

Response to comments of reviewer #1 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:

This manuscript provides an assessment of the quality a RA and RA-2 derived freeboard and thickness data set. The altimeter data set is compared to ice draft from submarines and moorings, freeboard and snow depth from OIB, and snow depth from Warren et al., 1999. There are large differences in these data sets: each data set has limitations and not all of them are understood and there are a lot of issues in the conversion from freeboard to ice thickness. These limitations in available observations is acceptable as long as they are addressed.

There are two key findings: 1) the Warren climatology is not adequate for basin scale usage; and, 2) that ice density could cause significant biases in the estimation of thickness (although this needs to be demonstrated in more detail).

One topic that is missing is a discussion of the limitations/noise of the RA and RA-2 retrievals. With their large footprints, their estimates may be biased due to preferential sampling of areas with lower density of open water (i.e., thicker ice). In fact, that was one of the reasons why the CryoSat-2 design reduced the footprint of the radar by a Doppler processing technique. In any case, it would be important to understand the instrument issues before attribution of the biases to geophysical parameters. Perhaps this is not under the purview of the round robin exercise but certainty this is something that is required in this examination of the uncertainties of thicknesses from radar altimeters.

The reviewer is pointing out an important point. Indeed the title of the manuscript is promising more than we are providing. This was also marked by reviewers #2 and #3. We agree that more work needs to be done to assess the uncertainties in the RA and RA-2 retrievals. This is seemingly a difficult thing to do and most likely is not going to be solved during the first phase of the ESA-CCI sea ice ECV project. Based on this and the recommendations of reviewer #2 we changed the title of the paper 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". We further have changed the content of the manuscript such that it is more clear that we are not yet at a stage where we can say too much about uncertainties from RA and RA-2 data.

The text is somewhat difficult to wade through as there are a lot of details and could be improved. Detailed comments follow.

We have made an effort to both shorten the text and to make it more easy to read.

Detailed Comments:

page-line 1521-9: This is not in the reference list at the end of the manuscript.

A citation of this report has been added to the reference list.

1521-21: Just a clarification - is the starting point for the study here the averaged freeboard in these 2 deg by 0.5 deg grids? Please indicate so.

No. The starting point for the study are the non-averaged freeboards. For example the freeboards to compare with ULS of airborne data are calculated from the non-gridded freeboards. Main use for the gridded freeboards is visualisation in Figure 1 as well as comparison with other satellite data such as the AMSR snow depths.

We have re-written this part now: "... several measurements is required. **RA-2 freeboard estimates obtained along single orbits are averaged according to the co-location areas defined in section 2.2, or into a 2 degree longitude x 0.5 degree latitude grid (approximately 60 km grid cell size). Averaging is always done over one calendar month. Depending ...**"

1521-22: What is the expected uncertainty in altimetric height after the averaging process?

This is much dependent on the number of measurements. The biggest source of uncertainty in a single 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 $10 \text{ cm} / \sqrt{100} = 1 \text{ cm}$. 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 an can lead to substantial biases in obtained altimetric height.

1522-1: Are the freeboards and snow depths both 'monthly' averages within these grid cells?

Yes. Grid cells or other areas we have averaged over (such as for example all of the freeboards and snow depths within a certain radius from transect centre then comparing them to ULS data). This is described in more detail in section 2.2.

1522-12: The Baffin Bay area is outside the domain of validity of the Warren data set even though the polynomials could be evaluated in these regions. This is an incorrect use of the data set and thus the statement should be deleted.

We agree with the reviewer. We deleted the statement starting in 1522-10: "but reveals ... Baffin Bay."

1523-2: Why is a 12 deg by 30 deg (lat/lon) box required? Please add a statement to justify size of box.

We are aware of the fact that this box is quite large and could possibly be reduced to half the size. The rationale behind it was to maximize the number of individual measurements, i.e. reduce speckle noise and to minimize the effect of drift.

