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Interactive comment on "Consistent CryoSat-2 and Envisat Freeboard Retrieval of Arctic and Antarctic Sea Ice" *by* Stephan Paul et al.

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General Comments

In order to produce consistent freeboard from CryoSat-2 and Envisat, the authors outline the importance of having consistent leads and floes classification in one hand, and of having consistent waveform retracking in the other hand. The main difficulty of the task is due to the fact that the altimeters do not rely on the same technology, the traditional LRM altimeter for Envisat and the more recent SAR altimeter for CryoSat-2, and their radar echoes strongly differ.

For that purpose, they propose novel technics for these two main steps necessary to retrieve freeboard.

Regarding the classification, in order to overcome the rigidity of a fixe threshold on a unique parameter all over the year, they propose to rely on a combination of parameters, that can be computed on the waveforms for both altimeters: the pulse peakiness (pp), the backscatter (sig0) and the leading edge width (lew). The best criteria for the lead/floe classification is elaborated using a sequence of k-mean clustering followed by a random forest classification, and this for each of the months. In this approach, the first k-means clustering step is supposed to provide a ground truth for the next classification. Using this method, they produce statistically consistent lead/floe classifications between Envisat and CryoSat-2, and increase the number of "valid" waveforms in comparison with the SICCI-1 solution, which in turn allows increasing the resolution of the freeboard maps. However, if the consistency of the classification between the two satellite missions is an important prerequisite, it does not fully demonstrate the pertinence of the classification regarding the ground reality.

From the observation that a fixe threshold retracker is not able to retrieve a consistent freeboard distribution with a LRM altimeter, the authors propose to adapt this threshold using CryoSat-2 as a reference. The objective being to find out a law for the variable threshold according to two parameters of the waveforms that characterized the measured surface: the backscatter and the leading edge width. Thus, the philosophy is similar to the one suggested in Guerreiro et al (2017), but the method differs: this previous study uses fixe thresholds retrackers and computes a corrective law on the freeboard, from the discrepancies between Envisat and CryoSat-2 freeboards, according to the pulse peakiness to account for the surface roughness. In this new study, the threshold over the leads, as perceived by Envisat, is fixed whereas the threshold over the floes varies by step of 5% in order to find out the value that minimizes the freeboard differences with CryoSat-2, and this for each cell of each monthly grid. The law that expresses the threshold value according to sig0 and lew is then established using these datasets. In both cases, the freeboard outputs from CryoSat-2, using TFMRA, is considered as a ground truth. From this reference, a corrective law on the threshold -or the freeboard- outputs from the more sensitive LRM waveforms of Envisat, still using

TFMRA, is deduced. This emphasis the fact that, the heuristic retrackers, which are still necessary to study surfaces too complex for physical models, are approximations which need to be calibrated and validated against representative ground truth.

In summary, this paper presents two novel technics to improve the consistency between Envisat and CryoSat-2 concerning respectively the lead/floe classification and the retracking. The announced results seem very good, in particular in Arctic, but the corresponding plots are not shown (eg, plot showing the R2adj of 0.94 for the freeboards in Arctic) and some comparisons with in-situ data would strengthen the work.

Specific comments

- The second step of the classification is qualified as "supervised" but for me this means supervised by an operator or guided by some external data. It does not seem to be the case here, so could you precise what you mean by "supervised" and "supervised training"?

- The initial classification being done on selected surfaces, above 70°N and avoiding marginal zones, could not this explains the later-on difficulties for these zones?

- There are no quantitative results of the progresses regarding the classification.

- It looks like that you interpolate the heights of floes and leads - and thus the freeboard- all along the track, independently from the surface classification or the distance to the nearest lead. Could you confirm this (defendable) strategy?

- The impressive correlation obtains between Envisat and CryoSat-2 freeboards should be illustrated in order to make the fitting more demonstrative (or at least providing some other statistical characteristics).

- Some comments on the relative importance of the 3 considered parameters (pp, sig0, lew) for the classification and the range correction for Envisat floes would be appreciated.

- Some statements need to be argued (see the Technical Corrections part).

