The Cryosphere
Discussions

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 manuscript presents a time series of circumarctic sea ice freeboard (and thickness) from two satellite altimeters. This is an extremely relevant for TC and significant and the paper is reasonably well written, although lacks detail in some important parts. The authors address the problem of combining measurements of two different altimeters (Cryosat-2 being delay-doppler and Envisat RA-2 not) by introducing a correction to Envisat freeboards based on Pulse Peakiness, which is a novel and original idea that has never been published before. However, there is a striking problem with the Envisat freeboard estimates before the PP correction is applied, and thus I urge the authors to fix their Envisat base methodology before this paper is considered for publication.

Furthermore, even if the PP correction is shown to produce very good agreement with Cryosat-2 and BGEP data, the theoretical justification of the correction is lacking. Both of these two issues should be addressed before the manuscript is published in TC and my suggestion is that the manuscript should go through major revision before accepted.

The major issues in the manuscript are:

1. The Envisat FB product before PP correction

Looking at figure 2 middle column, one can see that the Envisat freeboards are unrealistic. For one, they are negative – something that the authors just attribute to "the difference of ice surface characteristics between leads and ice floes as well as the use of a threshold retracker drive a large bias on the estimation of Envisat freeboard height". I am confident that the culprit is elsewhere.

We've tested the TFMRA retracking scheme for Envisat as well in the CCI project, and we've arrived at more or less similar looking freeboard maps as with the original CCI retracking scheme. We seem to be missing the thinnest and the thickest ice, but freeboards are positive as they are supposed to be and the thickness pattern reflects reality (even with the thinnest and thickest ice missing). And furthermore, we do not see very high freeboards in the marginal ice zone.

I try to be a good reviewer and speculate possible causes for the Envisat freeboards being much off. My guess is that this may be due to off-nadir leads or new ice dominating significant number of waveforms. The authors give very little notice to filtering out mixed waveforms. Or filtering in general – it is hardly mentioned anywhere in the paper. They argue that they should keep in the waveforms with intermediate PP since they represent thin and undeformed ice. Fair enough, but at the same time they are letting in a lot of waveforms with deformed ice in the nadir and flat areas off-nadir which will lead into the retracker catching the off-nadir rise and biasing the elevation estimate low. This is consistent with the lowest freeboards seen in the area with lot of deformed ice (there will always be a significant number of flat new ice or leads around). This is

less of a problem in the area of new ice near the margins, where the ice is more or less flat all around and in likelihood there is a specular surface in the nadir as well. All consistent with the pattern in figure 2.

The authors hint that the use of TFMRA retracker is robust for off nadir reflections (page 5, lines 16-19). That is somewhat true, but it does not remove the need to filter out dubious waveforms – even Helm et al 2014 that the authors cite for the TFMRA have a filtering scheme to remove "bad waveforms" before retracking. I suggest the authors build one too and check if that improves their not-PP corrected freeboards. The SI-CCI scheme most likely filters too much waveforms, but I would still argue that some kind of filtering is required.

Finally , much less likely culprit than previous one , but worth mentioning still since applying an inverted snow correction (that is, a bug in code) results into something bit like the maps in Figure 2. The main reason I'm mentioning this is that I once had that bug in my code and the Figure 2 reminds me much of it. Don't waste too much time on this, but do check your snow propagation correction code.

2. Theoretical justification of the PP correction

The manuscript fails to explain the theoretical background of why exactly small PP (or more diffuse waveforms or heavily deformed ice) results into retracker picking up the tracking point later in the waveform that it would if the waveform was more peaky (less diffuse and most likely originating from less deformed ice). The authors state that "ice surface diffusion has a higher impact on LRM altimeters" but this needs to be backed up by something solid because from the evidence authors give. Because of the unrealistic Envisat FB, I do not believe that the disagreement of the Envisat and CS-2 freeboards is mainly due to surface diffusion. If the authors do not, theoretically step by step, explain the process of ice surface diffusion impacting LRM altimeter estimates, a good referee could (and should) claim that it is just as likely that what we are seeing here instead is something profoundly wrong with the Envisat FB retrieval and that something

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is connected to pulse peakiness.

The y(PP) is problematic anyway. Naturally, applying any correction derived from the difference of the two freeboard datasets will make the two agree. Strongest point the authors give for the use of the y(PP) correction is the improvement it brings to the fit of BGEP data throughout the Envisat period. This is all good and well, but looking at figure 6, the only real improvement is the level correction of about 1 – 1,5 m to the (unrealistic and often even negative) Envisat draft estimates. I would argue that what we see here is the constant term of y(PP) – there must be one since the dashed line in figure 5 does not cross zero – just fixes the large negative bias that the somehow broken Envisat freeboard method produces.

Furthermore one could argue that there is a relationship between PP and ice thickness. Thicker the ice, more deformation there is, thus rougher the surface and finally smaller PP. I find it likely that one would get reasonable results if they would just set thickness to be a function z(PP) only by fitting arctic-wide freeboard maps (from CS-2) to Envisat PP. One would also get a nice seasonal pattern of thickness since PP will go down through the winter with thicker ice and more deformation. Thus we do not even need retracking process to explain why introducing a PP correction improves the fit to independent data.

After the harsh critique above, I should mention that the idea presented in the manuscript is most definitely on the right track! A PP based correction would improve the problems of Envisat FB retrieval drastically. I know of similar attempts in the altimetric community lately. After fixing their uncorrected FB estimates and giving a theoretical justification of how the correction works, this will be a really good paper and I commend the authors for coming up with the idea and publishing it first. Problem with the manuscript at the moment is, that even if the final result of corrected Envisat freeboards seems to comply with validation data, the paper fails to give rigorous explanation exactly what are the processes why their methodology works.

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Minor issues:

1. Pan-arctic claim

The authors claim that they have created a pan-Arctic thickness estimate. They have not, since they have excluded all of the Arctic above 81,5 N. Thus I recommend the authors follow the lead of Giles et al and stick with "circumpolar" (or something similar) to emphasise that their estimate does not cover all of the Arctic.

2. TFMRA parameters

Nowhere in the paper the authors state, which threshold value they use for the TFMRA. 50%? It should be mentioned. Like other TFMRA parameters as well.

3. Local sea level interpolation

The description of the sea level interpolation is thin (page 5, lines 20 – 24). Not sure if the interpolation of leads could contribute to the unrealistic negative freeboards, but it is worth checking. Nevertheless, the authors must include a better description of the sea level interpolation – how exactly is it done? Taking a mean of all lead elevations within 25 km or some kind of along-track interpolation?

4. PP correction – are leads included?

On page 5, line 27 it is stated that PP is also averaged into gridded maps. Does this include the waveforms that are classified as leads? If it does, this will have a consequence to the PP correction – that is, areas with lot of leads will eventually have a stronger correction in the direction of thinner ice.

5. Mathematical description of y(PP)

The authors really must give a more thorough description of the y(PP). Is the y(PP) constant throughout the winter? I reckon it is the black dashed line in Figure 5, and constant over time and place and calculated on the gridded level and not for individual measurements, but a mathematical formulation would be most welcome.

6. A typo:

Page 10 line 5: Candian