

Editor Initial Decision: Reconsider after major revisions (16 Jul 2014) by Julienne Stroeve

Comments to the Author:

Thank you for the revised manuscript submission entitled "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". I appreciate the author's efforts to address the reviewers comments, though in some instances I feel that more could have been done. Below I list several suggested improvements to the paper.

One major comment is that you really need to include a discussion of freeboard uncertainties, a concern raised by all 3 reviewers. These uncertainties will impact your validation of total freeboard and ice draft. I think one thing you could do is clearly state in the introduction you are not focused on validating freeboard, but are taking existing published freeboard data (that are subject to a large range of uncertainties) and looking at the impact of snow depth, snow density and ice density on ice thickness and ice draft.

This is good advice and has now been implemented in the manuscript in the introduction (page3/4) as well as in the data section. Here (Page 4 line 24 to page 5, line 26) we dedicate two paragraphs to describe in more detail the uncertainties involved into the RA sea ice freeboard retrieval and we also demonstrate by a new figure (fig. 2) how quickly the number of valid single freeboard values per 100 km grid cell degrades when going to lower latitudes. We are confident that now the reader will not expect to see an uncertainty estimation of RA freeboard in this paper.

To me the manuscript still requires a lot of streamlining to make the results/discussion easier to follow and make your key points known.

We worked on the discussion and on streamlining the results. In particular we put much less emphasis on repeating results from snow depth inter-comparison and also avoid confusing the reader with the Figure 5 and the results of it written in the former version of the manuscript.

Also, I have to agree somewhat with reviewer 3 that it's questionable how novel this study is as many of the results have been discussed in other studies. As a response you mention that you show the limitations of using a RA freeboard product with a grid resolution of 50 to 100 km for sea ice thickness retrieval.

I don't see that you have clearly shown this, so perhaps you need to focus a discussion on that conclusion.

This comment was particularly helpful as it made us to review once again what we did which we think we have now described in a better way. We agree with reviewer 3 that the novelty of this study is limited and we know that we cannot compete with papers like Ricker et al., Zygmuntowska et al., Kurtz et al., 2014, etc. But again, this was not the aim of this paper which we hope to have stated clearly enough now. We are really looking at this less from the research as more from the pragmatic view point ... questioning how much effort should go into validation of satellite sea ice thickness products if most validation data are based on similar assumptions to convert a sea ice parameter into thickness.

The same with your other novel points you mention in the response. Make these more clear within the manuscript, perhaps by restructuring the manuscript to clearly highlight what is novel.

I also question the need for the snow depth intercomparison as other studies have already looked at W99 vs OIB vs AMSR-2. Is it not possible to refer to those studies and discuss impacts, rather than discussing it again here? What new information are you presenting regarding the accuracy of the W99 data set?

We are testing it as one of the possible snow estimation sources for the RA sea ice thickness retrieval. Except a considerably shortened section 4.1 (old Figure 5 has been removed and Figure 1 has been replaced as well) in which we show our results of the OIB to W99 to AMSR-E snow depth comparison using data collocated on the spatial scale used by RA sea ice thickness retrieval. This is still useful information and is driven by pragmatism as we need to use these products either as snow depth for the freeboard-to-thickness conversion or to “obtain validation data” as no other direct sea ice thickness observations other than drill holes do exist.

It is important information to see that OIB and AMSR-E snow depth data agree over FYI not only in the Arctic Ocean but also in the Canadian Archipelago. This suggests that AMSR-E could be used over FYI in these areas as well which is increasing the potential application area of RA sea ice thickness retrieval beyond the region in which W99 is valid.

It is important to note (as is done on page 15 at the end of Sect. 4) that W99 climatology is not only relying on few data north of Svalbard but also north of Alaska and close to the Canadian Archipelago.

I do find the abstract still is a bit too long and should be shortened to summarize the main points

Abstract is now largely rewritten.

Introduction:

page 2, seems you should make it clear that ERS1/2 RA and Envisat RA-2 do not in fact provide hemispheric coverage of ice thickness, large parts of the Arctic are not sampled.

Added short text to mention the latitude limit.

page 3, line 19, this paragraph seems to pertain to all the different techniques for measuring ice thickness, but obviously laser altimetry and ULS measurements are not based on assumptions of radar wave penetration, perhaps rewrite that part for clarity.

Rephrased to read better.

page 4, line 1-9, I think in here you should somehow make it clear you are not focused on a consistency check of freeboard, I often find the discussion becomes too unclear as to what you are doing.

It is indeed not the best practice to write what we did but are not presenting here. Rephrased to make more sense.

page 4, line 9, I don't follow your reasoning for not using the most recent OIB data.

We saved it for the validation exercise, like we did save all of the ICESat and all of available EM measurements. Rephrased.

page 4, line 14, remove e.g.

Removed.

Section 2.1

page 5, first paragraph, I think in here you should mention the resolution of RA and RA-2.

This is now mentioned.

page 5, line 19, so about 20-150 measurements are used to create the average?

Yes. Rephrased in the text and also included a new figure (Figure 2) which demonstrates the mean number N of single freeboard measurements per grid cell.

page 5 lines 23-30, I do find statements such as "the majority of RA-2 sea ice freeboard values are in a reasonable range". Please be more specific (same with the snow depth statements). What is defined as reasonable?

