Author Response to Reviewer #1

We thank the reviewer for their positive assessment and constructive suggestions. The comments have helped us improve the clarity and completeness of the manuscript. This is my first paper and it helped me improve my writing. The comments lead to additional validations. Comments from the other reviewer lead to a large restructuring of the paper. Hence, the manuscript has changed considerably. We present here a high-level overview of the changes before addressing each specific comment, in order to help the reviewer navigate the revised version. Changes have been highlighted in green.

The first important change is that the dataset has been given a name: SO_SLICE.

Original manuscript structure :

- **Sect. 1** — Introduction
- **Sect. 2** — Data
    - 2.1 Satellite data / missions
    - 2.2 Open ocean satellite data / CMEMS
    - 2.3 In-situ data for validation
- **Sect. 3** — Methods
    - 3.1 Altimetry above the open ocean: SSH equation, corrections, MSS, ADT
    - 3.2 Lead detection and retracking
    - 3.3 Editing (including MERIS validation mixed in here)
- **Sect. 4** — Results
    - 4.1 Product overview mixed with CMEMS comparison and seasonal climatology
    - 4.2 Validations.
- **Sect. 5** — Discussion and conclusions

Revised manuscript structure :

- **Sect. 1** — Introduction
- **Sect. 2** — Data and standard processing
    - 2.1 Missions + range-to-SLA short paragraph. Shortened, cites references instead.
    - 2.2 Ancillary and validation data. New: introduces *all* comparison datasets upfront: C3S (instead of CMEMS), Dragomir, Auger and TGs + BPRs.
- **Sect. 3** — Sea-ice altimetry processing
    - 3.1 Lead detection, retracking, and along-track editing
    - 3.2 Ocean/lead bias correction
    - 3.3 Inter-mission harmonization
    - 3.4 Optimal Interpolation. New, detailed subsection on OI inputs
    - 3.5 Derived variables
- **Sect. 4** — Results and validation
    - 4.1 Along-track qualitative validation. New: MERIS moved here from methods
    - 4.2 Product overview. New: Seasonal cycle has been dropped + spectral analysis and comparison with C3S to quantify SO_SLICE capabilities.

- 4.3 Validation: Inter-satellite consistency (new diagnostics), formal error, comparison with in-situ observations, comparison with other altimetry products (new).
- 4.4 Effective resolution of SO_SLICE.

Sect. 5 — Discussion and conclusions

Below, we address each comment individually. Reviewer comments are in bold italics, and our responses follow in regular text.

General Comments

1 - Many citations have parentheses when they should not have (l. 45, 46, 48, 51, …)

All citation formatting has been corrected throughout the manuscript.

2 - Please revise all the figures. Too often the labels for the axes, legend, colorbar are not readable because too small. Every letter of a plot should be readable, otherwise it should be removed. And often the images do not look high-resolution.

All figures have been regenerated at 300 dpi with larger labels and consistent font sizes throughout. We apologise for the poor resolution in the initial submission. We had put some care in producing the figures at the correct resolution, but the Copernicus preprint rendering process degraded them.

3 - The generated product needs a name. It would help the reader in this manuscript (instead of calling it "our/the product" in the text or just "Product" in the images). Also, it will be beneficial for future works using and referencing it.

This is a valuable remark. The product is now named SO_SLICE (Southern Ocean Sea Level In the ICE) and is referred to consistently throughout the manuscript and all figures.

4 - Check if the tables' caption has to be below or above the table.

Table captions have been moved above their respective tables, consistent with the layout of published ESSD papers.

5 - Use consistent layout for tables (linewidth of borders).

The table styles have been updated with consistent border widths throughout.

6 - In the methods section, it would be useful to have a recap of all the processing applied to the data as a diagram. What editing, debiasing techniques are applied to each product sequentially, otherwise it's hard to keep track.

We have added a processing pipeline diagram as the new Figure 3, which describes the full processing chain from L1b waveforms through to the final gridded product, indicating which steps are described in which section. It can be found in line 205.

7 - The altimetry product created is not characterized enough in terms of capabilities. The minimum information required is at least the effective spatial resolution (which I imagine will change in time and in the different regions due to coverage) and the temporal resolution. This is crucial information to understand what kind of processes can be resolved and studied.

