

Title: High-resolution Antarctic sea-ice thickness and snow depth from drift-aware multi-mission altimetry

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5 Reviewer 2 - global comment

This paper proposes a sea ice thickness (SIT) and snow depth (SD) product for the Antarctic derived from multi-mission altimetry. Corrections are applied to account for the sea ice drift over the month-long windows in which parcels of sea ice may appear in co-located satellite tracks.

10 The paper is clear and easy to follow, and the research is relevant and timely. The inputs and methodology are stated in good detail. There are significant gaps in the literature review and with the treatment of assumptions, particularly around Ku-band scattering within the snowpack in Antarctica (which are stated but subsequently ignored).

15 A broad evaluation of the “drift-aware” product is attempted, including exploring pan-Antarctic spatial patterns, intercomparisons with in situ data, and considering differences with non-drift-corrected products. However, quantitative assessment to justify the “drift-aware” product is missing. Some supporting interpretations of the figures do not accurately represent the findings and require revision. Further validation is needed.

The discussion needs a complete revision. Inclusion of references which place the study within the current literature is missing and limitations of radar scattering within the snowpack needs addressing. Some inaccurate interpretations from the findings propagate into the discussion.

20 Generally, there is work required throughout the paper to correct typographical inconsistencies, sentence clarity and brevity, and precision of language.

Most figures require format alterations including larger text and plots, clearer colour scales and more revised panel positioning.

Answer to Reviewer 2 - global comment

25 *We would like to thank the reviewer for his careful reading of the manuscript, for this positive feedback and for the relevant and constructive remarks that have helped to improve the quality of the manuscript. In order to fit with your comments, we have made a revision of the manuscript that should have corrected the textual issues and well improved the readability of the document. We hope that these modifications will meet your requirements. Please find below the details on how your specific comments have been taken into account.*

30 *We acknowledge that several aspects of the manuscript required clarification and further development. In response to these comments, we have substantially revised the Introduction and Discussion sections. In particular, we expanded the review of existing Antarctic snow-depth and sea-ice-thickness products, improved the positioning of the MMS dataset within the current literature, and strengthened the discussion of Antarctic-specific snow processes and their implications for the retrieval.*

35 *We have also clarified the objectives of the different analysis sections, improved the description and justification of the comparison and validation datasets, and revised several interpretations to ensure that the conclusions are fully supported by the presented results. Additional material has been included where appropriate to better document the robustness of the methodology and the impact of the drift-aware framework. Figure 11 and impact analysis is now placed before the evaluation and inter-comparison section, earlier in the manuscript, to support better the value added by the DA processing compared to the CV.*

Finally, the manuscript has undergone a thorough revision to improve figure readability, terminology consistency, sentence clarity, and overall presentation. The figures have been revised accordingly, and detailed responses to each specific comment are provided below.

Before addressing the individual comments, we note that the dataset has been updated following the correction of an issue in the along-track filtering procedure. Although some reported values differ slightly from the original submission, the scientific conclusions and overall interpretation of the results remain unchanged.

In this document, the referee's comments are in bold type, the answers are in italic type.

Answers to Reviewer 2 : specific comments

Section 1 – introduction:

A reasonable introduction to the approaches but there is no comparison with other approaches, e.g. Lawrence et al. (2024) vs Garnier et al. (2021) vs Bocquet et al. (2024). What are the uncertainties from each? How well validated are they? There is no justification for your approach included in the paper, this is fundamental.

We thank the reviewer for this comment and agree that the introduction should provide a clearer comparison of existing approaches and their respective uncertainties and validation efforts. We have revised the introduction to include insights on other methodologies such as of Garnier et al. (2021), Bocquet et al. (2024), and Lawrence et al. (2024), and to better justify the methodology adopted in this study.

The uncertainties of radar scattering and penetration must be addressed with a section in the introduction.

We thank the reviewer for this suggestion and agree that the uncertainties associated with radar scattering and snow penetration are fundamental to radar-altimetry-based sea-ice thickness retrievals. We have therefore expanded the introduction to include a dedicated section of these processes and their implications for freeboard and sea-ice thickness estimation.

Section 2 – Methods:

Line 127: Is there any issue with only using coverage where SIC > 40% vs IS-2 with the drift correction? What happens between SIC 40% and 15%?

The drift products (OSI-455/435) provide no vectors below about 40 % SIC, but almost all ICESat-2 parcels lie above this threshold: only 0.2 % of the parcels are at 15–40 % SIC on 15 August 2020, and 6.5 % on 15 February 2020. Between 40 % and 15 % SIC, the parcels are kept and still drift-corrected. The drift is interpolated or extrapolated between the nearest valid vectors when a valid parcel has been created outside of the 15-40% area. We added a sentence to tackle this point in the corresponding paragraph in the revised manuscript. The following figure 1 supports this statement for the 15th of August 2020.

Line 149: Why was this important and is it the best approach for this newer SAR data?

Thanks for the comment. The use of TFMRA was primarily motivated by the need to ensure consistency with heritage radar-altimetry missions and existing climate data records. Our objective was to minimise differences arising from changes in processing methodology when comparing observations from multiple missions over multi-decadal timescales.

