

Responses to Reviewer #2 (Anonymous)

We thank the reviewer for the thorough review of our manuscript and for the valuable comments and suggestions. We have carefully addressed each point and believe that the revisions have strengthened the manuscript. Our responses are provided below, in [blue](#) below each comment in black. When relevant, we have added quotes from the manuscript in [orange](#).

General comments

The introduction has a number of repetitive statements. For example: measurements need to be aggregated for full Arctic wide coverage, passive microwave radiometers are limited to below 1 m thickness, etc.

[We thank the reviewer for this comment. We have revised the Introduction to remove repetitive statements and improve the overall clarity and conciseness of the text.](#)

The introduction and title should mention this is just winter sea ice thickness studies.

[We fully agree with this suggestion. The Introduction has been revised to clearly state that the study focuses on winter sea-ice thickness. We did not modify the title, as we believe it remains sufficiently representative of the scope of the study.](#)

There are a lot of acronyms used to describe datasets in Section 2 that should be defined.

[We thank the reviewer for this relevant remark. Definitions have been added for all dataset acronyms used in Section 2 to improve readability.](#)

The reference data is referred to as the ULS and the BGEP data interchangeably. Choose one and stick to it. Typically it is ULS in text, but in figures and captions it is BGEP.

[We have revised the terminology throughout the manuscript to improve consistency. "BGEP" is now used when referring to the project or the associated dataset, while "ULS" is retained where the discussion specifically concerns the measurement technique.](#)

The pairwise comparison (Section 3.2) lacks motivation and could include more discussion. Why was this conducted? Can you elaborate on what this says about the regional variability and previous section about comparison with the reference data. For example if two products are biased high versus the reference data how do they compare Arctic wide with each other? And why?

The motivations indeed need to be specified: The objective of the pairwise comparison is not to assess regional variability, but to evaluate the consistency and differences among the various sea-ice thickness products and retrieval techniques. Similar to the spread of climate model ensembles, the dispersion among these products provides an estimate of the uncertainty associated with sea-ice thickness estimates. A sentence has been added to clarify this objective in the manuscript.

In the comparison of the large scale datasets, you state that the largest disagreements between products is in the Beaufort Sea. What does this mean for your comparison with the ULS data? When you compare products to each other across that region, how can you relate this to the comparison with the reference data?

The explanation is indeed not that straightforward. As explained in the manuscript (l. 302), the Beaufort Gyre is characterised by the transport of multi-year ice that mixes with younger ice, resulting in strong spatial variability in sea-ice thickness. Accurately representing these transition zones remains challenging for both observational products and models, leading to larger discrepancies and higher standard deviations in this region, including against BGEP. We have revised the corresponding paragraph to make this explanation clearer.

Check misuse of the word “would”.

We have reviewed all occurrences of the word *would* throughout the manuscript and corrected them where appropriate.

We would like to thank the reviewers for the quality and professionalism of their comments. Many of these comments emphasize that, in order to quantify sea ice trends, it would be necessary to perform validations using other reference measurements than BGEP only (other moorings, airborne measurements), to study the uncertainties, and to correct anomalies for seasonal variability.

We fully agree and are taking these points into account for future developments of SINXS data and tools (some of which have already been implemented or are currently being implemented).

However, the objective of this paper was not to characterize sea ice trends themselves, but to characterize SINXS products. This ambiguity is our fault, as we have on several occasions referred to "sea ice trends" rather than "trends in sea ice products". We have therefore corrected all such ambiguities to ensure that we are referring specifically to the products.

Specific comments

Line 10-11: Confusing wording. Consider “deviation from” instead of “difference towards”.

We thank the reviewer for this suggestion. The wording has been revised by replacing "difference towards" with "deviation from" to improve clarity.

Line 31: what is “good” accuracy.

We agree that this characterization was subjective and unnecessary, and it has therefore been removed.

Line 35: you have already said “south of 81.5 N” no need to repeat with until 81.5 N.

We thank the reviewer for this comment. The redundant wording has been removed.

Line 35: “until x N” is a strange phrase. “up to” or “south of” would be better. (maybe a personal preference)

We thank the reviewer for this suggestion. The wording has been revised accordingly.

Line 56: “discrepancies between single products” is a strange statement. What do you mean between single products? The word between suggests multiple products.

We appreciate the reviewer's comment. We agree that the wording was unclear and have revised the sentence for clarity.

Line 104: What do you mean “more model output would be available from the community”.

We thank the reviewer for this comment. The sentence referred to the expectation that additional model outputs may become available in the future. However, as this statement applies equally to all product categories, we have removed it from the manuscript.

Line 105: spell out CFS acronym.

We thank the reviewer for this remark. The acronym has now been defined at its first occurrence.

Line 142: “such as those from young sea ice”- consider including “and leads”.

We appreciate the reviewer's suggestion. "And leads" has been added to the text as suggested.

Line 187: This is the first time you have defined the EASE2 acronym but it has been used throughout the description of the datasets before.

We thank the reviewer for pointing this out. The acronym is now defined at its first occurrence in the manuscript.

Figure 1 caption. Instead of left to right note (a), (b), and (c).

We thank the reviewer for this suggestion. The figure caption has been revised accordingly.

