Response to Review #3 (Robert Ricker)

Date: 20 March 2019

We thank the Editor, reviewers and those who provided short comments on the manuscript for their inputs. The feedback has helped to improve both the clarity and content of the manuscript. We have provided responses to both the short comments and full reviews. We indicated by section, specified by paragraph, where revisions were made within the manuscript text. Since we include figures in response to both the short comments and the full reviews, there is a letter code to indicate if the figure is related to a short comment (e.g. SC1.1) or to a reviewer comment (e.g. RC3.1). The figures in the manuscript itself maintain normal numbering convention (e.g. Figure 1, Figure 2, etc.).

The following is a list of the major changes to the manuscript:

- We have revised the manuscript text for clarity and brevity. In particular we have shortened the Introduction and rearranged the text regarding the treatment of snow depth in each satellite data product.
- Based on input received during the review, we have revised the text of Section 2.1 and the information provided in Table 1 to clarify specific aspects of the processing chain and waveform retrackers used in each satellite thickness product.
- We have extended Table 1 so as to include the details of two additional satellite-derived sea ice thickness data sets, although these data are not included in the further analysis. This decision is a compromise between providing the pertinent details of publicly-available data products, while not overwhelming the reader with too much information in the figures and tables.
- We have replaced the CS2SMOS data set used in the original submission with an updated version of the data set, and revised all figures and tables containing CS2SMOS data.
- We have updated figures and tables wherever possible with new data that has become available since the original submission. In particular, we now include the BGEP ULS ice draft observations for the 2016-2017 season.
- We expanded our results to include winter growth rates, adding a new table (Table 5).
- The reviewers highlighted concerns regarding the original methods used to calculate the correlation between data products, and that using a near-neighbour interpolation with a search radius of 50 km could potentially artificially improve the correlation results. To address these concerns, we have revised the approach to calculate the correlation statistics between the satellite data products, as well as between the satellite and airborne observations. In the revised manuscript the thickness observations are placed onto a common grid (0.4° latitude by 4° longitude) before common grid cells are compared and correlation statistics calculated. This follows the approach originally taken in Laxon et al. (2013) as well as in subsequent studies and allows the reader to place our results in the context of the published literature. We note that this did not change the results of the correlation analyses in a substantial way.

Within the manuscript text, all edits (additions/deletions) are indicated in red font. The manuscript version indicating track changes is posted as a separate author's comment in order to keep the response document concise.

Review of Assessment of Contemporary Satellite Sea Ice Thickness Products for Arctic Sea Ice by Sallila et al.

Summary

The paper "Assessment of Contemporary Satellite Sea Ice Thickness Products for Arctic Sea Ice" evaluates and compares 6 different sea ice thickness data products that are publicly available, including data from CryoSat-2 (CS2), SMOS and the Advanced Very-High-Resolution Radiometer (AVHRR). For the evaluation, they use independent ice draft and thickness measurements obtained from the Beaufort Gyre Exploration Project (BGEP) upward looking sonars (ULS) and Operation IceBridge. The authors find that products utilizing CS2-only measurements are reliable for sea ice thickness between ~0.5 m and 4 m, while the merged CS2-SMOS product was the most reliable for thin ice. In contrast to the other products, the AVHRR dataset does not seem to represent a reliable ice thickness distribution at the end of the winter season.

I think this study is potentially very useful for the user community of those sea ice thickness data sets, since so far, such a review of current sea-ice thickness data sets is not existing to my knowledge. It is well written, has a clear structure and generally is easy to follow. It presents useful information bundled in one paper. Table 1, for example, is great as an overview. However, I also find that the paper lacks some crucial information regarding the products and the way the data are analyzed. I also have the feeling that there are some inconsistencies in the data analysis. My 3 major concerns are:

RC 3.1. I find that the description of the sea-ice thickness data sets in section 2.1 is incomplete. The AWI and the CPOM processing is quite different in some aspects, e.g. retracking of ice and lead waveforms. I think this should be described in more detail. See therefore also my comments below.

Thank you for pointing out that the AWI and the CPOM processing is quite different. Text in Sections 2.1.1 and 2.1.2 has been substantially revised to present additional details of the CPOM and AWI processing chain. We have also revised Table 1 to clarify the details of the CPOM retrackers.

