

Point-by-point response to editor and reviewer concerns by
corresponding author: H. Jakob Belter
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tc-2019-307:

Satellite-based sea ice thickness changes in the Laptev Sea from 2002 to 2017: Comparison to mooring observations

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Belter, H. Jakob, Thomas Krumpfen, Stefan Hendricks, Jens A. Hoelemann, Markus A. Janout, Robert Ricker, Christian Haas

Dear anonymous Reviewer #3,

on behalf of all authors, I would like to thank you for your comments and suggestions to our manuscript. Please find our point-by-point response to your review below. We hope you agree with our changes and feel that your comments have been answered properly. We would also like to refer you to the responses to the other reviewers for more improvements and changes to the manuscript

Specific comments:

- (1) *Line 12: This phrase does not fully correspond to the results presented in the paper. Overestimation (underestimation) of sea ice draft for thin ice below 0.7 m (thick ice above 1.3 m) is indicated from comparison of the mean values, but not with respect to the modal draft.*

-Response:

You are right, this sentence was a little misleading we revised it accordingly.

-Changes:

LINES 12-14

- (2) *Line 40: Authors could also note that in (Kern et al., 2018) the airborne Operational Ice Bridge data were used for validation of the satellite product as well.*

-Response:

Thank you, we added Operation IceBridge to the list of observational data sets that have been used for the validation of the CCI-2 SIT products.

-Changes:

LINES 43-44

- (3) *Section 2.1: I guess that open water was excluded from the sonar-based measurements. If so, please, mention it in the text.*

-Response:

We added a paragraph on the VAL data averaging and a sentence clarifying that open water values were not included in the calculations of averages.

-Changes:

LINES 121-124

- (4) *Line 101: The phrase 'bottom track mode measurements of surface and error velocity' sounds not clear. Although paper by Belter et al. (2019b) will, I guess, describe details of the methodology, some clarifications on what is, e.g., 'error velocity' would be helpful.*

-Response:

You are quite right that this description is not sufficient. We therefore added a couple of explaining sentences and the reference to the ADCP sea ice draft data on the PANGAEA data archive where a short summary of the processing steps is provided.

-Changes:

Also in response to Reviewer #1, did we add further explanations to the respective section (**LINES 112-116**). Furthermore, did we publish a short summary of the processing steps with the ADCP sea ice draft data sets on PANGAEA (<https://doi.pangaea.de/10.1594/PANGAEA.912927>).

- (5) Line 125: *This way of estimating draft uncertainty is applicable if SIT uncertainty accounts for the sources of freeboard uncertainty. If so, please, mention it in the text. From the other side, since the authors do not use this draft uncertainty further in the analysis, it is not clear what it was estimated for.*

-Response:

Thank you for bringing this up. As mentioned in the response to Reviewer#1 the presented draft uncertainty is more complex than what we presented. Following your final comment regarding this issue we removed the draft uncertainty estimation completely and also removed the uncertainty bars from Fig. 7. Since uncertainty is still a very important parameter for the analysis we added information about satellite SIT uncertainty in the respective 'Data and methods sections'.

-Changes:

Uncertainty equation and uncertainty bars in Fig. 7 have been removed. Information about uncertainties have been added to the respective 'Data and methods' subsections.

- (6) Line 127: *It is not clear how authors calculate weighted mean values. Possibly this weighting account for the distance between grid center and mooring location? If so, please clarify it.*

-Response:

You are completely right, weighted means account for the distances of the selected satellite data points to the mooring location.

-Changes:

We clarified that in the text (**LINES 162-171**).

- (7) Line 133: *As a frequency of the orbit tracks that pass over the mooring sites the authors specify 'four overflights'. However Envisat and Cryosat-2 have different orbit inclinations and this frequency should be different for these missions.*

-Response:

You are right the number of overflights is different for ENVISAT and CS2. In fact, the number of overflights is occasionally below and some times above four for individual months. We were not accurate enough in the commented sentence. We clarified that the number of overflights is different for the two satellites and also changed 'approximately' to 'about'. On average the number of overflights is even more than four for both satellites. However, the point of the sentence was not to provide a fixed number of overflights per month (which does not exist since it varies) but to indicate that in any case orbit data provides more data points for the comparison to VAL data than the monthly mean gridded CCI-2 data. We hope you agree with the changes

we made to clarify the sentence.