These indeed are not good scientific practice. We added exact values in the text. Note that the Figure 1 has been replaced by a new one in which we do focus on RA-2 freeboard only and in which we also show the latitudinal limit plus the location of all data used for inter-comparison.

page 5, lines 26-29, reviewers have raised concerns about the use of the W99 data in regions that were extrapolated. I feel that this needs to be more explicitly addressed in the study. Both reviewers 1 and 2 have suggested to remove that discussion. If you are going to keep it in, you need to state here that while you are showing the results, they cannot be trusted.

We now focus on the facts and on those regions where W99 is valid. Old Figure 5 has been removed completely. We note where W99 is valid and where not. We note that indeed the area of the April 2010 OIB flights in the Fram Strait area falls within the region where W99 is valid. We note that areas close to Alaska and the Canadian coast contain as few data as the Fram Strait region.

page 7, lines 21-29, this seems out of place as you do not discuss freeboard uncertainties in the RA/RA-2 data. Since you are not validating sea ice freeboard, I think you need to reframe this discussion slightly to make that point clear.

Removed the sentence about the RA freeboards which indeed was out of place here. Added text to clarify that we are discussing the quality of the OIB product here.

page 10, line 5-6, since the paper is already rather long, you could remove the first two sentences.

We could. And we did.

Figure 4, I don't understand your figures, are you saying that Figure 4a and Figure 4c are AMSR-E snow depths only? Then why say they are for CryoVEx in the caption? Also, I'm surprised you start the discussion with Fram Strait since W99 doesn't have data there. Seems better to start with Figure 5 discussion (moved to Figure 4) and then go back to Fram Strait, together with your responses (revisions) to the reviewers concerns about the Fram Strait results.

In the context of putting less focus on snow depth we have replaced figure 4 by the comparison of sea ice and snow freeboard obtained during the CryoVEx campaigns and removed the old figure 5. Therefore the paper is much more to the point here now.

page 10, line 15-19, I wouldn't say OIB underestimates W99, rather the other way around (i.e. W99 overestimates snow depth compared to OIB).

Yes. It is most likely that it is the OIB which is actually giving us the true snow depth.

page 10, lines 22-28, this belongs with a discussion of the figure about the Fram Strait results not here.

This part has been rewritten anyways and figures have changed.

page 11, line 26-28, Indeed a climatology will not capture any interannual variability, thus one doesn't expect W99 to match with yearly variations in snow depth seen in OIB.

This is true and now is explained in the text.

Table 4, specify over what months these summaries are for. Same with Table 5

We added months to the table.

page 13, line 15, wouldn't you expect a seasonal change in density? So couldn't that be part of the explanation?

That might be possible. This possibility is now mentioned in the text.

page 13, lines 18-19, I don't understand this reasoning here. Are you saying that for the FYI parts when you use the AMSR-2 snow depths, you should also use FYI density for retrieval of RA-2 derived draft?

That is what we are saying. If there are AMSR-E (not AMRS2 btw, we are talking about 2008 here) snow depth estimates, there should be FYI ice around – at least in parts of the averaging area and thus using a MYI density might cause too small drafts. This is now rephrased in the text.

page 13, line 24, should specify here too what ice density data are used.

Does now.

page 13, line 31, should define what is meant by the Arctic Ocean here (i.e. what areas are sampled - this should have gone into the description of the coverage of RA).

We hope that this is more clear now as we either talk about the Arctic Ocean OR the Canadian Archipelago OR the Fram Strait.

page 14, lines 13-16, awkward sentence, rewrite. Also, a limit to this comparison is the W99 snow depth data is an extrapolation.

A lot of this part has been re-written. Extrapolation is now mentioned where needed.

The Discussion section seems to need a bit of reworking. The first part of it goes back to the ULS comparison, which should have been included when presenting those results. The section should really start with the discussion starting on page 15, line 7. Also, please try to better separate out the draft vs OIB comparisons in the discussion.

We moved the first sentences into section 4.3. This required to change the order of the Tables. Table 7 became Table 5 and Tables 5 and 6 became Tables 6 and 7, respectively. We worked on structuring the discussion in a better way and stick to the different parameters investigated first before we talk about some general issues at the end of the discussion.

A paper that should be referenced within this work is the recent one by Zygmuntowska et al., 2014 (TC).

Actually we refer to both TC-papers of Marta now.

page 19, recommendation 1) I'm surprised at the recommendation to use the W99. As the authors are

aware, the Arctic Ocean is now covered by more FYI than MYI, but also that atmospheric conditions are changing and the W99 does not capture interannual variability in precipitation. Thus, while it may be the only thing we have right now, I would think a recommendation towards developing an interannual varying snow depth product would be of more value.

This is true. The original recommendation stems from the CCI project where we could only recommend to use W99 with a pinch of salt because that is the all we have. However a need for a novel product is in place here and we feel that we have now given ample of evidence that W99 might not be the ideal data set to use.

page 19, recommendation 2) I don't feel that you have quantified that the impact of sea ice density is as large as the impact of snow depth. Other studies have suggested this, and I agree, but I think you should try to quantify that in a table or figure. What ice type product do you recommend using?

We agree. We have added a new figure (figure 9) which sheds more light on this topic. Apart from this we are not becoming tired of referring the reader to Figure 7 (old figure 8) which nicely demonstrates the sensitivity of the freeboard-to-thickness and (in this case) the freeboard-to-draft conversion (see new Eq. 2) to the choice of densities by displaying the sea ice draft derived from sea ice freeboard using different sea ice and snow densities.

We now state already in the discussion section that an ice type product should be used and say that this could be QuikSCAT for (most of) the Envisat RA-2 period but that more work is needed to have a consistent ice type product covering the entire RA period 1993 to today.