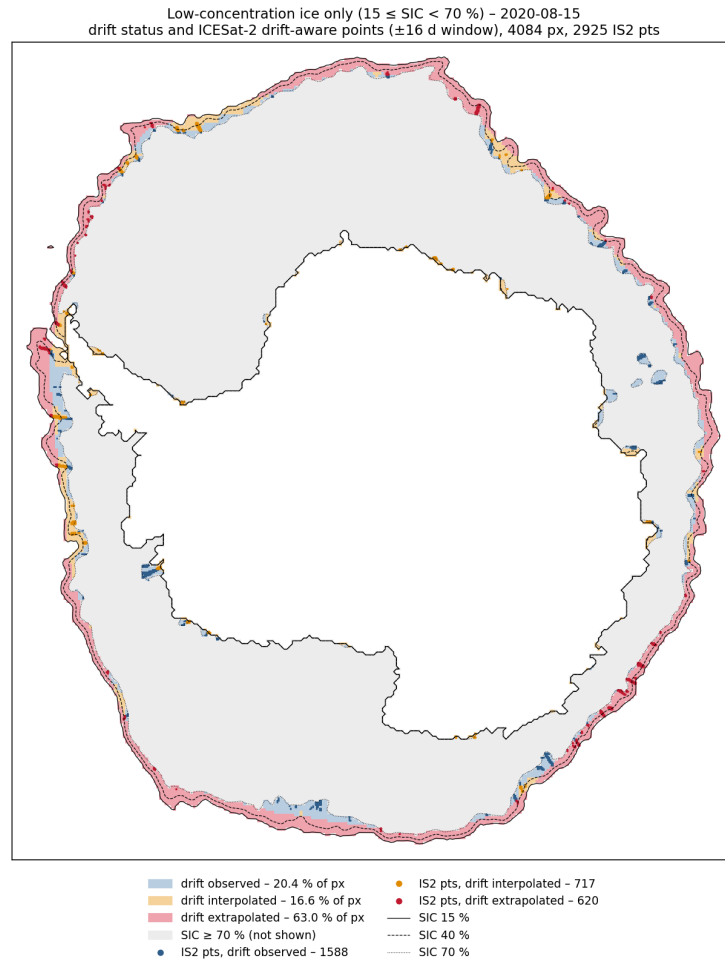


Figure 1. Illustration of the IS-2 points where the sea-ice drift has been interpolated or extrapolated. The highlighted zone is the area between 15 % and 70 %.

75 We acknowledge that alternative physical retrackerers and waveform-fitting approaches have recently been proposed for Antarctic sea ice (e.g. Fons et al., 2023). However, their relative performance and the impact of retracker choice on Antarctic sea-ice thickness retrievals remain areas of active research and are not yet fully understood. As discussed later in the manuscript, noticeable differences can arise between products that primarily differ in their retracking approach.

80 We do not claim that TFMRA is the optimal retracker for SAR-altimetry, but rather that it represents a pragmatic choice for constructing a homogeneous multi-mission sea-ice thickness record also consistent with heritage missions.

We extended the discussion also to the choice of the retracker and developed clarify the commented line.

Line 162: Justify whether these uncertainties are actually independent and can be treated in this way?

85 We thank the reviewer for the question. While the sea-ice elevation and SLA uncertainties may not be fully independent, their covariance is currently unknown and is therefore neglected. Consequently, uncertainties are combined in quadrature to provide

a first-order estimate of freeboard uncertainty. If positive correlations exist between both terms, this approach is expected to provide a conservative estimate of the resulting uncertainty.

We added justification to this paragraph.

90 **Line 165: This method was developed for the Arctic. Do you make any modification for it's use in the Antarctic? Justify its applicability.**

Thanks for the comment, if we understood the comment correctly, the comment refer to the correction that accounts for the reduced propagation speed of radar waves within the snowpack and this is based on the dielectric properties of snow rather than Arctic-specific empirical relationships. The underlying physics is therefore equally applicable to Antarctic sea ice, 95 provided appropriate snow-property assumptions are used. If the comment refers to the parametrization of the c_s therefore, the answer is the same as it has been established during lab experiments.

Line 180-188: There is information missing here. Look at references by Giles et al. (2008), Fredensborg Hansen et al. (2025) x2, Willatt et al. (2010), Willatt et al. (2025) and discuss how you account for the Ku-band scattering occurring 100 above the ice surface.

We thank the reviewer for highlighting this point. We have expanded the discussion of the Ku-band scattering horizon, added the suggested references, and clarified that no explicit correction for variations in the scattering horizon is applied in this study but that it implies limitation on the product that are further discussed.

105 **Line 195: Are there any more recent seasonal densities of snow and sea ice published available for use?**

Thanks for the comment. More recent seasonal snow density estimates are available (Lawrence et al., 2024). However, the benefit of adopting alternative density assumptions for the retrieval has not yet been assessed.

The drift correction seems like a good idea, however how much difference does it make to use this approach? Can this 110 be quantified? The drift patterns seem to converge on similar tracks in Figure 1 (particularly for S-3A). Is this explained by physical behaviour or a systematic bias in the method? Further, understandably the terminology “drift-aware” has been inherited from Ricker et al. (2025) but this terminology could be clearer. “Drift-corrected”, “drift-subtracted” or similar is more intuitive to describe the method.

Thanks for the comments. The apparent convergence of some trajectories reflects cases where sea-ice drift is broadly aligned 115 with the satellite sampling geometry, such that the same ice parcels may be observed during consecutive overpasses. This is not interpreted as a systematic bias of the drift-correction approach, but rather as a consequence of the interaction between sea-ice motion and the satellite sampling pattern. Accounting for drift is important in these situations, as neglecting it may lead to observations of the same ice parcel being treated as independent samples. We acknowledge that alternative terminology such as “drift-corrected” may be more intuitive at first glance. However, we retain the term “drift-aware” as it more accurately reflects 120 the methodology, which accounts for sea-ice drift during the collocation procedure rather than applying a direct correction to the sea-ice thickness retrieval itself. The term is also consistent with the terminology introduced by Ricker et al. (2025).

The importance of using the Drift-processing is now illustrated and quantified in the following figure 2. This figure replaces figure 11 (manuscript) and additional plots originally in figure 11 (manuscript) have been added to the appendix. This section has been moved before the inter-comparison and evaluation paragraphs.

Line 263-267: This need to be discussed fully. Why does it occur and why are weightings required? How do you account for it elsewhere?

130 *We thank the reviewer for this comment. We agree that the weighting scheme was not sufficiently explained in the original manuscript. The weighting is intended to limit the influence of known inter-mission freeboard differences between CryoSat-2 and Sentinel-3 when combining observations from all missions within the same grid cell. These differences have previously been reported when applying a common fixed-threshold retracker to both missions and are primarily attributed to differences in waveform characteristics, particularly leading-edge width (Lawrence et al., 2021). Because observations from both Sentinel-3A and Sentinel-3B may contribute simultaneously, an unweighted average would increase the influence of Sentinel-3 retrievals and therefore of any residual inter-mission differences. The manuscript has been revised accordingly.*

135

Have you considering applying weights to days within the +-15 day windows? Weighting retrievals on days closer to the target day higher address some of the product's uncertainty.

140 *Yes, we have considered this weighting, we agree that a temporal weighting could be justified, as observations farther from the target date are more likely to be affected by sea-ice thickness evolution. However, this effect is already partly accounted for through the SIT-change correction applied during the drift-aware processing. An additional temporal weighting would therefore risk double-counting the same effect. Instead, uncertainties associated with the SIT-change correction are propagated to the final product.*

145 **Line 271-272: Justify this treatment of uncertainty. The Ku-band is used for all radar altimeters over similar surfaces, is this independent? Clarify what the uncertainty represents.**

We thank the reviewer for this comment. The independence assumption refers to the three uncertainty components introduced (L2 retrieval uncertainty, drift-correction uncertainty and SIT-change uncertainty), rather than to uncertainties associated with the different radar altimeter missions. We agree that this was not sufficiently clear and have revised the text accordingly.

