Response to comments of reviewer #2 to

"About uncertainties in sea ice thickness retrieval from satellite radar altimetry: results from the ESA-CCI Sea Ice ECV Project Round Robin Exercise"

by

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In the following we give the comments of the reviewer in *italic font* followed by our response in regular font. We thank the reviewer for the helpful comments.

General comments

The authors present an analysis of the effects of snow depth, snow density and ice density on the retrieval of sea ice thickness from pulse limited radar altimetry. They conclude that the Warren climatology is no longer representative of snow conditions, in line with several other studies. They also make recommendations regarding the sensitivity of ice thickness to ice type dependent sea ice density.

One of the main objectives of the study is to 'characterize the uncertainties in the sea ice thickness product based on the uncertainty of the input parameters'. However, as has already been mentioned by another reviewer, the authors do not present an analysis of the error on the freeboard retrieval itself. This is not an insignificant source of error or possibly bias and is dependent upon several factors including the radar speckle noise, the local sea level interpolation (the abundance of lead measurements), radar penetration into the sea ice pack, filtering of contaminated waveforms, retracker noise/biases as well preferential sampling of larger (thicker) floes. See Ricker et al, 2014, 'Sensitivity of CryoSat-2 Arctic sea-ice volume trends on radar-waveform interpretation',The Cryosphere, 8 1831-1871 for a more detailed discussion of freeboard retrieval uncertainty. The abundance of lead measurements, and sea level interpolation error, in particular is considered to be a significant source of error even for CryoSat-2, and will be more severe for pulse limited systems. Kurtz et al, have also looked in some detail at the freeboard retrieval (again for CryoSat-2) in their paper "An improved CryoSat-2 sea ice freeboard and thickness retrieval algorithm through use of waveform fitting", The Cryophere, 2014, 8, 721-768. There is also the issue of off-ranging to leads biasing the sea surface elevation low, which was examined for CryoSat-2 by Armitage et al, 2014, 'Using the Interferometric Capabilities of the ESA CryoSat-2 Mission to Improve the Accuracy of Sea Ice Freeboard Retrievals' IEEE Transactions on Geoscience and Remote Sensing. Similarly, this effect can be expected to be more severe for pulse-limited systems. Unless this paper is extended to address the impact of the freeboard retrieval uncertainty I feel that the scope of the study should be limited to 'examining the impacts of snow depth, snow density and ice density on ice thickness uncertainties', or something similar. In effect, what the authors have presented thus far is an assessment of the sensitivity of ice thickness retrieval to snow depth and the density of ice and snow that is somewhat independent of the altimeter measurements themselves.

We take this note and thank the reviewer for this suggestion. Based on this we indeed changed the title of the manuscript to: "The impact of snow depth, snow density and ice density on sea ice thickness retrieval from satellite radar altimetry: results from the ESA-CCI Sea Ice ECV Project Round Robin Exercise".

It is beyond the scope of our current capabilities to come up with an uncertainty estimation of RA-2 and RA data which would satisfy the readership. This needs to wait until a later stage of the project.

The manuscript is in general well written and the figures are clear however the discussion section, in particular, is quite long-winded and hard to follow. I feel that this section could be trimmed down to 2/3 or even half of it's current length without a significant loss of content.

Noted. The revised version of the manuscript will be more concise in the discussion section.

Detailed comments
P1519 L16: Given the claim that this is the first time a combined time series of ERS1/2 and Envisat data has been constructed, has an inter-satellite comparison been performed to check for inter-satellite biases? These could arise from e.g. differences in the on-board tracking systems. An inter-satellite comparison could be indicative of the accuracy of the freeboard retrieval algorithm and is fairly standard practice when looking at multi-mission time series.

Yes, we agree that this should be done. However, at the time of writing the manuscript we did not have the re-processed REAPER data of the ERS1/2 RA data in our hands so that this part of the work needed to be postponed to a later stage of the project.

P1521 L9: What does this stand for?