-Changes:

Sentence was changed (**LINES 146-150**).

- (8) *Section 3: I suggest the authors to change structure of this section: to combine sections 3.2 and 3.3 in one section 3.2 with the title, for example, 'Validation of CCI-2 products', and with the subsections '3.2.1 Gridded CCI-2 sea ice draft' (currently section 3.2), '3.2.2 Orbit CCI-2 sea ice draft' (currently section 3.3.1), and '3.3.3 Intercomparison of CCI-2 and merged CS2SMOS drafts' (currently section 3.3.2). Then the accordingly revised text from the first paragraph of the current section 3.3 could be moved to the beginning of new section 3.2*

-Response:

Thank you for this suggestion. Reviewer #1 suggested changes to the titles of this section and we agree that your suggested structure improves the readability of this section. However, we were a little confused by your suggestion to move the first paragraph of the old 3.3 section to the beginning of the new 3.2 section. Since 3.2 is the overarching section for the comparison between all satellite and VAL data we moved the paragraph in question to the end of the new 3.2.1 section. We feel that it fits here since we finish the results part of the monthly mean gridded data and show the reader that we are moving on to the higher temporal resolution products here. We hope you agree with this change. However, calling the last section 'Intercomparison...' is a really good suggestion since the CS2SMOS/VAL comparison is rather short and the paragraph focusses on the results of all presented satellite products.

-Changes:

Changes were made to the structuring of the 'Results' section, including new titles.

- (9) *Line 228: I guess that this enhanced underestimation of thick ice by CS2SMOS data is observed because for some bins corresponding to thick ice the CS2SMOS product is the only available data (as I can see from Figure 5). It means that for these bins CS2SMOS product is generated only from the SMOS measurements. If so, this could be explained in the text.*

-Response:

This is an interesting observation but you have to remember that Fig. 5 shows satellite data from products with different temporal resolutions. Gridded ENVISAT/CS2 data (filled circles) is based on the initial orbit data (empty circles) and CS2SMOS is based on the gridded CS2 and SMOS data. 'Missing' thicker CS2 data could also be caused by missing VAL (when there is no VAL data point available there is no comparison), for example due to a long open water periods that prevented the generation of a monthly mean VAL sea

ice draft value (see new paragraph on open water influence and calculation of VAL daily, weekly and monthly averages, end of section 2.1). However, we agree that reason for this increased underestimation is the influence of SMOS data on the merged product and this could very well be caused by a total lack of CS2 data, resulting in SMOS data being the only product defining an individual data point. In response to a comment from Reviewer #1 we revised the sentence about the SMOS influence to show that the larger impact of SMOS leads to the increased underestimation. We hope that this answers your comment as well.

-Changes:

Additional paragraph at the end of section 2.1 (**LINES 121-124**) and changes to the sentence in the 'Results' section (**LINES 271-272**).

- (10) Line 285: *The reference (Paul et al., 2018) is not appropriate here. Paul et al., 2018 do not provide regional estimates of the differences between SIT derived from ENVISAT and CS2 data.*

-Response:

You are right, Paul et al., 2018 provide maps showing the differences between ENVISAT and CS2 freeboard. The value we are presenting here has been calculated from the available SIT data sets.

-Changes:

Reference to Paul et al., 2018 was removed from the sentence (**LINE 197**). Please note that the entire 'Data limitations' subsection was moved to the 'Data and methods' section (following a comment from Reviewer #2).