We agree that this was a significant gap in the original manuscript. The maps we distribute are on a 25 km grid at daily to monthly frequencies, but this is not the effective resolution of SO_SLICE. We now describe the capabilities of the dataset in two ways.

First, we computed the spatial and temporal power spectral density (PSD) of SO_SLICE and compared it with the Copernicus reference product (Fig. 6, Sect. 4.2). This shows the scales at which energy is present within the altimetry products.

Second, we estimated the effective spatial and temporal resolution following the noise-to-signal ratio (NSR) protocol of Ballarotta et al. (2019). The effective spatial resolution ranges from approximately 120 km between 50°S and 65°S to 200 km south of 65°S, and does indeed vary regionally. The effective temporal resolution averages 37 days across in situ stations. Both are discussed in the new Sect. 4.4. In the paper, we present resolution estimation on the year 2019, where we have 3 satellites. The same diagnostic was done in 2011, with Envisat only maps and the results were similar.

Specific Comments

1 - 85: Cryosat-2 not Cryosat.

Corrected throughout. We now use "CryoSat-2" with a capital S, consistent with the ESA official spelling.

2 - Page 4. The table of the satellite missions should be Table 1.

Corrected.

3 - 116: Supplementary Table S2. Somewhere in the text or in the Supplementary table it would be interesting to add the distance of the in-situ stations from the coast. This would strengthen your comparison later, when you explain why some of the differences with the altimetry may arise.

We have added a distance-to-coast column to the supplementary table listing the in situ station characteristics (Table S1).

4 - Fig 2: Please add in the caption the meaning of the red dot of the bottom panels.

The red dot indicates the retracking point on the waveform. This has been added to the caption. (line 171-176).

5 - 157: the sentence is missing a verb.

We've removed this sentence from the original paragraph (now lines 124-135).

6 - 174: LaTeX rendering not working.

Corrected. lines 124-135

7 - 206–211: this explanation is too quick. Why is the extra-editing needed? What does the threshold remove? Why do you first perform an "extra-editing" on the data and then an "iterative editing" on the ocean data (only open ocean, no leads?)? Also add one sentence to briefly describe the "off-nadir hooking" phenomenon. Is this processing the same for all satellites? Please rephrase the full paragraph adding information and clarity.

The editing procedure has been completely rewritten with three explicitly numbered steps.

Step 1 (empirical threshold) applies to all lead data and removes lead measurements that go through the classification step, but for which we are not 100% confident. For example, we have a condition on sea ice concentration where sea ice concentration needs to be at least > 30% for us to accept the waveform as a lead.

Step 2 (iterative Lanczos filter) applies to open-ocean data only, where observations are dense enough to compute meaningful along-track statistics. It iteratively removes points that deviate from a low-pass filtered profile.

Step 3 (hooking filter) applies to lead data from LRM altimeters only (Envisat, SARAL). The hooking effect is now described in the text. This correction is not applied to SAR altimeters (CryoSat-2, Sentinel-3A), whose smaller footprint makes them much less susceptible to off-nadir contamination. (lines 218-230).

8 - 215: fewer by how much? What is the proportion of data removed? Is there a general statistic about the difference between leads and open-ocean points kept, compared to the initial amount?

We have added a supplementary figure (Fig. S3) showing the number of lead measurements per 100 km × 100 km × 1-month bin before and after editing, for each satellite mission. For Envisat, the median number of lead observations per bin decreases from approximately 300 to 35 after editing. Similar values are obtained for SARAL. In both cases, the hooking filter accounts for the largest fraction of removed points. CryoSat-2 and Sentinel-3A retain a higher fraction of lead observations, as the hooking correction is not applied to SAR altimeters. For open-ocean waveforms, approximately 85% are retained after editing for all missions. These statistics are briefly discussed in Sect. 3.1. (lines 232-238).

9 - What is the statistical occurrence of the limitations mentioned in paragraph 225–235 for Envisat, and other satellites if relevant? How many points are correctly classified as leads, how many are incorrectly classified as leads and how many are missing? How does a misinterpretation of a lead influence the results? This is important to judge the robustness of the method.