150 **Equation 5: Defines the snow depth uncertainty and states an assumption that the three terms are independent. However, the laser and Ku-band freeboard are used in the calculation of the snow density term, i.e. not independent. This seems flawed and requires justification or correction.**

155 *We thank the reviewer for this comment. We would like to clarify that the assumption applies to the uncertainty propagation and concerns the variables FB_{La} , FB_{Ku} , and c_s . While the sensitivity coefficient associated with c_s contains the term $(FB_{La} - FB_{Ku})$, this reflects the local sensitivity of the retrieval and does not imply statistical dependence between the variables themselves. The uncertainty associated with c_s originates from the prescribed snow density used to derive the wave-propagation velocity in snow and is therefore not linked to the measurement uncertainties of the laser and Ku-band freeboards. In its most general form, the first-order uncertainty propagation can be written as*

$$u_{Z_s}^2 = \sum_i \left(\frac{\partial Z_s}{\partial x_i} \right)^2 u_{x_i}^2 + 2 \sum_{i < j} \frac{\partial Z_s}{\partial x_i} \frac{\partial Z_s}{\partial x_j} \text{Cov}(x_i, x_j), \quad (1)$$

160 *where $x_i = \{FB_{La}, FB_{Ku}, c_s\}$. Equation 5 corresponds to the standard first-order approximation in which the covariance terms are neglected. The presence of $(FB_{La} - FB_{Ku})$ in the sensitivity coefficient therefore does not imply that covariance terms should be included. We therefore retain the formulation of Eq. 5.*

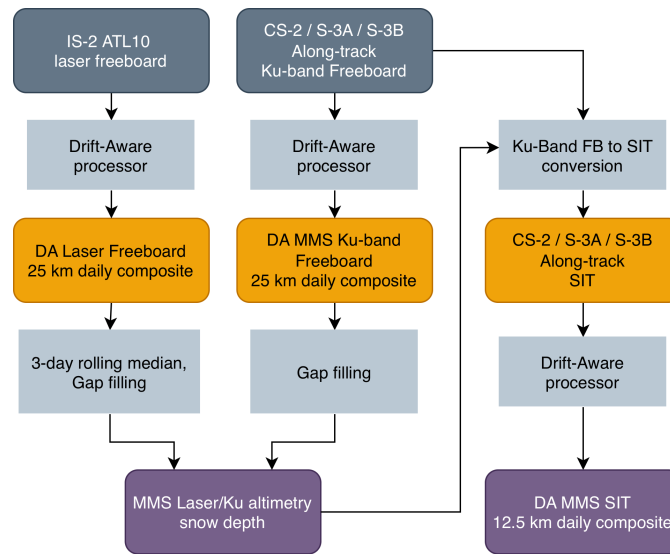


Figure 2. Schematic of the processing chain.

The methodology summary in Section 2.6 may be elevated with a flow-chart style schematic to provide visual explanation of the processing chain.

We thank the reviewer for this suggestion. We agree that the processing chain is relatively complex and could benefit from a more visual overview. We have therefore added a schematic workflow summarizing the main processing steps from the satellite observations to the final MMS sea-ice thickness and snow-depth products. The flow-chart in the following figure has been added to the manuscript.

Section 3 – Results:

Figure 3 analysis: As most uncertainty is due to drift, is it actually worthwhile? What happens if is not done?

We thank the reviewer for this comment. The magnitude of the drift-related uncertainty should not be interpreted as evidence that the drift-aware correction is not beneficial. Rather, it reflects the importance of sea-ice motion in the Antarctic and the explicit propagation of uncertainties associated with correcting for this motion. Without the drift-aware framework, observations acquired at different times would be composited without accounting for sea-ice displacement, leading to spatial smearing and reduced representation of sea-ice variability. The objective of Figure 3 is therefore to quantify the uncertainty associated with the correction, not to assess whether the correction itself should be applied. The impact of neglecting drift is instead evaluated through the comparison between the drift-aware and conventional products was presented in Section 5, especially spatially. Nevertheless, we agree that the message of this section 5 can be misleading, as suggested we have slightly change the structure of the manuscript to convey better this message.

Figure 4 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure.

185 A statement should be made here that positive relationships shown in Figure 4 are possibly unsurprising, as this is comparing the output (i.e. the product) with its inputs.

Line 344: Statement here does not really seem true, there is large differences occurring in the Weddell, Amundsen Seas and East Antarctic coast in Figure 4 which is not discussed. Mean differences of 0.03 m is quite large when snow depth is only 10 cm. A non-zero mean difference may not directly prove that there is no-systematic bias.

190 Line 347: Why is sampling differences a reason here, please explain?

Line 348: It cannot be concluded that residual differences are random based from standard deviation values alone – The requires more evidence.

195 *We thank the reviewer for these comments. We agree that the positive relationships shown in Figure 4 are expected, as the MMS product is partly derived from the individual mission estimates. We have clarified that the purpose of this comparison is not to provide an independent validation, but to assess the consistency of the merged product relative to each contributing mission.*

200 *We also revised the discussion of the mean differences. While the mean MMS–mission differences remain small, we agree that a non-zero mean difference alone does not demonstrate the absence of systematic biases. The text has therefore been modified accordingly.*

In addition, we expanded the discussion of the spatial patterns. Larger differences are observed in the Weddell Sea, the Amundsen–Bellingshausen sector, and along parts of the East Antarctic coast. These regions are characterized by thicker and more deformed sea ice and stronger thickness gradients. We also clarified that sampling differences refer to the fact that the different missions do not necessarily observe the same sea-ice parcels within a given grid cell and compositing period.

205 *Finally, we removed the statement suggesting that the residual differences are predominantly random, as this conclusion cannot be inferred from the standard deviation alone.*

210 **Figure 6 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure. It is not clear what is being investigated here and why. Please explain:**

Line 370: An additional panel in figure 6 to support this statement on the April–November period is required as evidence of this statement.