Line 215: Discussion of the lack of multi-product data in Figure 1b. This is confusing can you elaborate on the “design” of the ESA SMOS product that would show thinner ice.

Also can you discuss more about the bias correction in the TOPAZ model that would bias these data.

We appreciate the reviewer's insightful comment. We have expanded the discussion to clarify that passive microwave radiometers and radar altimeters are complementary measurement techniques. Radar altimeters have limited sensitivity to very small freeboards, whereas passive microwave radiometers are specifically designed to retrieve thin sea-ice thickness. Consequently, the inclusion of SMOS increases the representation of thin ice. An additional paragraph has been added to the section describing passive microwave radiometer products to explain this behaviour.

Figure 1a. In the altimetry section you mentioned altimeter accuracy is greater for thicker sea ice. Here it shows greater biases for thicker ice. Can you discuss this here?

We thank the reviewer for this comment. We have clarified that the limitation of radar altimetry concerns its reduced sensitivity to very small freeboards (typically below 1–2 cm), rather than its performance over thicker ice. The larger biases observed for thicker ice are primarily related to increased surface roughness, and this distinction is now explained more clearly in the manuscript.

Line 222: instead of saying “most products” and “some models” name specific products that have the highest and lowest bias and suggest why this may be the case. Can you indicate in Figure 2 which products use the same assumptions for freeboard to thickness estimates as the reference data and discuss this in the text to determine if this actually the reason that some product perform better than other against the reference data.

We appreciate the reviewer's suggestion. While such an analysis would indeed be informative, it is currently not possible because the assumptions used for freeboard-to-thickness conversion are not fully documented for all products. These retrievals depend on several factors, including the snow thickness product as well as the ice and snow density assumptions. We have therefore not expanded this comparison, but we intend to investigate the impact of using common snow thickness products and density assumptions in future work.

Line 260: Why do you compare products to each other? Is this just so that you can look at a larger region? Please describe motivation?

We thank the reviewer for raising this important point. As explained in our response to General Comment 5, we have expanded the manuscript to better describe the motivation for the pairwise comparison and its role in evaluating the consistency and uncertainty among sea-ice thickness products.

Lines 275 to 281: this discussion seems out of place and maybe belongs in the outlook section.

We appreciate the reviewer's suggestion. While we agree that the implications are further developed in the Outlook section, we believe this discussion is also relevant here because it directly relates to the differences highlighted by the pairwise comparison. The manuscript has been revised to improve the flow while retaining this discussion in its current section.

Line 283: RMSD units of meters (?).

We thank the reviewer for pointing this out. The units of metres have been added.

Line 313: What does that mean “snow thickness increases over multi-year ice and decreases elsewhere” from the Lee et al. study. That statement does not make sense.

We appreciate the reviewer's observation. We agree that the original wording was unclear and have rephrased the sentence to more accurately describe the findings of the study.

Line 318: trends in what months Do you look at the monthly data for each month from November through March?

We thank the reviewer for this comment. We have clarified that the trend analysis was performed separately for November and March, resulting in one time series for each month.

Line 351: Explain how less export of sea ice through the Fram Strait affects the average sea ice thickness.

We appreciate the reviewer's comment. We have expanded the discussion to explain that a reduction in the export of multi-year ice through Fram Strait allows older ice to

remain in the Arctic for longer, enabling it to continue thickening through both thermodynamic growth and dynamic processes. This relationship is now described more explicitly.

Line 354: I don't think this is consistent with the IPCC statement. The IPCC statement and consideration of "ice-free" is based on extent not thickness and is thus not a relevant statement in this section or paper. Unless you can describe it differently?

We thank the reviewer for this comment. We agree that the IPCC definition of an "ice-free" Arctic is based on sea-ice extent rather than thickness. We have therefore clarified the text to explain the relationship between sea-ice thinning and the reduced likelihood of sea ice surviving the summer melt season, which ultimately contributes to the decline in sea-ice extent.

Line 363: "end of freezing season" – just say March? Or define the freezing season.

We appreciate the reviewer's suggestion. The text has been revised to refer directly to March.

Line 380: this sentence does not make sense: what do you mean "most of the sea ice thickness products also include the snow thickness used."

We thank the reviewer for this comment. We have clarified that many sea-ice thickness products also provide the variables used in the retrieval process, such as the primarily measured quantity (e.g. freeboard for altimetry-based products) together with the snow thickness information used to derive sea-ice thickness.

Technical corrections

Line 21: Inconsistency with hyphenation of sea ice throughout introduction.

We thank the reviewer for pointing this out. The hyphenation has been made consistent throughout the Introduction.

Line 36: ICESat-2 is Ice, Cloud, and Land Elevation Satellite.

We appreciate the reviewer's observation. The full name of ICESat-2 has been added at its first occurrence.

Line 194: include the data availability statement in the backmatter data availability section instead of in the main manuscript.

We thank the reviewer for this suggestion. The data availability statement has been moved from the main text to Appendix B.

Line 220: the sentence ends in a colon.

We thank the reviewer for pointing this out. This typographical error has been corrected.

Line 223: model → models.

We appreciate the reviewer's careful reading. This has been corrected.

Line 293: two periods at the end of the sentence.

We thank the reviewer for identifying this typographical error. The extra period has been removed.