The limitations described in this paragraph (now Sect. 4.1) are relevant for all LRM altimeters (Envisat, SARAL). We have not ourselves quantified the fraction of correctly vs incorrectly classified leads. This has been done in part by Poisson et al. (2018), who compared waveform-based lead classifications against MERIS optical imagery for 42 Envisat along-track sections. In their Figure 8, they show scatter plots of two optical properties for leads, open ocean, and ice floes. The three classes are efficiently discriminated, although a small overlap exists between leads and ice floes. The cases we discuss in Sect. 4.1 (points 3 and 4 in Fig. 4) fall within this overlap region. Poisson et al. do not provide a single fraction of misclassified leads, but the overlap is visually small. A comprehensive quantification of the classification error rate is beyond the scope of this study. However, the impact of residual misclassifications on the final gridded fields is diluted by the OI, which combines hundreds of observations per grid point over each mapping window. The validation in Sect. 4.3–4.6 confirms that the product achieves comparable accuracy in ice-covered and open-ocean regions, indicating that classification errors do not significantly degrade the final fields.

10 - 229: what do you mean “at least for this example”? How do the results change for the other satellites?

What we meant is that this is a single illustrative example, not a systematic assessment. The classification methodology is the same for all missions (Poisson et al., 2018). While we did not repeat this qualitative illustration for the other satellites, the gridded product validation in Sect. 4.3–4.6 provides an integrated assessment across all missions. We have rephrased the text to avoid ambiguity. This paragraph refers to Sect. 4.1.

11 - Fig 3: Add the units for the time dimension in panel (a) and add the geographical coordinates in panel (c). Panel b is hard to interpret. Please make bigger. One suggestion could be to keep the horizontal panel (a) at the top of the figure, while (b) and (c) can be displaced in a second row next to each other. Make legend labels and markers bigger because they are hard to understand. Colorbar label and ticks must have bigger font.

Figure 3 (now Figure 4) has been redesigned following all of these suggestions. Panel (a) is at the top, with panels (b) and (c) side by side below. All labels, markers, legend entries, and colorbar ticks have been enlarged. Units have been added to the time dimension in panel (a) and geographical coordinates are now shown in panel (c). (lines 340).

12 - 257: Why is the additional debiasing step only performed for Envisat?

The additional debiasing step is applied to Envisat because its ocean/lead bias exhibits a very strong spatial pattern that cannot be treated as spatially homogeneous noise, unlike the other missions. We empirically found a correlation between the residual ocean/lead bias and the average peakiness of the neighbouring lead waveforms. This relationship is exploited to apply a spatially varying correction: the bias, which was initially spatially heterogeneous, is reduced to a level where it becomes more like a white noise and is comparable to the other missions. For CryoSat-2, SARAL, and Sentinel-3A, the ocean/lead bias is spatially more uniform and is adequately removed by the standard correction described in Sect. 3.2. (lines 242-257).

13 - 262: Why is CryoSat-2 used as a reference? You say it at l. 234, but should be better explained here.

CryoSat-2 is used as the inter-mission reference because it is the only mission with temporal overlap with all three other satellites (Envisat from 2010–2012, SARAL from 2013 onward, Sentinel-3A from 2016 onward). This makes it the natural pivot for harmonising the missions. This has been clarified in the relevant paragraph. (line 261).

14 - Add reference for EASE2 grid: https://doi.org/10.3390/ijgi1010032

The reference (Brodzik et al., 2012) has been added. (line 284).

15 - Section 3.6: rephrase. The interpolation is an important step of your methodology and needs more details. For example, you say that "the expected variance and noise estimation necessary for the interpolation also come from Auger et al. (2022a) and Veillard et al. (2024)" without mentioning why you need these variables and how they influence the interpolation. Mention the basics of the DUACS-DT2022 mapping and how this kind of interpolation could influence the results.

We have expanded Sect. 3.4 (previously 3.6) to explain the role of each OI input parameter. The text now reads: "The OI weights are determined by a space-time covariance function controlled by three inputs: the spatial and temporal correlation scales, the expected signal variance, and the measurement noise variance. The correlation scales define the space-time radius within which observations contribute to each grid point estimate. They control the smoothness of the output and set a lower bound on the resolved scales. The signal variance determines the expected amplitude of SLA variability. The noise variance controls how much the OI trusts individual observations relative to the prior." The prescribed correlation scale values are described and shown in supplementary figures (Figs. S3–S4). We also explain that the correlation scales and variance files come from Auger et al. (2022a) and Veillard et al. (2024), which were tuned for the subpolar Southern Ocean. This can be found in lines 270 – 279.