215 **Line 372 to 377: Not true. It seems fundamentally incorrect to state that the close relationship displayed in Figure 6 is not a causal relationship given that the MMS snow depth product is directly derived from IS2 freeboard. The result of Kacimi and Kwok (2020) is framed incorrectly here. The correct interpretation is that IS-2 freeboard explains most of the variance in MMS (IS2 – CS2) snow depth. In the case of Kacimi and Kwok (2020), it seems they suggest it points to the potentially for using this relationship to skip the IS2–CS2 comparison step where CS2 observations are not available. This entire paragraph needs re-consideration.**

225 *We thank the reviewer for these comments. The section has been revised to better explain its objective, which is to investigate the relationship between freeboard and the dual-altimetry snow-depth retrieval and to place the MMS estimates in the context of previous studies. To support the discussion, an additional figure showing the April–November period has been added to the Appendix.*

Since the MMS snow-depth product is derived from the difference between ICESat-2 total freeboard and Ku-band freeboard, the observed relationship is not independent. The text has therefore been revised to clarify that the analysis primarily illustrates

the extent to which ICESat-2 freeboard explains the variability of the MMS snow-depth estimates, and we have reconsidered the discussion in light of Kacimi and Kwok (2020).

The motives for the analysis conducted in section 3 needs to be made clearer. Additionally, bring the analysis from Section 5 earlier into Section 3 may build a stronger argument for justifying benefits of this of this product, as this should be a core aim of the conducted analysis.

235 *We agree that the benefits of the drift-aware processing should be introduced earlier in the manuscript. We have therefore reorganized the Results section and moved the analysis of the drift-aware processing ahead of the product intercomparison and evaluation sections.*

Section 4 – Intercomparison and validation:

240

How far the MMS product aligns with the Fons 2023 results is overstated throughout this section and requires re-framing.

Figure 7 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure.

245

Line 398-400: Commentary on the SIT from Fons 2023 being aligned to the MMS seems overstated as there are clear absolute differences in the bias maps including Weddell (2m) and Ross (1m) seas, as well systematically around the ice edge perimeter. There is a 20% difference between the stated means of MMS and Fons 2023.

250

Line 403: The histograms distributions are not aligned.

Line 405: The CSAO bias with MMS do not look that spatially different from Fons 2023, albeit of larger values, so this comment is puzzling.

255

Line 410: The Fons SIT seasonal variability does not look weaker than CSAO, it looks a similar amplitude.

Line 414: A similar comment is missing on Fons 2023 for snow depth here.

260 *We thank the reviewer for these comments. We agree that several statements overstated the level of agreement between the MMS and Fons (2023) products. We have therefore revised the section, moderated several interpretations, and expanded the discussion of both the similarities and the differences between MMS, Fons (2023), and CSAO, including regional patterns, distributions, seasonal variability, and snow-depth behavior.*

Figure 8 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure.

265

Line 447: Panel b differences may be proportionally large which is not discussed. It is hard to see due to shallow snow depth on this transect.

270 **Line 450: Not convinced of this from the figure. Using this figure you can make comments on “following similar trends”, however there is no quantification provided to prove that MMS is within the range of the two OIB observations. For example, providing root-mean squared values for quantifying the differences with respect to the OIB range.**

Line 451-452: Not really, this only looks like it in panel c.

275 **Line 453: MMS distribution does not really reproduce the central part of the observed distributions in figure 7.**

Line 455: Not demonstrated, requires quantification and re-writing.

280 *We thank the reviewer for these comments. We agree that the original discussion relied primarily on qualitative interpretation of the transects. To better support the comparison, we now quantify the agreement by reporting the percentage of MMS estimates falling within the range defined by the two OIB retrieval methods. The text has also been revised to provide a more balanced discussion of both agreements and discrepancies between the datasets. Accordingly, several statements regarding the level of agreement have been moderated.*

285 **Figure 9 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure.**

Line 473: It is not reasonable to make the statement. Figure 9 SIT and snow depth values are completely different.

290 **Line 474-477: Would ASPECT-BIO underestimate? This requires expanding and references to justify inclusion.**

We thank the reviewer for this comment. We agree that the statement was too strong, but the sea-ice thicknesses are not completely different, especially because they fall into the uncertainty bounds. The text has been revised to remove the claim that the comparison provides confidence in the MMS estimates. Instead, we now emphasize that the observed differences highlight the difficulty of validating Antarctic sea-ice thickness and snow-depth products using sparse in situ observations and the importance of more spatially representative validation datasets. Regarding the lower snow depths observed in ASPECT-BIO, we acknowledge that the choice of measurement location may influence the representativeness of the observations. Sampling is often conducted on level ice, where access is easier and safer, which may not fully capture the spatial variability of snow conditions. This may be particularly relevant in the western Weddell Sea, where sea ice is generally rougher and more deformed, potentially leading to higher regional snow depths than those captured by the in situ measurements.

300

Including “conventional gridding” (as in Section 5) in this analysis in Section 4, along with quantification will help indicate how much improved the “drift-aware” product is with respect to these observational datasets.

305 *We thank the reviewer for this comment. We do not think that adding the conventional-gridding (CV) product to Figure 9 would be particularly informative, given the limited number of ASPECT-BIO observations and the substantial uncertainties associated with both datasets. Moreover, the objective of this section is to evaluate the MMS product rather than to assess the impact of the drift-aware methodology itself.*

We agree, however, that the benefits of the drift-aware approach should be presented earlier in the Results section. We have therefore reorganized the manuscript so that the impact of drift-aware processing is discussed before the product intercomparison and evaluation sections.

310

Section 5 – Impact analysis:

Line 492: Couldn’t it instead mean that the drift correction introduces the errors?

We thank the reviewer for this comment. We agree that the original wording implicitly assumed that the differences were caused by neglecting drift in the conventional approach. As noted by the reviewer, the figure alone does not allow us to

315 attribute these differences uniquely to errors in the conventional product. We have therefore revised the text to use a more neutral formulation, emphasizing that the choice of gridding methodology can lead to differences exceeding the retrieval uncertainty and that accounting for sea-ice motion has a significant impact on the resulting thickness estimates.

320 **Figure 10 analysis: Some interpretation provided does not accurately reflect what is displayed in the figure.**

Line 488: This is not the case in the figure, drift-aware variability looks like it occurs more when the SIT change slows down, i.e. at the peaks.