We have included "(SICCI project)" in line 2 of the introduction now. ATBD stands for Algorithm Theoretical Basis Document. We have added a reference in our reference list for clarity.

P1521 L22: The number of freeboard measurements per grid cell affects the thickness uncertainty as well as the use of external datasets like snow depth & density. This should be addressed in this paper.

The biggest source of uncertainty in a single freeboard measurement is speckle, which for RA2 is of the order of 10 cm. The uncertainty due to speckle of an elevation measurement of 100 averaged elevation measurements would then be 10 cm / sqrt(100) = 1 cm. We note, however, that this is just one contribution to the uncertainty and that ice-type and surface roughness influence the altimetric waveform and the radar backscatter and can lead to substantial biases in obtained altimetric height. Also different floe sizes are likely to introduce biases. But this is not what we are after in the current study. We are after finding a proper set of input parameters for the freeboard-to-thickness conversion and assume – in a way – that the freeboard obtained from the RA-2 data is correct. Actually, the comparison between OIB total freeboard and total freeboard computed from RA-2 sea ice freeboard using OIB snow depths point into the direction that this assumption is perhaps not the worst one. It was the nature and the rationale of the Round Robin Exercise to do this. The rationale of the RRE was not to play around with different re-trackers to obtain different realizations of RA sea ice freeboard but rather to check out how reliable are the input data sets and assumptions of constant densities etc. The validation of sea ice thickness will happen in a later part of the project.

P1522 L3-13: As has been pointed out by another reviewer, the Warren climatology cannot be considered to be valid in Baffin Bay, the Canadian Archipelago or Hudson Bay (as well as the Fram Strait on P1526 L7, figures 4&5) as the polynomial fits are not constrained by measurements in these regions. See figure 1&3 from Warren et al, 1999, for an indication of where there are observations in their climatology. It is not justified to present comparisons in regions where the data are not valid.

We have answered the concerns of reviewer #1 in a similar way as we will do here. The Fram Strait area is pretty close to the region for which the W99 Climatology claims to provide reasonable snow depth data. In contrast to areas such as the Bering Sea, Canadian Archipelago and Baffin Bay sea ice is exported out of the Arctic Ocean directly into the Fram Strait area; the time to reach the area shown in Figure 4 is maybe one month. We can assume that during winter the snow depth on sea ice in that area is predominantly determined by the snow depth on the sea ice upstream. This is confirmed by rather similar values of W99 snow depth upstream, in the Arctic Ocean, and in the Fram Strait area. Furthermore, during winter it will be rather unlikely that snow melt would reduce the snow depth from about 40 cm to about 20 cm during the passage from the Arctic Ocean to the Fram Strait area shown. Therefore, because the link between the snow depth on sea ice in the Fram Strait area and the Arctic Ocean is much stronger than the link between the snow depth in the Canadian Archipelago or the Baffin Bay and the Arctic Ocean, we keep the figures and statements related to snow depth in the Fram Strait area. In order to comply with the reviewers' concerns we added the information stated above further down in the discussion of Figure 5. There we now write: "…one in the Canadian Archipelago. For the latter region we only compare OIB and AMSR-E snow depth data in the following because W99 snow depth and density data rely solely on extrapolation in this region. The same applies to the Fram Strait area (see Figure 4). However, the sea ice cover in the Fram Strait area is quite dynamic and originates from the Arctic Ocean while the sea ice cover in the Canadian Archipelago is much more static. Hence it can be assumed that at least during winter the sea ice and snow properties in the Fram Strait area are similar to those upstream in the Arctic Ocean, which is actually confirmed by the W99 data, while those in the Canadian Archipelago are determined by local processes and the sea ice which entered the region during the previous summer season."

We write further:
"… depth; data from the Canadian Archipelago are excluded. Figure 5 b) suggests that W99 snow depths are twice as large as AMSR-E ones over FYI in the Arctic Ocean; the difference… "

P1523 L10: Provide references for this statement i.e. lab work by Beaven et al 1995. It should also be mentioned that this idea has come under question, in particular by Ricker et al, 2014 (above), and justification for continuing use of this assumption should be made.