RC 3.2. I am not really sure about the way the original data are re-gridded for the comparison. The paper lacks motivation regarding the chosen values for the re-gridding resolution of 5 km and the search radius of 50 km. I am also not sure if this is really the best way to do it, see detailed comments below. In any case, I suggest to revise section 3.1 in order to motivate your chosen method.

Thank you for your comment on gridding which is consistent with a comment from Reviewer 1 (see RC1.1). We have changed the gridding approach used to calculate correlation between data products and we have clarified this in the methods, Section 3.1. We have also revised all text where the results of the gridded data are discussed. Please see further details in our response to RC1.1.

RC 3.3. It also seems strange that CS2SMOS underestimates ice thickness in the MYI zone compared to the AWI product. CS2SMOS uses the AWI product for merging with SMOS, but in MYI areas, SMOS should not have a significant impact. See also my detailed comments below.

See our response to SC1. We discovered that there was an issue related to data used in the first version of our manuscript, where some corrupt data files were used accidentally. The results for CS2SMOS have changed now that we are using the correct data product files, as shown in Figure RC3.2 below. In agreement with your comment, the thickness of the MYI region in the CS2SMOS data product now resembles that of the AWI CryoSat-2 product.

Detailed Comments

P1 L18-19: "... are reliable for sea ice [thickness] between ~0.5 m and 4 m" - it should be mentioned that you are talking about thickness.

Added "thickness" on page 1, Abstract.

P2 L26: "One of the most widely used thickness data sets derives from the radar altimeter flown on CryoSat-2 ..." - this sentence sounds a bit odd to me.

Altered this text.

P4 L11-12: Please mention the version of the CS2SMOS weekly ice thickness product.

To avoid being repetitive and keeping the introductory paragraph of 2.1 compact, we decided to include the details of the product versions in the product descriptions that appear in the subsections. Specifically, please see Section 2.1.5, where we added the version number (v1.3) for the CS2SMOS data product.

P5 L8-9: I don't think that the CPOM processor uses the TFMRA algorithm (AWI). They also use a different retracking method for lead returns: a gaussian function is fitted to the waveform, as far as I know. See therefore, Tilling et al. (2017) - "Estimating Arctic sea ice thickness and volume using CryoSat-2 radar altimeter data".

Thank you for pointing this out. This has been corrected in Section 2.1.1. and also in Table 1.

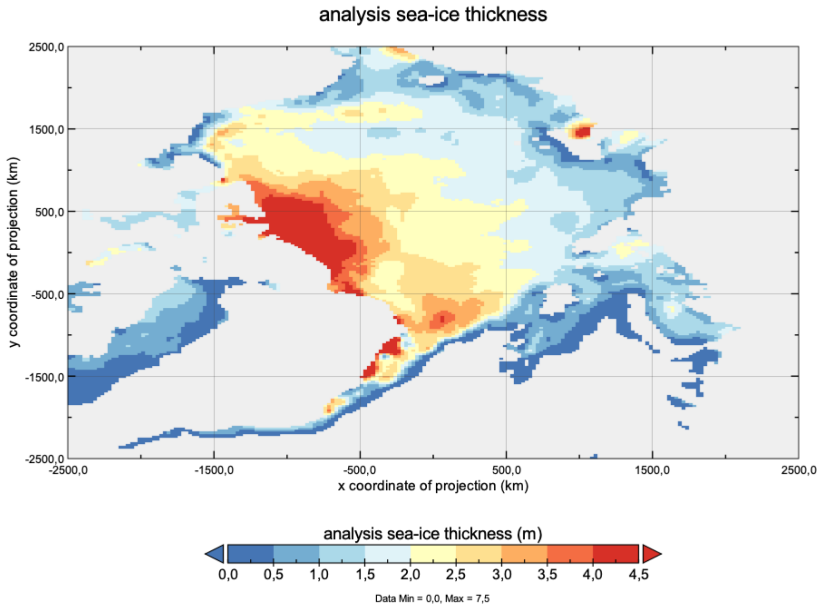

Figure RC3.1: CS2SMOS Analysis Thickness from 20150323-20150329, version 1.3.