- Some references should be added for: the product that discriminate FYI and MYI, for OSISAF and for DTU15 (even if well known, it is nice to reference them).

- Because of the distinction between sea-ice and leads all along the study, the expression "sea-ice backscatter" is ambiguous as most of the cases it refers to the "surface backscatter" (ie, a mix of sea-ice and leads). This expression can also be simply replaced by "backscatter" as it is a parameter that characterizes the waveform, like the pp or the lew.

- I recommend using the same color-bar theme for the map plots when the purpose is to compare some parameters (of course not necessarily with the same extreme values which depend on the units).

Technical Corrections

- §1, p.2, l.5: what do you mean by "quasi-nadir" (off-nadir data do not measure the right range) and by "run-time measurements" (data are processed off-line).

- §1, p.2, l.6: I dont agree with the sentence: "so accurate that one can see the difference in elevation of the snow surface or the sea-ice surface relative to the sea surface on the leads". All the along-track plots of the ranges show terribly noisy measurements, which justify all the studies to classify the surfaces and filter the ranges. At least you should illustrate or quantify this affirmation.

- §2.1.2, p.3, l.27-28: references for OSISAF and DTU15

- §2.1.2, p.3, l.28: you mean "data filtering"? I suppose you don't analyse the waveforms.

- §2.1.2, p.4, l.2-4: for me the discrepancies between W99 and the snow depth on FYI is mainly coming from the more and more late development of the new sea-ice in the season due to the global warning that strongly impact the Arctic. This delay limits the

possible accumulation of snow on sea-ice. But this worth to be checked.

- §2.1.2, p.4, l.6-17: could you provide with the name of the used product and if possible a reference?

- §2.2, p.4, l.29-32: could you precise the percentage of removed data ?

- §2.3.1,p.5, l.4-6: could you precise what makes you tell that the sea-surface height products are not reliable? Which products?

- §2.3.1,p.5, l.10: I would say more precisely that off-nadir measurements provide wrong ranges.

- §2.3.2,p.6, l.9: with "three classifiers", you mean "three (classifier) parameters"?

- §2.3.2,p.6, l.10: what is the limit for the southern ocean?

- §2.3.3,p.8, l.2 and 7: could we state that 1 « 3? What are the possible impacts?

- §2.4,p.9, l.13: please precise the smoothing function that is used.

- §2.4,p.9, l.25: in what way a 50% threshold for leads and floes is "consistent". Why is it more consistent for CryoSat-2 than for other altimeters?

- §2.4,p.9, l.30: "However" can be removed as the same conclusion is drawn in Guerreiro et al 2017

- §2.4,p.10,F.2: use a unique color-bar

- §2.4,p.10,l.9-10: is there any reason to prefer sig0 than PP ? It could be nice to have also a plot with PP. Visually, the matching with lew is impressive.

- §2.4,p.10,l.5: the sentence here could let imagine that only one monthly value is used in Guerreiro et al. 2017 to establish the correlation. Perhaps you could remove the 2 words "monthly" or precise that all the monthly cells are used.

- §2.4,p.12,l.18: Could you also provide R2 which is more frequently used and for which

we have more references. A plot showing the distribution and the fitting curve would be very welcome. The correlation is just one characterization, among many others, of the fitting and it is not very intuitive.

- §2.4,p.12,l.19: could you display the central Arctic region on one of your maps?

- §2.4,p.12,l.29: could you show on a map the regions where the sig0 and the lew are less correlated? In particular for the lew it is not so obvious.

- §3.1,p.14,l.12-13 and p.15: Could you provide with some quantitative values to illustrate the progress regarding SICCI-1?

- §3.2,p.16,l.23-25: it is not clear whereas all the numbers are related to the current study or some of them concern SICCI-1. For instance the "three cm" line 23 seem in contradiction with the "2.2cm" line 25. Could you provide some quantitative comparison with SICCI-1?

- §3.2,p.19,l.10: typo "Shown are the same months".

- §3.2,p.19,l.21: I don't understand the sentence: "In Antarctic, while the differences are lowered, the overall differences remain larger".

- §4,p.22,l.20-21: and how far are you confident in the AMSRx solution in Antarctic?

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