325 **Line 494: Figure 10 does not seem to directly indicate an improvement using MMS, moreover MMS captures more variability. There is no evidence provided here to trust this temporal variability and therefore this figure alone cannot be used claim an improvement. This needs re-writing. This also propagates into Line 531 in Section 6.**

330 *We thank the reviewer for these comments. We agree that the original wording was too strong. Figure 10 shows that the drift-aware and conventional approaches can produce different representations of temporal variability and, at times, differences that exceed the estimated uncertainty. However, the figure alone does not demonstrate that these differences necessarily correspond to an improvement. We have therefore revised the text accordingly and limited the interpretation to the impact of the gridding methodology itself.*

335 *In addition, Figure 10 has been revised (Fig. 3). We added a panel comparing the daily thickness change, dh/dt , with the thickness change expected from advection by the ice drift, $-\mathbf{u} \cdot \nabla h$, computed from the OSI SAF ice drift (OSI SAF and EUMETSAT SAF On Ocean And Sea Ice, 2022; Lavergne and Down, 2023). The daily changes are about twice as large in the drift-aware product as in the conventional one, and are largely explained by advection ($R^2 = 0.61$, vs. 0.00 for the conventional product). The additional variability is therefore consistent with the transport of thickness features by the ice drift. As the drift-aware method itself uses ice drift, we do not present this as an independent validation but as a property of the gridding method. The text at Lines 488, 494 and 531 has been revised accordingly.*

340

Section 5.2 must be expanded to a full quantitative treatment, e.g. passive microwave snow depth. This is critical for this paper as it justifies the entire concept. After doing so, this should be promoted into results Section 3. [major corrections]

345 *We thank the reviewer for this suggestion. We agree that the impact of the drift-aware processing should be assessed more quantitatively. Rather than relying solely on the qualitative comparison shown in Section 5.2, we now include quantitative spatial metrics (called Dice) and move this analysis earlier in the Results section. This allows the benefits and limitations of the drift-aware approach to be established before the subsequent product intercomparison and evaluation sections. We focus on the MYI export event illustrated in Fig. 11 and track its evolution in both space and time. Note that this exported MYI event is the most significant one observed during the study period and therefore provides an excellent case study for assessing the added value of the DA framework. To quantitatively assess the consistency between sea-ice thickness (SIT), SAR backscatter (σ_0), and MYI fraction, we first standardized each variable to make them directly comparable despite their different units and dynamic ranges. We then identified regions characterized by enhanced values ($Z \geq 1$) and computed the Dice coefficient between the corresponding binary masks.*

350 *The Dice coefficient quantifies the spatial overlap between two datasets and is defined as:*

355 *$$\text{Dice} = 2|A \cap B| / (|A| + |B|)$$*

where A and B represent the regions with standardized values greater than or equal to one. A Dice coefficient of 1 indicates perfect overlap, while a value of 0 indicates no overlap. This metric therefore provides an objective measure of the degree to which the spatial structures identified in the SIT fields coincide with those visible in the SAR backscatter and MYI fraction fields.

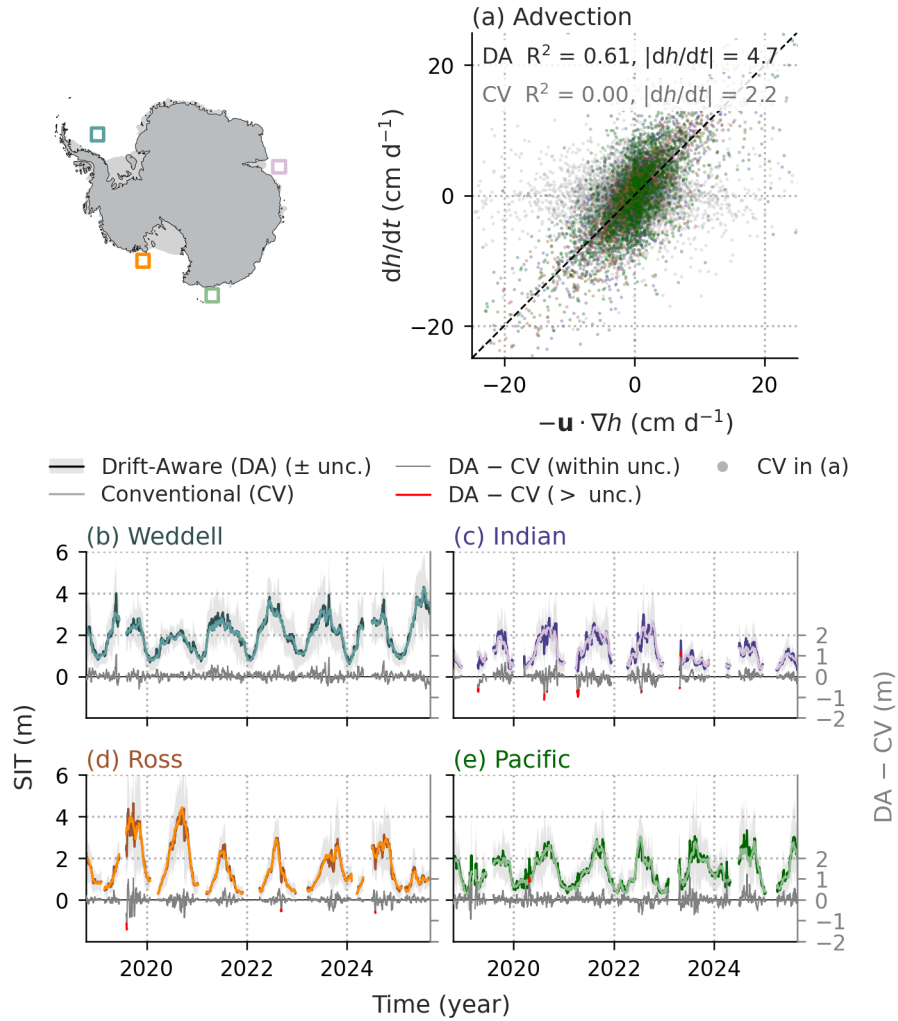


Figure 3. Revised Figure 10. (a) Daily thickness change versus the thickness change expected from advection by the ice drift ($-\mathbf{u} \cdot \nabla h$, OSI SAF drift), for the drift-aware (DA, coloured by region) and conventional (CV, grey) products; the dashed line is the 1:1 line. (b–e) Box-averaged SIT time series for DA ($\pm$ uncertainty) and CV; the DA–CV difference is shown on the right axis, in red where it exceeds the uncertainty.