We added: "**For BGEP a 12 degree by 30 degree latitude-longitude box is used. This box may be oversized. The rationale behind using such a large co-location area was to maximize the number of valid RA-2 freeboard estimates and to minimize the effect of sea ice motion changing substantially the ice type composition in that area.**"

1524-5: Minimum should read lowest.

Corrected. We also added "**of the magnitude**" in front of 0.05 m as we are not talking about an exact value.

1527-6 & Fig. 4: Again, this is not a fair comparison. The W99 climatology is valid only inside the Arctic, it does not extend to the Fram Strait. If you mean that W99 should not be used outside the Arctic, then OK? But, the data set does not claim and is not expected to be useful in regions outside the Arctic.

We do not fully agree with the reviewer 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 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..."

1527-6 to 20 (Fig. 5): Once more, W99 should not be used in the Canadian Archipelago Please delete all comparisons with W99 in the Canadian Archipelago.
Fig. 5: Isn't there more extensive coverage of the Arctic by OIB than just the data shown here?

We apologize for not having written clearly what we did.

The maps shown in Figure 5 show only OIB data of April 2010; we omitted to show data from March 2010 and also from March 2009 in this figure for better visibility.

We decided to keep the OIB data from the Canadian Archipelago in the map of Figure 5 c), and added a statement in the text.

Yes, there are more OIB data available. However, the strategy in the ESA-CCI sea ice ECV project was to use only a part of the OIB data in the Round Robin Exercise (RRE) to keep some data for the evaluation of the product at a later stage. Similar comments by the other two reviewers point into the same direction. We deliberately only used a sub-set of independent observations in the RRE.

1528-23: Okay, so the authors recognize that the W99 estimates are extrapolated into these the Canadian Archipelago. So, what's the rationale of showing these results? Is there something to be learned by showing comparisons that are not expected to be valid.

We have deleted all aspects of the comparison between W99 snow depth and OIB or AMSR-E from the manuscript.

1529-1: W99 does show the expected interannual variability, that should be quoted.

We are not sure whether we understood the reviewer correctly because we would tend to see the opposite: While OIB gives 0.36 m and 0.23 m, W99 gives 0.35 m and 0.34 m. We made a statement in the text that W99 does NOT capture the interannual variability. We added the sentence: **"It could be that the W99 snow depth climatology does not capture the inter-annual variability in snow depth over MYI in the Arctic Ocean."**

1529-7: Was there a reason why ASIRAS did retrieve the ice-snow interface? Is there a reference one could provide?

We don't understand the comment. ASIRAS, as a Ku-Band radar altimeter is supposed to sense the snow-ice interface (similar to Cryosat-2). This was apparently not the case as we found a very good agreement with ALS (laser scanner) data which senses the snow surface. We did not write that ASIRAS retrieved the ice-snow interface.

We added: **"at the used frequency in Ku-Band according to laboratory experiments (Beaven et al., 1995)"** and further, to account for the fact that this might not be state of the art anymore: **"There is growing evidence that this assumption does not hold for more cases than previously thought (e.g. Ricker et al., 2014)."**

1529-15: So, perhaps the rationale for using the W99 data should be clarified at the outset otherwise it would be very confusing to try to understand why one would attempt such comparisons.

We have responded this comment further up and have also included some clarification in the text further up. We keep Fram Strait W99 snow depth data and we have deleted the part of the sentence starting with "... and that W99 ... 1999)."

Fig. 6: Are these comparisons at all OIB tracks?

These comparisons include OIB data from 2009 and 2010 in the Arctic Ocean. We have added: **"OIB snow freeboard observations are compared with RA-2 snow freeboards computed from RA-2 sea ice freeboard and OIB or W99 snow depth in the Arctic Ocean (Table 3, Figure 6)."** at the beginning of the paragraph on 1529-17

1529-26: I think this is expected as W99 pertains to multiyear ice.

Sure but still worth mentioning.