We added the suggested reference and also added the sentence: "There is growing evidence that this assumption does not hold for more cases the previously thought (e.g. Ricker et al., 2014)."

P1523 L15,26: Do you use different averaging distances for the different datasets? If so, why? Or is this a typo?

We do use them. The reason for a shorter averaging distance for FS flights is that they were much shorter than OIB flights. 100 km would had been optimal to use for FS too, but that would had resulted in 5 and 10 data points for years 2008 and 2010 respectively. For OIB we have thousands of kilometres of track available and we can use a longer averaging span.

P1523-24: How come airborne Electromagnetic sounding (i.e. EM Bird) data has not been used? This could be a useful comparison as it measures the ice thickness directly.

We agree. This is driven by the nature of the SICCI project and the layout of the Round Robin Exercise. It was decided to keep data such as EM-Bird measurements or ICESat sea ice thickness data for the validation of the product. We would like to mention though, that EM-Bird data provide the total (sea ice + snow thickness) and are thus not a direct measurement of the ice thickness. Again additional information would be needed such as snow depth and / or the location of the ice-snow interface as can be obtained, e.g., from the OIB data.

P1525 L15: Please see my above comments about addressing the freeboard uncertainty. As the paper stands you have not addressed all of the input parameters.

We agree. We refer now to the fact, however, that we like to follow the suggestion of the reviewer to let the paper indeed focus on the impact of snow depth, density and ice density on ice thickness retrieval using radar altimetry and not try to provide some half-ready information from our preliminary investigations about ice freeboard uncertainties based on RA and/or RA-2 data.

P1526 L9,13: Where are these values taken from?

The sea ice density value of 900 kg/m^3 is simply the arithmetic mean of the densities of FYI and MYI given further down. A reference for the water density used is now given.
Snow densities 240 and 340 kg/m^3 have been chosen according to the mean seasonal cycle of the snow density according to Warren et al., 1999. We tried to make this more clear in the revised manuscript. It reads now:
"For the standard computations as given above under C) and D) the following values are used: ρi = 900 kg m-3 (mean of the densities of first-year and multiyear ice) and ρw = 1030 kg m-3. For multiyear and first-year ice we use sea ice densities published elsewhere (e.g., Timco and Frederking, 1996; Alexandrov et al., 2010): 882 kg m-3 and 917 kg m-3, respectively. Snow density varies over space and time in the W99 data set (see Fig. 1 e, f). For C) we vary snow density values accordingly between 240 kg m-3 and 340 kg m-3."

P1526 L15: Both radar and laser altimetry techniques are sensitive snow depth, snow density and ice density, but in different ways. This is not clear from your statement.

We agree. We found that the paragraph "Note that the sea ice … according to W99" is not required here and deleted it from the manuscript.

P1527-1528: See above comments on the Warren climatology validity.

See our reply to this topic above.

P1529: See comments on Warren climatology validity. Also, considering that you have shown the Warren snow depth is about double that of OIB and AMSR-E snow depths, it is rather trivial to say that the altimeter 'snow freeboard' is higher than the campaigns freeboard when you apply the Warren climatology. The rationale for converting altimeter freeboard to a 'snow freeboard' should be clarified anyway, or else left out and focus on the sea ice thickness.

The rationale behind this was that we wished to inter-compare RA sea ice freeboard values as much as possible with independent data. As there are no other sources for sea ice freeboard (except ASIRAS which unfortunately failed) we needed to find a way to compare the data at a different level. OIB provides snow freeboard. This is a direct measurement. OIB provides snow depth. This is also a direct measurement but more uncertain than the snow freeboard measurements. Hence our strategy was not to fiddle around with OIB data and claim that the sea ice freeboard one can retrieve from two uncertain products can be compared well with the RA sea ice freeboard but rather look how close snow freeboard computed from RA sea ice freeboard – which we assume to be correct in this case – comes to the observed snow freeboard.
We would like to again stress that the manuscript does not aim for the validation of sea ice thickness obtained from RA data. We are not there yet. The manuscript aims at providing useful information about the limitations and caveats of utilizing RA data. Validation / inter-comparison of RA sea ice thickness will come at a later stage. At the time of writing RA-2 sea ice thickness was not yet retrieved.

