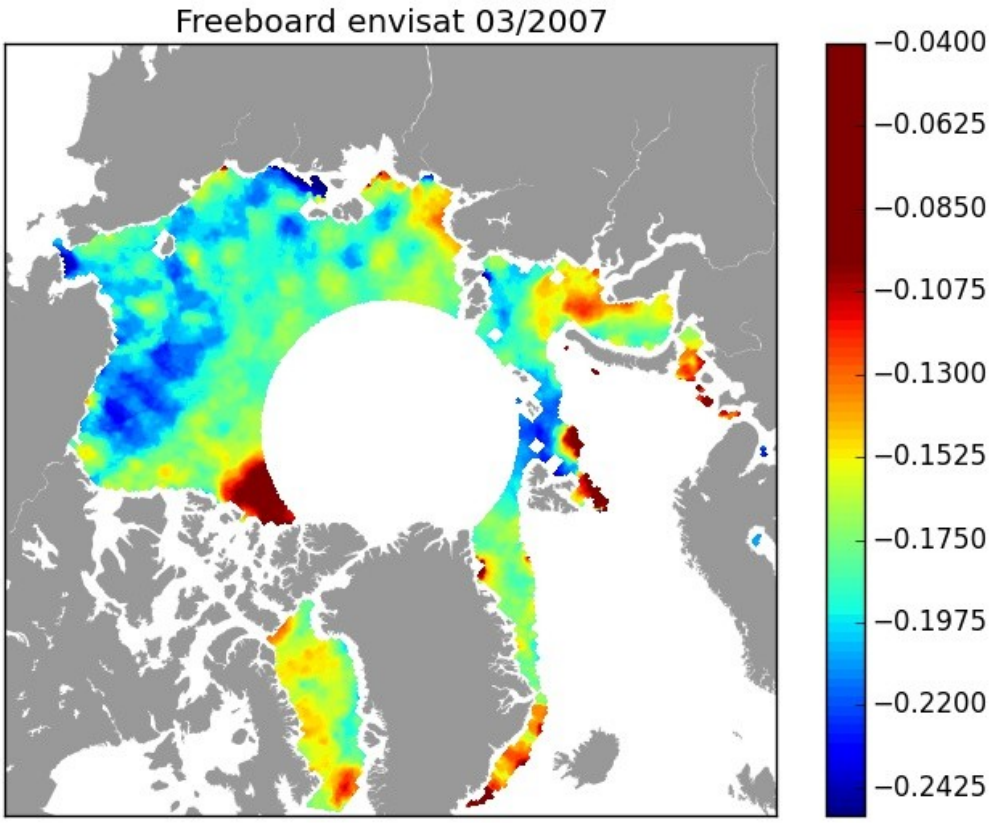

Figure 1: Envisat "native" radar freeboard for March 2007.

Detailed answer to referee #2:

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)?
These informations are now provided in the new version of the manuscript including the description of the TFMRA retracker. It is now more clearly stated that the same retracker is used for CryoSat-2 and Envisat as well as for leads and ice floes.

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 freeboard, 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 retracking 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).
The section in which the freeboard retrieval is described has been improved. In particular, a comparison of radar observations with optical imagery is now provided and a section describing along-track freeboard estimates has been added as you recommended it.

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.
Hopefully, the new sections and further explanations will clarify this particular phenomenon.

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 parametrization. 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.
Excellent point. We clearly do not expect to obtain the same freeboard at the end of the processing chain. All we want to do is to minimize the impact of the processing chain on freeboard height differences between the two sensors. This is now stated in the manuscript.

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.
In the new version of the manuscript, we do not use any longer these ambiguous terms.

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 differences 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 absolute freeboard of CS2 and ESC (similar to former Fig 2 a/b) and the difference between CS2 and ESC.
This is one of the major changes we operated. Thanks to your comments and suggestions, the "Result" section is now developed as follows:

-Comparison of CryoSat-2 and Envisat waveform echoes
-Along-track analysis of surface elevation and freeboard height
-Gridded radar freeboard difference and link with ice surface properties
-Improvement of the native Envisat freeboard height fields with the PP-correction
-Validation of the approach with moorings observations

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.
This comment was also expressed by the other reviewers. We therefore chose to remove this section from the manuscript and to provide more complete results and explanations in a future study.

Title: no fullstop.
Ok
page 1:

l1: sea-ice . . . I suggest to use hyphenation here and in general, improves readability, though not used uniformly in literature.
Thanks for the advice. The hyphenation is now used in the manuscript.

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.
The abstract has been modified in order to clearly display the aim of this study: improving Envisat freeboard retrievals in the aim of producing accurate Arctic ice thickness estimates.

l4: . . . height(s)
This sentence was rephrased.

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.
In the new version of the manuscript, "negative winter growth" is no longer employed and "negative freeboard" is used as few as possible.

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.
In fact, we do quote a published paper to refer to the SI-CCI product [Ridout and Tonboe, 2012]. As the SI-CCI product is the only previous study we can refer to, we would prefer to keep these sentences.