360 The main results of figure 11 have been replaced by the following figure 4 and remaining findings corresponding to coastal ice will be place in the appendix:

Discussion and conclusions:

365 There are no references to the literature in this discussion. Needs to be completely re-written with references and discussion of quantified analysis. [Major corrections]

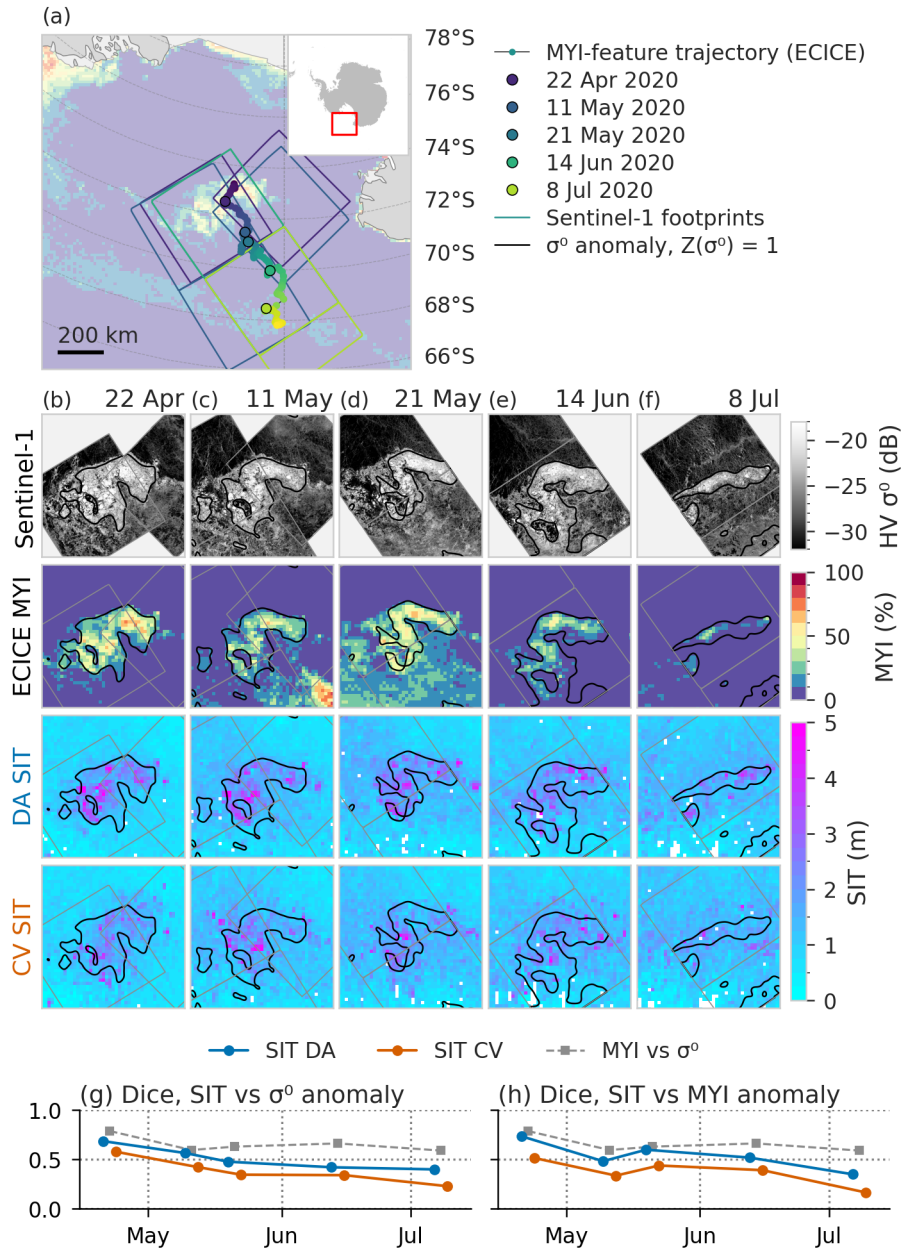


Figure 4. Multiyear-ice (MYI) feature drifting northward across the Ross Sea in austral winter 2020. (a) Trajectory of the feature from 10 April to 20 July 2020, tracked as the centroid of the ECICE MYI fraction $\geq 20\%$, with the Sentinel-1 EW footprints of the five dates shown below; background: ECICE MYI fraction on 22 April 2020. (b–f) For each date, from top to bottom: Sentinel-1 HV backscatter (σ_0), ECICE MYI fraction, and sea-ice thickness (SIT) from the drift-aware (DA) and conventional (CV) products, in a 560×560 km window centred on the feature. The black contour marks the σ_0 anomaly, $Z(\sigma_0) = 1$, where Z is the field standardized over the common 12.5 km cells of each date (σ_0 smoothed with a 12.5 km Gaussian filter). (g–h) Dice coefficient, $2|A \cap B| / (|A| + |B|)$, between the SIT anomaly ($Z \geq 1$) and the σ_0 (g) or MYI (h) anomaly ($Z \geq 1$). Grey: Dice between the MYI and σ_0 anomalies.

We thank the reviewer for this comment. We agree that the Discussion section does not sufficiently place our results in the context of previous Antarctic sea-ice thickness and snow-depth studies. We have therefore substantially revised the Discussion to include a broader comparison with the existing literature.

370 **Line 545: Why did you not try other re-tracking?**

We thank the reviewer for this comment. Alternative retrackers have indeed been proposed for Antarctic sea-ice applications, including waveform-fitting approaches such as those used by (Fons et al., 2023). However, the objective of this study was not to optimize the freeboard retrieval itself, but to develop a harmonized multi-mission and drift-aware processing framework. We therefore adopted a common TFMRA configuration for CryoSat-2 and Sentinel-3A/B to ensure consistency across missions and compatibility with existing climate-data-record approaches. We have clarified this motivation in the revised manuscript.

Answers to Reviewer 2 : Technical comments

Abstract

2: “sea-ice” and “sea ice” is used inconsistently (throughout paper).

380 *This correction has been done.*

2: Remove “further”.

This correction has been done.

3: Use of “we”, not clear if it refers to authors or sea ice community?

It corresponds to this study, a correction has been done.

385 **5: Quality – Change “provides” to “is assumed to provide”.**

This correction has been done.

6: Quality - Remove “very” from “very complex”.

This correction has been done.

12: Merging “retrievals from” several altimeters, not merging the altimeters themselves.

390 *This correction has been done.*

14: Change “measurements” to “observations” when talking about retrievals from satellites (throughout paper).

This correction has been done.