P1531 L5: The seasonal range of ice draft derived from altimetry will also be influenced by the seasonal range of snow depths and densities that are applied, as well as the FY/MY ice densities. Would it be possible to examine the effect of using different snow depth/density values as well as ice density values? If not, this should at least be mentioned.

We hoped that Figure 8 will give enough information into this direction as we here "play" around with both different ice densities (MYI and FYI) and snow densities.

Figure 9: Whilst these results are not particularly encouraging for RA-2 thickness retrievals, I am still not convinced about your freeboard retrievals and data filtering given that you are reporting such large negative thicknesses. Considering that these are monthly, gridded and smoothed data you would not expect to see RA-2 ice thickness of -2.5m. Negative ice freeboards are understandable from a data processing point of view (and also can occur in reality) but on the spatiotemporal scales that you are presenting the data one would not expect to see these large negative thickness values. This requires some explanation.

We agree. We thank the reviewer for pointing this out to us. In fact, when reviewing Figure 9 we became less worried about the one negative RA-2 sea ice thickness value but more worried about the varying number of also negative OIB sea ice thickness values. We really should have captured this MUCH earlier and we apologize that we have been that sloppy in our assessment of the figures. However, as the RRDP does not contain negative sea ice thickness values from OIB flights we have to assume that something with the analysis went completely wrong when performing this analysis for the first version of the manuscript. Meanwhile we repeated the analysis and are coming up with a new version of Figure 9 and changed numbers in Tables 5 and 6. Note that we have omitted sea ice thickness values computed for the Fram Strait region for the CryoVEx campaigns. This is motivated by the fact that because we don't have an independent ice thickness estimate as in case of the OIB flight but need to compute sea ice thickness from either ALS or ASIRAS data with snow information from, e.g. the Warren Climatology.

P1532 L3-9: In line with the points made by another reviewer, and by myself, I think that any comparison between RA-2 and CryoVEx data in the Fram Strait that uses the Warren climatology in any way should be left out. This includes the "W99", "AMSRE+ W99" and "KF11" results.

As we have pointed out further above: we are pretty sure that the W99 snow depth data are still valid in the Fram Strait area because of a) its proximity to the upstream area of sea ice from which its sea ice cover originates, b) W99 snow depth data are quite similar between the two regions (north of Fram Strait and Fram Strait area), and c) the ice export takes place on the scale of weeks to 1-2 months which, during winter, does not justify large snow depth changes between the upstream area and the Fram Strait area. We therefore have kept the comparison.

P1535 L25-P1536 L2: I think this is an important point. It should perhaps be put into the previous section where you talk about the sea ice thickness results, to add some balance to the very negative results that you have presented! I think it is almost certain that OIB is much more sensitive to the small-scale range of ice thicknesses – it will pick up pressure ridges where RA-2 will likely not, and will be sensitive to smaller, thinner floes where RA-2 will not.

Agreed. The revised manuscript will contain a corresponding discussion into this direction.

P1336 L10-24: I do not understand what you are trying to demonstrate here. You seem to be calculating the sea ice density from the ice thickness equation, using values of ice thickness that already assume a particular ice density? What is the benefit of doing this? The derived ice densities are not meaningful since they depend on the density assumed to derive the OIB ice thickness.

This was pointed out by reviewer #1 as well. We have considerably shortened this paragraph.

P1538 L18-22: Again, I would emphasize that this over/under estimation of ice thickness does not necessarily mean that either OIB or RA-2 are incorrect. Rather it is probably indicative of the spatiotemporal differences between the two retrieval methods – i.e. OIB being more sensitive to e.g. ridging and smaller, thinner floes, RA-2 measuring over one month.

We agree with the reviewer and will try to formulate this more clearly in the revised version of the manuscript.