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.
Right. We applied corrections here and throughout the manuscript to take this comment into account.

And also, you argue that the bias in the Envisat ice thickness is driven by the freeboard and not by the freeboardto-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.
That is exactly the message we wanted to pass through. Hopefully, the changes we made will help to clarify the message.

page 3

l29: "than" = as
OK

l32: What does the CTOH netcdfs contain? geo-located waveforms? l1b elevations? What kind of data are you using? Please, be more specific here.
Even though more details are now provided in the new version, I am not sure what you mean by "what kind of data are you using" considering the information we already provide. Is it better now? If not, could you be more specific on you expectations please?

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.
Right, a short description has been added.

Page 4
l12: "than" = as
OK
l14: Which sea-level corrections do you mean here? DTU15? Tides?
OK

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.
As explained above, we do give a reference for the SI-CCI project. In the new version, we repeat a few times this reference to help the reader.

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

Changes were operated as much as possible.

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.
As you and the other reviewers expressed the same concerns about the data filtering, we now filter our data to eliminate ambiguous observations that could potentially drive off-Nadir reflections. Further details on the filtering are provided in section 2.4.

l26: WF represents the echo power distribution, no?
Yes, it is now stated in the manuscript.

page 5
l1: [upper] PP . . .. [lower] PP . . .
This section has been deeply modified.

l10: "In Laxon . . ." . . . Are you sure? Didn't they use a Gaussian plus exponential model fit for lead waveforms?
You are absolutely right. This is now corrected.

l19: "the TFMRA retracker is parametrized identically" . . . Given that, it is seems clear that there will be a bias.
Definitely, yes. But this bias should be constant except if the sea-ice surface scattering has a different impact on one of the two sensors...

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.
In areas where the lead density is relatively low and where the average between 2 leads gets larger, it is likely that even though the effects of these corrections is low, they are not negligible.

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.
YES! We now provide such figure (within a brand new section). We hope it clarifies the explanation about the unrealistic Envisat freeboard values.

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?
As a matter of fact, we simply took the same grid as the one used in the NSIDC sea-ice extent product.
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".
While we could use a lower radius for CryoSat-2 radar freeboard, the Envisat radar freeboard is much noisier and requires a wider smoothing.

Page 6
l11: "every" = any
OK

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

l30: "An another" . . . typo
OK

page7
l17: "The parameter . . ." I think it would be better to name it here already and then refer to section 3.3.
This section was rephrased

l17-18: Again, I find the spatial smoothing too coarse and certainly the SARIN box should be masked when not using the SARIN data.
In the first maps we plotted, we filtered the data found in the SARIN box that were recovered by the coarse spatial smoothing. Unfortunately, the amount of thick MYI is quite rare during the period of study (2010-2012) and this filtering caused the loss of most of the thick freeboard estimates and made the y(PP) relation less valuable. This is the reason why we decided to keep these observations.

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. (2015).
You're right. But in Schwegmann et al. (2015), the authors used 2 retrackers, which artificially correct the negative bias. Everybody processes the LRM freeboard (Envisat, ERS, AltiKa) this way but the reason why such processing is applied is rarely discussed.

It means that your lead elevations are significantly higher than those from the ice surface.
As shown in section 3.2, yes indeed.

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.
Hopefully, we now provide enough details to clarify this topic.

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?
Here and throughout the manuscript, we modified the way of explaining this phenomenon. In particular, we now describe the impact of the ice surface properties on the waveform shape and the consequences when using a threshold retracker.

l32: "As suggested by the visual observation“: rephrase, for example: "As suggested (indicated) by Fig.3"
OK
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.
Right, we removed this part of the sentence...

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.

The new sections provided in the second version should help to clarify this point.

page 10
l3: "Looking at" -> "Considering"
OK

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.
We now use the classical parametrization (882 kg/m³ for MYI and 917 kg/m³ for FYI).

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.
This section has been removed and will be part of a future study.

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).
We tried to clearly distinguish the discussion and the conclusion section in the new version of the manuscript.

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.
We changed back to “jet” in the new version.

III-Summary of changes #1:

With respect to the new version manuscript order:

→ The abstract and introduction have been slightly re-written to clearly express the aim of this study and the key steps.

→ The freeboard processing is now more detailed (sea level, TFMRA retracker, etc). In addition, we add a comparison with Landsat images to validate the use of our PP thresholds.

→ Changes in the freeboard processing chains were applied, all freeboard estimates were re-calculated and figures were updated.

→ The ice density parametrization has been modified and is now more in phase with the literature (882 kg/m^3 for MYI and 917 kg/m^3).

→ A short analysis of CryoSat-2 and Envisat waveforms is now provided (sect 3.1)

→ An analysis of along-track radar freeboard is now provided (section 3.2).

→ Section 3.3 and 3.4 have been inverted.

→ The section showing ice thickness time series has been removed and will be part of a future study.

→ Tables with statistical parameters were improved

→ In general, the physical impact of ice surface properties on the radar signal is more clearly explained.