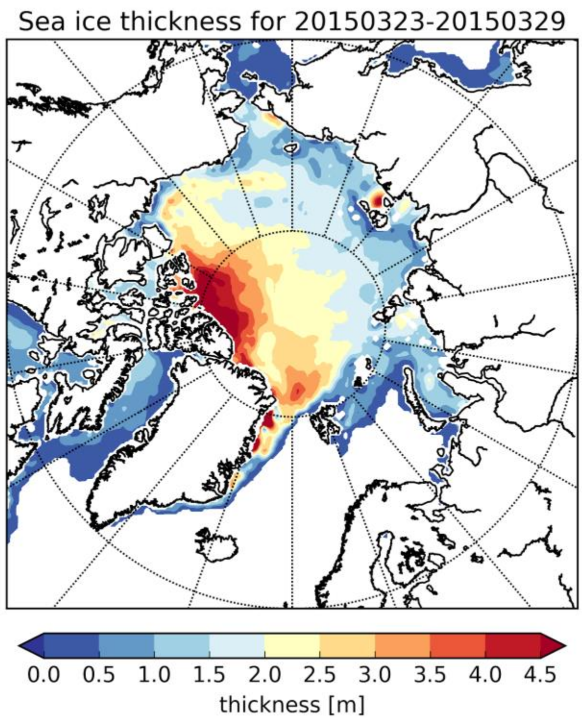

Figure RC3.2: Response to comment RC 3.3 and Figure RC3.1: Revised mapping of sea ice thickness from the CS2SMOS data product, version 1.3, for the week 20150323-20150329. The revised map is consistent with Figure RC3.1 and illustrates that data in the files we are now using in the revised manuscript are consistent with those from the data provider.

P5 L18: Please provide the version number of the AWI product. Is it 1.2?

The version of the AWI data product used in our manuscript is 2.0. This detail was added to Section 2.1.2.

P5 L18-28: Here it should be mentioned that the TFMRA is used. Moreover, I do not agree with the statement "The algorithm employed by AWI does not differ significantly from that used in the derivation of the CPOM product". While I agree that there are some similarities in the processing and also the derived ice thickness distributions look similar, some significant differences exist. For example, AWI uses the same retracking for lead and sea ice echoes, while CPOM does not (see above). Moreover, the surface type classification is different. CPOM also applies a retracker bias correction (Tilling et al. (2017).

Thank you for this clarification and please also see our response to RC3.1. We have substantially revised the data product descriptions in Section 2 and we now include details of the TFMRA retracking algorithm in the second paragraph of Section 2.1.2.

P7 L16: What is the reason to include the APP-x data here? They do not seem to represent the entire Arctic sea ice thickness distribution and ice growth during the season (see Figure 2 a,b). Then, one could argue to also include the SMOS sea ice thickness product.

Regarding the APP-x data product, please see our response to RC2.4.

Regarding the SMOS-only sea ice thickness data product: While we agree that the SMOS data product differs to the ones presented in the manuscript, it is only reliable for studies of thin ice <1 m thick (Ricker et al., The Cryosphere, 11, 1607–1623, 2017). Thus it is not representative of overall ice thickness in regions 1-6 of the Arctic, which is the focus area of our study. Also, for the sake of readability we limit the comparison to six data products. We believe that by including the CS2SMOS data set in our analysis, which differs from the CS2-only products and highlights the most relevant aspects of the SMOS observations, it provides the most useful comparison and insight for the reader.

P9 L9-11: Why do you choose a 5 km grid and a 50 km search radius? What happens at the ice edges or at the coasts. For example, areas, where the original grid contains NaN's in case of open water. Does this mean that you obtain ice thickness estimates on your new grid, where there were NaN's before, if valid ice thickness grid cells are found within the 50 km search radius? Wouldn't that erroneously enlarge the ice area? If there are more than one grid cell found within the search radius, are you only considering the closest one? Or is there some weighting applied?

