

Response to RC3

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.

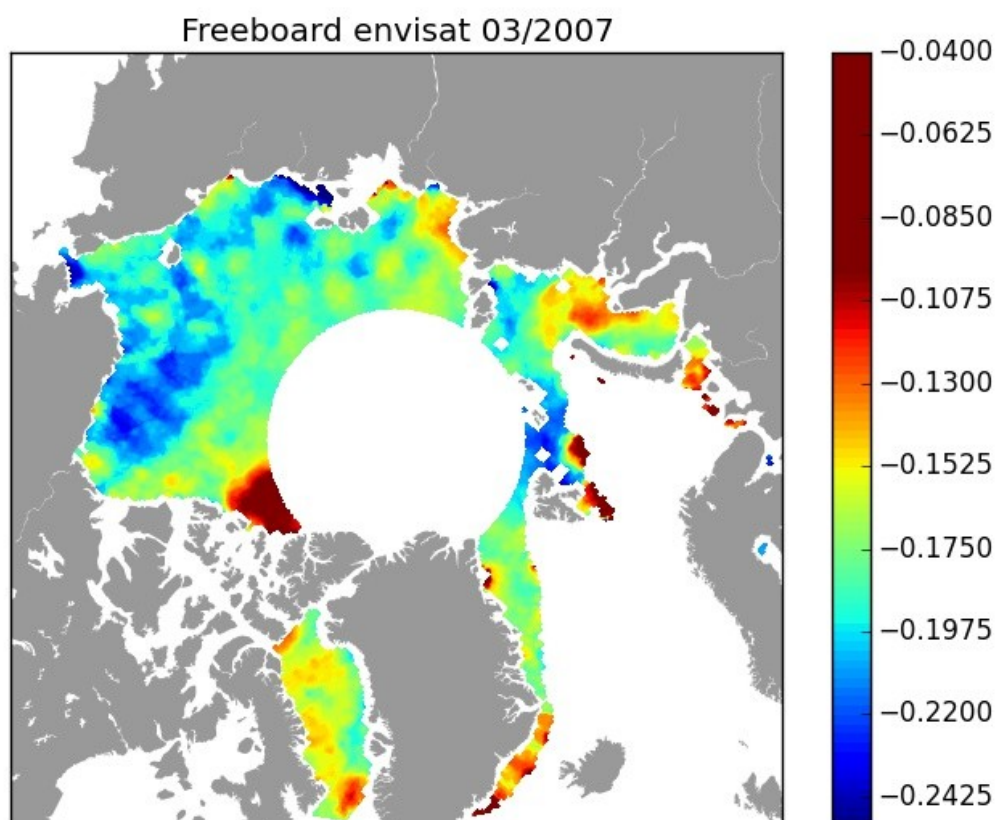


Figure 1: Envisat "native" radar freeboard for March 2007.

Detailed answer to referee #3:

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 C2 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.

As you and the other reviewers agree on the fact that it is essential to filter radar observations characterized by an intermediate PP value, we now filter these ambiguous data. The threshold values were selected by combining optical imagery with radar observations as you recommended it. As a matter of fact, the freeboard is slightly improved but is still highly negative.

Let's consider your interpretation. If off-Nadir reflections are indeed the cause for the negative freeboard estimates, then the Envisat radar freeboard should be further negative in regions with a high concentration of leads (regions with a high PP). However, it is precisely in these regions that the freeboard is the least underestimated (relatively to CryoSat-2). Thus, this interpretation doesn't really get along with the results we show.

To present the problem in a different way: you recently posted a paper in TC concerning sea-level estimates. To obtain these sea-level estimates, you use one retracking algorithm for sea-ice leads

and one retracker algorithm for open ocean surfaces. But if you estimated the sea level with only one retracker, you would obtain an average sea level elevation in leads 20-30 cm above the elevation you obtained over open ocean surfaces.

For sea level studies, this approach seems fairly reasonable as you have 2 very distinct types of surfaces (leads and open ocean). But if you now consider sea ice, there is a wide range of ice types that all have a different impact on the freeboard retrieval. It is the main purpose of our study to describe and correct this phenomenon. Hopefully the new organization of the paper and the new details we provide will help to clarify this.

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?

It is a phenomenon widely described in oceanography and it applies even more over sea-ice. In the study by Chelton et al., 2001 (http://geodesy.geology.ohio-state.edu/course/refpapers/Chelton_ altimeter_02.PDF) it is explained how the footprint size can be impacted by surface roughness (have a look at figure 7b). I believe that, one should say “effective footprint” rather than “footprint” alone to avoid any confusion, which is not commonly done in the literature...

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 freeC3 board may not correspond directly to the ice-snow interface.

You are absolutely right, talking about radar freeboard would avoid lots of confusions. Changes have been operated throughout the manuscript.

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.

Here as well the other reviewers share your opinion. We therefore remove this section that will be further developed in a future study.

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”.

Thanks for the advice. We operated the corresponding changes throughout the manuscript and it does indeed make the manuscript clearer.

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.

This section was rephrased.

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.

OK

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.

We agree with you. The message we want to bring here is that the PP correction works for every months and during the 2 ice growth seasons. This part has been rephrased to make the message clearer.

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.

It is indeed something we would like to do in the future study to emphasis or not the ice thickness estimates bellow 81.5°N.

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

OK

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.

We now use pulse-limited instead of LRM.

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

Only Tilling et al. (2015) treats of ice thickness. This reference is now added in this section.

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.

Right, but it is still insightful to understand why C2 is better and we think that it is important to not reduce radar altimeters as an instrumental concept and mission requirement. In fact, it is by FULLY

answering this question that we improve the accuracy of the Envisat freeboard estimates.

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.

Yes indeed, this is the message we want to pass through... It is now rephrased. Hopefully in a better way.

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

That is correct, thanks.

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.

As discussed above, this part has been strongly modified and we now filter ambiguous observations.

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 instruments??

Regarding the PP threshold, there is now a demonstration with the use of collocated images. Regarding the retracker parameters, the issue that we might faced when applying the same retracker to the two sensors are now discussed in the new version.

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).

You are right. Thanks for pointing at this error.

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.

As Dr. Rinne (RC1) mentions it in his review, the TFMRA retracker have been tested on Envisat by the SICCI group and seems to have good results. Hopefully, Dr. Rinne will provide a reference for this result.

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.

In fact the methodology we use is quite similar as what is generally found in the literature. The main difference is that we do not estimate freeboard height where no tie point is identified. We rephrase this section in order to clarify our methodology.

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).

Yes indeed. However, in Laxon et al. (2013) the authors use a binary parametrization (0.5 or 1). What we use in our study is a progressive parametrization (from 0.5 to 1), which was first developed in the study by 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.

This is indeed a good idea that we will most likely develop in the future. As our study is no longer related to climatological studies, we stick to what is done in the literature.

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

This was also proposed by reviewer #2. We modified the organization of the manuscript so the problem you rise is no longer an issue.

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.

We changed back to regular “jet” colormap.

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.

OK

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.

This section was highly modified.

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.

As explained above, we now emphasis more on the good correlation for each month of the period of study than on the general agreement, which is indeed not an actual evidence.

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.

For our next study (more climatological this time), we consider using new validation datasets such as the one you cited. I doubt that measurements obtained by the Fram Strait moorings will match perfectly with the altimetric estimates considering the sea-ice dynamics in this region. Still, it is still worth to give it a try.

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?

As mentioned above, this section has been removed and will be part of a full study.

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

OK

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/m³ for MYI and 917 kg/m³).
- 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.