Table 4: What is BS 1994, BS 1996, BSS 2007, etc.? Please provide legend.

We added: **"See Table 1 for data set acronyms."** in the caption of Table 4.

1530-23: Please show that the area is mostly multiyear ice during these years or provide a reference. The statement, as is, is too qualitative.

Actually, there is evidence that the region of interest was covered by a mixture of FYI and MYI in most winters except 2007/08 (e.g. Kwok, et al., JGR 114, 2009; Swan and Long, TGRS 50, 2013). We took the information whether we have MYI in that region or not from the AMSR-E snow depth data product. We added this information in the text after “in winter 2007/2008”: “(taken from AMSR-E snow depth data set, Cavalieri et al., 2004)”

1531-5: So, perhaps it is attributable to snow or ice densities. But, isn’t it also possible that the RA estimates are biased towards retrieval of higher freeboards?

From our experience with the RA-2 freeboard data we would say no. We rather have problems at the other end, i.e. that we have too much low freeboards because the ocean surface is not referenced properly under the absence of leads.

*Table 5, Table 6 and Fig. 9: pretty discouraging for RA-2.
Fig. 9: Which OIB year is this?*

This was for 2010. However, this is obsolete now as we have changed Figure 9, and Tables 5 & 6. Reviewer #2 pointed out the unrealistic negative sea ice thickness value from RA-2 data in Figure 9. Triggered by this comment we recognized (VERY LATE, we know) that also a varying number of OIB sea ice thickness values is negative. Because the RRDp does not contain any negative OIB sea ice thickness values we have to assume that something fundamentally went wrong when performing this analysis. As the reviewer will note the agreement has not really improved with the new version but at least we are more consistent now with physics. 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.

1532-paragraph starting at 19: Again, it is possible the RA freeboards, because of the large footprints of the radar, are themselves biased (thicker) in areas with thinner or FY ice? Thus, the signal you are seeing could actually be due to freeboard issues rather than density issues.

We are demonstrating how density issues will cause significant uncertainties in thickness regardless of freeboard uncertainty. So yes, disagreement of RA-derived thickness could be due to freeboard uncertainties too, but that is not the point we are making here.

1533-3 to 15: The authors are too eager to attribute the issues to snow densities. I am not entirely convinced.

We are not sure whether the reviewer is referring to the right paragraph here. We take the note that we should equally weight the contributions of the different factors and to not emphasize only one parameter. We will change the manuscript accordingly.

1534-20: *almost all ULS data are acquired under MYI ice? What percentage?*

Based on the AMSR-E snow depth data set, of the BGEP data 98% are of MY ice and of the Tireless data 96% are of MY ice. We note that the actual MYI fraction is possibly smaller because we assigned the 100 km area co-located around a 50 km submarine transect already to MYI if one AMSR-E snow depth grid cell is flagged as MYI.

We did not check the fraction for the two U.S. submarine cruises because AMSR-E snow depth data are not available for the 90ties. But we assume that the fraction of MY ice is as high as for the other two data sets.

1536-3 to 24: *The argument is difficult to follow: there is noise in all these measurements. There is the underlying assumption that the average freeboard within each grid cell provide the true mean, deviations from that due to sample size and biases in RA freeboard could be issues. It would be useful to convince the reader of that before discussion of ice densities.*

This paragraph received comments from the other reviewers as well. We have shortened it substantially and removed the dubious parts.

1536-15: *Should you obtained the same ice density that OIB used? I am confused.*

See comment to 1536-3 to 24

1538-5: *This is over seasonal ice?*

No. We moved the part "This results is ... same direction" up so that it comes behind "...within 0.12 m in 2010."

1539-5: *the length scale issues are important as there could be significant gradients in freeboard within your large grid cells.*

Noted. The revised version of the manuscript will contain a sentence mentioning this issue.

1540-1: *The issues of radar biases that is ice type dependent need to be understood as well.*

Noted. The revised version of the manuscript will contain a sentence mentioning this issue.