We agree with the Reviewer that the gridding approach used in the original version of the manuscript (a 5 km grid with a 50 km search radius) was not appropriate for comparing the data sets against each other. We examined the results of the original gridding approach and found issues similar to those you describe, in particular in the Central Arctic region (at the northern limits of region 1), and also around the ice edge. We have therefore changed our approach and now follow the method originally introduced in Laxon et al. (2013), and used again in Tilling et al. (2018), to conduct the product vs. product comparisons and to calculate the correlations (Figures 4 and 9). In the revised manuscript we use a 0.4° latitude by 4°

longitude grid. We have revised the relevant areas of the manuscript text (in particular, Section 3) to clarify this change. Please also see our response to RC1.1.

P10 L3: Why do you choose such a large radius (200 km)? Especially in the Beaufort Sea, where those moorings are deployed, you may have very mixed ice regimes with some chunks of MYI surrounded by FYI. Wouldn't it be better to only choose the nearest one or at least a smaller radius?

We used a radius of 200 km, since it is consistent with the original evaluation presented in Laxon et al. (2013) and subsequently in Kwok and Cunningham (2015). This allows the reader to place our results in the context of the conclusions of prior studies.

P10 L16: Why are the IceBridge data interpolated on a 50 km grid? Why not 5 km as the re-gridded satellite products?

We have revised our methodology to calculate the correlations between the satellite data products and the IceBridge data. In order to place our results in the context of the existing literature, we have adopted the same approach as that of Laxon et al. (2013), who used a 0.4° latitude by 4° longitude grid to compare IceBridge and CryoSat-2 thickness estimates. This approach accounts for the uneven spatial and temporal sampling of the sea ice along the IceBridge flight-lines compared to the monthly means obtained from the satellite data products.

P11 L15-16: I am a bit confused about the CS2SMOS ice thickness maps. Here, it seems that they consequently underestimate ice thickness. Especially in MYI areas, the thickness should be very similar to the AWI CS-2 Product, since this is used for the data merging, and SMOS data should have almost no impact in the MYI zones. When I plot the CS2SMOS thickness for the week 2015-03-23 - 2015-03-29 (see Figure R1 (now Figure RC3.1), data from Meereisportal, version 1.3), using a similar color scale with the switch from light blue to yellow at 2 m, the MYI tail in the Beaufort Sea appears yellow, indicating thickness above 2 m. In your map it seems to be below 2 m. Of course, your map shows the average from March - April, but I would assume that this is not much different.

Your confusion is well justified! Based on your remarks, and comment SC1, we re-examined the CS2SMOS data and found that the CS2SMOS dataset we had used in the original manuscript was incomplete, and some corrupted data files were accidentally used. For this revision, we have corrected the issue. We downloaded all of the CS2SMOS version 1.3 data from Meereisportal again, and we can confirm that the thickness data are now consistent with that shown in Figure RC3.1 (see Figure RC3.2 above, and Figure 2 in the manuscript).

Figure 2: I really would recommend to use a different color scale with a linear color gradient, starting at 0.0 m. Moreover I would also suggest to use a finer resolution, e.g. 0.25 m. 0.5 m is too coarse from my point of view.

We revised the colour bars used in Figures 2 and 3, to start at 0 m, and we changed the increment from 0.5 m to 0.25 m according to this suggestion.

P12 L12-14: As mentioned above, it seems a bit strange that CS2SMOS is significantly thinner than AWI CS-2 in the MYI regions, since it uses the AWI CS-2 data, while the SMOS ice thickness estimates are not valid in MYI areas.

We have corrected this. See response to SC1 and RC3.3.

P17 L2-3: See above.

We have corrected this. See response to SC1.

Figures 5: The CS2SMOS data seem very noisy and show some strange behavior, e.g. strong decreases in mean ice thickness in some months. This should be checked. It does not seem to occur in Ricker et al (2017): “Satellite-observed drop of Arctic sea ice growth in winter 2015–2016”. Although they show sea-ice volume, I would expect similar behavior.

We have corrected this. See response to SC1.

Figure 5 and 6: Over which area are these averages calculated? Regions 1-6 as indicated in Figure 1?

Yes, the averages are calculated for the regions 1-6 as defined in Figure 1. While we had indicted this in the figure caption for Figure 6, we had omitted the information in the caption for Figure 5. We have thus modified the text of the figure caption for Figure 5.

Figure 6: “a)” and “b)” are missing in the figure.

We have added the pertinent letters to Figure 6.