15: Description “daily updated, monthly sea ice...” needs rewording to clarify - “daily updated, monthly-window averaged”?

395 *This correction has been done.*

18: “especially if only one sensor is used.” – Clarify. Do you mean that whilst multiple sensors result in some improvement (line 12) the combined results do not compare as well to airborne sensors?

We removed the last part of the sentence as we feel it doesn’t help understanding the outcomes of the study.

21-25: Re-write – Insufficient references and not up to date.

This comment has been taken into account

29: Revise sentence for readability and brevity.

This comment has been taken into account

405 **32: Quality - Remove “uniquely” from “considering uniquely”. Improve this sentence’s readability.**

This correction has been done.

33: Choose more appropriate word than “reflects”.

This correction has been done.

33-35: Add a comment on sea ice volume here.

410 *This comment has been taken into account*

35: Does Massonnet et al. (2013) actually say this? Is this paraphrased correctly?

39: Add “which TOGETHER enabled...”

This comment has been taken into account

41: Define snow, ice and total freeboard here.

415 *This comment has been taken into account*

43-45: Quality – Split up sentence for brevity and readability.

This comment has been taken into account

45: What is meant by “such as negligible ice freeboard”? – Clarify wording

This comment has been taken into account

420 **54: Remove “strongly” and change “depends” to “depend”.**

This comment has been taken into account

60: Re-write for quality and readability.

This comment has been taken into account

61-64: Provide typical and max velocities to quantify ice drift.

425 *We added median and maximum sea ice drift for both hemisphere during the studied period.*

Methods

87: Add “merging RETREIVALS of the”. What does “dedicated” refer to here? Change “delayed” to “delay”.

This comment has been taken into account

430 **94: Ensure all types of units all have consistent font and spacing (i.e. 13 GHz not 13GHz) (throughout entire paper).**

This comment has been taken into account

95: Change “providing” to “which provides”.

This comment has been taken into account

97: Add “same TYPE OF radar...” and remove “called the”. Revise sentence for brevity and readability.

435 *This comment has been taken into account*

100: “similar enough” – poor quality, please quantify and clarify.

We thank the reviewer for this comment. The wording has been revised to clarify that CryoSat-2 and Sentinel-3 rely on comparable SAR (Delay-Doppler) processing, resulting in similar spatial sampling characteristics.

102: Remove “using satellite altimetry alone”.

440 *This comment has been taken into account*

109: Specify the quality flag used here.

This comment has been taken into account

131: Correct format to “Lavergne and Down (2023) and Lavergne et al. (2010)”.

This comment has been taken into account

445 **139: Correct format of Paul et al. citation.**

This comment has been taken into account

142-144: Re-write for quality and readability.

This comment has been taken into account

145: Need to supply another reference here as Laxon et al. (2003) did not do this.

450 *This comment has been taken into account*

151-152: Re-write for quality and readability.

This comment has been taken into account

151-155: Requires references.

This comment has been taken into account

455 **159-160: Why the need to mention this? Also, notation rFB is not defined.**

The reason to describe the computation here is to explain the uncertainty computation in the next paragraph. We have defined the missing notation.

164: Why is this value set? This conflicts with the statement above. Please clarify.

460 *We clarified this paragraph. This value is set from the SI-CCI ATBD, assuming the correlation lengthscale is 100km for the SLA, also from the SI-CCI ATBD (Paul et al., 2024).*

168: Ulaby F.T and R.K (1987) citation seems wrong format (throughout paper).

This comment has been taken into account.

169: Centre all equations (throughout paper).

465 *We thank the reviewer for this comment. The equations are formatted using the journal LaTeX template and follow the default equation formatting provided by the template. We will nevertheless carefully review the final manuscript formatting to ensure that all displayed equations are correctly rendered.*

175-178: Needs explaining further.

We clarified this sentence.

182 Change “affects significantly” to “significantly affects” or remove “significantly”.

470 *This comment has been taken into account.*

203: Add “AS uncertainties In radar...”

This comment has been taken into account.

216: Typically drifted 100 km from where to where? Please explain further. Is “has typically drifted” to “can typically drift” more appropriate?

475 *We thank the reviewer for this comment. Our intention was not to refer to successive satellite revisits, but rather to the temporal separation between observations used within the monthly compositing window. The text has been revised to clarify that observations acquired at the beginning and end of the compositing period may correspond to substantially different ice parcels because of sea-ice drift, making them not directly comparable without drift correction.*

223: The relevance of this statement is unclear, please explain further if it is important.

480 *We removed this sentence from this paragraph.*

236: Some headings in the paper are missing their number, change to “2.3.1 Uncertainties” throughout. Also ensure capitalisation of words in all headings is consistent (throughout paper).

This comment has been taken into account.

485 **Figure 2: Plots and text too small (All figures throughout the paper). Histogram y-axis label is confusing. Distributions are not transparent so they block each other.**

This comment has been taken into account.

246: Change “25-km” to “25 km” (in line with the line 94 correction above).

This comment has been taken into account.

490 **257 262: Anonyms for satellite missions are used inconsistently throughout paper. Define in the introduction and subsequently use acronyms throughout (i.e. CS-2, IS-2, S-3A etc.)**

This comment has been taken into account.

275: Justify this statement and reference. Define “total freeboard”.

Total freeboard has been defined in the introduction, according to another comment. We added references there.

495 **276-277: This statement requires reference over Antarctic sea ice and justification, it is not in line with current literature.**

275: See correction 14 above.

This comment has been taken into account.

307: Change “Ku freeboards” to Ku-band freeboards. Clarify what is meant hear co-located? Grid pixels or fixed distance?

500 *We thank the reviewer for this comment. The term “co-located” was potentially misleading in this context. Our intention was to indicate that the laser and radar freeboard estimates are first drift-corrected and referenced to a common grid and reference date before calculating snow depth. The wording has been revised accordingly.*

308: Change “using” to “by applying the principal of”.

This comment has been taken into account.

505 **310: Is step 5 a repeat of step 2?**

The step 5 refers to the compositing of the sea-icce thickness whereas the step 2 refers to the compositing of the IS-2 freeboard and the MMS Ku-band freeboard.

316: Specify sea ice thickness, as this reads as snow thickness.

This comment has been taken into account.

510

Results

319: Define SIT in introduction and do not repeatedly define (also line 354).

This comment has been taken into account.

320: See correction 94 above (1.24 m not 1.24m). Change SIT from italics to normal. Specify pan-Antarctic.