- (11) Line 315: *The indicated trends are small, that supports the conclusion that the gridded CCI-2 CDR is stable over considered period. However Fig.6 shows that these trends might be caused not only by the intermission differences. The trends for thickness ranges 0 to 1 m and 1 to 2 m looks negative even separately for Envisat and CS2 data as well as for combined dataset. For thickness range 2 to 3 m two overlapping points in 2011 shows that Envisat rather overestimate sea ice draft as compared to CS2 as well as for thinner ice.*

-Response:

First of all, we agree with your assessment that these trends are small and support the conclusion that the gridded CCI-2 CDR is stable over the considered time period (we mention that in the text as well). You are also right that these trends seem to be there even for each of the two satellite periods individually, however, since the number of data points available for each thickness range is already very small over the full period, looking at both satellite periods separately would decrease this number even further and make those trends even more uncertain. Furthermore, should we consider that these val-

ues are not recorded at the same location, as we see from differences in the agreement between SAT and VAL data in other Arctic regions. This could be simply caused by regional differences in the performance of the satellite products. The reason why we attribute those trends for the three thickness ranges to the inter-mission bias is that fact that it agrees rather well with what Paul et al., (2018) found. The inter-mission bias seems to be dependent on thickness. ENVISAT overestimates (underestimates) thin (thick) ice compared to CS2. For the first two thickness ranges we would expect ENVISAT-VAL differences to be larger compared to CS2-VAL differences. This is the case for the thickness ranges from 0 to 1 and 1 to 2 m (as also suggested by the trend lines). However, we agree that the statement made for the 2 to 3 m thickness range might be a little strong. We revised the sentence to indicate that the small number of data points (even smaller than for the other two thickness ranges) and the underestimation by the satellites that strongly increases within the 2 to 3 m thickness range make this trend rather uncertain and we therefore call it inconclusive. We hope you agree with this change.

-Changes:

Revised sentence (**LINES 327-332**).

- (12) *Line 380: It can be noted here that not only snow depth, but specifically snow properties that influence the location of the main scattering horizon are a major source for uncertainty in the freeboard retrieval process.*

-Response:

Thank you for this important addition. We added snow properties to the sentence in question.

-Changes:

LINE 407

Technical comments:

- (1) *Line 79: I suggest to replace 'approaches' by 'instruments', otherwise one may interpret it that both ADCP and ULS data are processed by two methods.*

-Response:

-Changes:

Corrected.

- (2) *Line 101: Abbreviation 'BT' is not needed here as it is not used further in the text.*

-Response:

-Changes:
Corrected.

- (3) *Figure 2: Colours of the first and second lines indicating trend values should be switched*

-Response:

-Changes:
Corrected.

- (4) *Line 231: I suggest to reformulate this sentence: 'While individual stations deviate from this average the overall tendency indicates a dependency of the agreement between monthly mean gridded CCI-2 and VAL sea ice draft on sea ice thickness.'*

-Response:

Thank you for pointing this sentence out. It is a little confusing.

-Changes:

We changed the entire sentence (**LINE 275-278**).

- (5) *Line 332: In the 'newly formed FYI ice' the word 'ice' is not needed.*

-Response:

-Changes:
Corrected.

- (6) *Line 383: The sentence 'Furthermore...' sounds not clear. Please, consider revising.*

-Response:

-Changes:

Sentence has been revised (**LINE 410-411**).

- (7) *Table 4: In the captions to the Table it is noted that the statistical parameters 'were calculated for the four VAL data sets'. However this table presents the results only for two stations with ULS measurements: Taymyr and 1893.*

-Response:

We are looking at Taymyr and 1893 data from the 2013/2014 and 2014/2015 periods which results in a total of four different data sets.

-Changes:

We clarified this in the Tab. 4 caption.

Additional changes from the authors

- (1) *Due to changes in the review process of the ADCP sea ice draft derivation method paper (previously Belter et al., 2019b, now Belter et al., 2020b, in review at the Journal of Atmospheric and Oceanic Technology) the estimated uncertainty values provided for the daily mean sea ice draft time series have been changed. See changes in **LINE 116-117 and LINE 179-181**.*
- (2) *Daily mean sea ice draft time series from the Laptev Sea ADCPs have been published and a reference was added to the 'Data availability' section (**LINE 415**).*

In the end we want to thank you again. We really appreciate your input and hope you agree that the manuscript has improved.

Kind regards,

H. Jakob Belter