515 *This comment has been taken into account.*

322: Revise for brevity and readability.

This comment has been taken into account.

332-335: No evidence provided yet, this is a concluding statement of this section and lacks quantification.

We removed this sentence. This comment has been taken into account.

520 **Figure 3: Too small. Colour scales need revising, particularly for uncertainties and snow depth as smaller values are lost (throughout paper). 10 cm of snow is impossible to identify in plot G (similar in later figures). Colour bar has four ticks across 30 cm intervals, change to three.**

Figure 3 caption: Re-write for quality and readability. Remove capital letters.

350: Choose more appropriate word for “coherent”.

525 *This comment has been taken into account.*

Figure 5: impossible to sea snow depth detail between 0 and 10 cm. Plots too small.

This comment has been taken into account as well as for the Fig 3.

354-355: This should be in figure caption instead.

This comment has been taken into account.

530 **358: Also the east Antarctic coast in sept?**

We thank the reviewer for raising this point, the thick ice in the Eastern part might come from Landfast ice. We added a comment and limitation on these coastal part in the manuscript.

362-364: This statement needs evidence to justify and reference. Are these uncertainties isolated to the months stated?

We thank the reviewer for this comment. We agree that the original statement was not sufficiently supported. Our intention was not to suggest that these uncertainties are limited to December-March, but rather that they may become more pronounced during the austral summer when flooding, snow-ice formation, wet snow, and changes in snow stratigraphy are more likely to affect the radar scattering horizon and the interpretation of radar freeboard measurements. Similar uncertainties may also occur outside the summer period depending on local snow and ice conditions. We have revised the text accordingly and added the appropriate references.

——- modify in the text !!! ——

Figure 6: Poor colour scale for scatter points. Paler points are hard to see. No colour bar provided. Figure 6 is not explicitly cited anywhere in the text.

372: Specify what variance, i.e. add “94% of the MMS SNOW DEPTH variance”.

This comment has been taken into account.

370: Check if capital R-squared notation is correct here.

This comment has been taken into account. The R^2 is the right notation.

379: Change “no systemic offset” to “less systemic offset” or similar.

This comment has been taken into account.

Intercomparison and validation

Title: change “validation” to “evaluation”

This comment has been taken into account.

403: Reword “aligned with MMS” to accurately reflect results in figure 7.

This comment has been taken into account.

Figure 7: Again colour scale is bad for looking at snow depth from 0 to 10 cm.

This comment has been taken into account.

Figure 7 caption: Change “histograms shown on right” to “left”. “Relative to MMS for MMS” is confusing.

This comment has been taken into account.

423: Does “CSAO” ASD need stating here for clarity?

This comment has been taken into account. We changed this paragraph to refer to the description of the product earlier in this section.

432: Define OIB here (not line 439)

This comment has been taken into account.

Figure 8: Revise the panel configuration here (Possibly one column of transect plots, one column of histograms, one column with a larger map). Histograms too small and cluttered. CSAO and Fons 2023’ line colours switch between transect plots and histograms. Change labels from “rolling average” to “rolling mean”.

This comment has been taken into account.

Figure 8 caption: 25 km rolling mean applies to OIB Peakiness and Wavelet - revise the description.

This comment has been taken into account.

457: ASPECT-BIO section has not been introduction and requires further detail to explain this comparison.

570 *This comment has been taken into account.*

Figure 9 caption: Requires more detail including the explanation of error-bars.

This comment has been taken into account.

466: Change to “x- and y- directions”

This comment has been taken into account.

575 **470: Clarify, error or standard deviation? What are the bars in the figure?**

This comment has been taken into account.

471: “essentially zero” is not great accuracy - revise.

This comment has been taken into account.

Impact analysis

580 **Figure 11 plots are too small. Contour lines require defining in caption.**

This comment has been taken into account. We split the figure in two different figures to improve readability. We also removed the Paul et al 2024 as we figured out it was more confusing than insightful.

Discussion and conclusion

Re-write with references to literature and discussion of quantified analysis.

585 *The conclusion and discussion have been rewritten completely to strengthen the manuscript.*

258L Correct “static”.

This comment has been taken into account.

579: Change to “Antarctic applications”.

This comment has been taken into account.

590 *Appendix*

594: Define DTU21.

We now refer to the relevant references and to earlier definition in the manuscript.

609-610: Justify 5 degree, 350 m and 700 m values.

595 *Filtering the snow radar data based on altitude and attitude is common in snow depth retrievals (i.e. Kurtz et al, 2013 and Jutila et al, 2021). The values used are based on values stated in that literature. We now refer to these in the text.*

614 and 620: Hyphens too big.

The inconsistency with the rest of the manuscript has been fixed.

A3 requires references throughout.

600 *Thanks for the comment! We added the relevant reference to the whole section. Most equations and formulations stem from the NSIDC documentation.*

647: Add “retrieve THE same elevations”.

Fixed.

659: Change “Wavelet at can” to “and”.

Fixed.

605 **Supplements**

Line 681: add “doi:” to start of link to video supplement.

Fixed.

References

- Fons, S., Kurtz, N., and Bagnardi, M.: A decade-plus of Antarctic sea ice thickness and volume estimates from CryoSat-2 using a physical model and waveform fitting, *The Cryosphere*, 17, 2487–2508, <https://doi.org/10.5194/tc-17-2487-2023>, 2023.
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- Lawrence, I. R., Armitage, T. W., Tsamados, M. C., Stroeve, J. C., Dinardo, S., Ridout, A. L., Muir, A., Tilling, R. L., and Shepherd, A.: Extending the Arctic sea ice freeboard and sea level record with the Sentinel-3 radar altimeters, *Advances in Space Research*, 68, 711–723, <https://doi.org/10.1016/j.asr.2019.10.011>, 2021.
- 615 Lawrence, I. R., Ridout, A. L., Shepherd, A., and Tilling, R.: A Simulation of Snow on Antarctic Sea Ice Based on Satellite Data and Climate Reanalyses, *Journal of Geophysical Research: Oceans*, 129, e2022JC019002, <https://doi.org/10.1029/2022JC019002>, 2024.
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- 620 Paul, S., Hendricks, S., Rinne, E., and Sallila, H.: CCI+ Sea Ice ECV - Sea Ice Thickness Algorithm Theoretical Basis Document (ATBD), <https://doi.org/10.5281/ZENODO.10605840>, version Number: 3.2, 2024.
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