16 - 277: is this the effective spatial resolution of the product or the distance between grid points? What is the effective temporal resolution? What are the covered latitudes?

This is the distance between grid points (25 km), not the effective resolution. The effective spatial resolution (120–250 km depending on latitude) and effective temporal resolution (23–48 days, mean 37 days) are now discussed in the new Sect. 4.4. The product covers the Southern Ocean south of 50°S. We've switched from resolution to spatial grid cells for clarity (line 285).

17 - The comparison between your product and CMEMS is too quick. We need some spectra (at least in the supplementary material). You could do frequency-wavenumber spectra to know exactly what kind of structures the product resolves in space and time. At least in the open-ocean region, to provide more robust results for this comparison.

You should add an extra panel per row with the difference between CMEMS and "your product", to highlight where the major differences are.

The comparison with CMEMS has been substantially expanded, though we now use C3S as an altimetry product. We have added difference maps (Fig. 5c) and computed both spatial and temporal PSD for SO_SLICE and C3S, separately in the ACC and the subpolar Southern Ocean (Fig. 6). The spatial PSD shows close agreement in the ACC at wavelengths longer than 150 km, with SO_SLICE retaining slightly more energy at shorter wavelengths due to less aggressive along-track filtering. In the subpolar SO, SO_SLICE contains substantially more energy at all wavelengths, with a PSD ratio ranging from 5 at 1000 km to 17 at 50 km. The temporal PSD confirms consistency at periods longer than 90 days, with SO_SLICE retaining more energy at shorter periods (90–20 days), which we again attribute to differences in filtering. We chose to present spatial and temporal PSD separately rather than combined frequency-wavenumber spectra, as we found this more visual and more directly informative for the user who needs to know what scales are resolved. This can be found in section 4.2 (lines 365-280).

18 - Section 4.2.1: Frequency-wavenumber spectra of the SLA would be a good way to quantify the scales at which the satellites agree or differ. It is not enough to visually compare the images finding "good agreement in large-scale spatial patterns across missions". What is large and small scales? It is important to do this to be sure of what you are comparing and to add robustness to the final product. Generally throughout the entire paper, scales need to be quantified better. You can do it monthly, but it would be even better to do it at the highest temporal frequency that you have.

We now present the spatial spectral coherence between mission pairs (Sect. 4.3.1, Fig. 7a), computed on binned 75 km × 10-day maps. This directly quantifies the scales at which the satellites agree or differ: all mission pairs display coherence greater than 0.5 at wavelengths longer than 300 km. In the ACC, the 0.5 threshold is crossed at approximately 180 km for all pairs. In the subpolar SO, it varies from 170 km (SAR–SAR pair) to 304 km (CryoSat-2 vs Envisat). This is done at the 10-day time step, the highest temporal frequency available for the binned maps. We believe the spectral coherence is a more targeted diagnostic than frequency-wavenumber spectra for answering the specific question of inter-mission agreement, while being easier to interpret. (lines 406-414).

19 - Fig 6: "(b-c/e-g) Comparison of monthly SLA snapshots" — it is not the comparison but just the individual snapshots. Right? - Panel (c) should be CryoSat-2.

Correct. Fig 6 is now Fig7, however we have removed the maps from the analysis now.

Corrected in Fig 7. (line 439)

20 - 362: "at face value"? Are you sure this is the right expression you want to use here?

No, what we meant was simply that this metric may overestimate the true errors of the final product. We have rephrased: "These leave-one-out RMSE values represent upper bounds on the errors of the final product, since SO_SLICE integrates all four missions simultaneously rather than withholding one ». (line 476-479).

21 - Fig 7b: what season is this plot? Is it showing only winter or also other seasons? If other seasons are included, how do you treat the region south of your white contour in spring/summer/autumn? Is it included in the open-ocean distribution?

Both the map and the distributions show winter (JJA) RMSE values, which was not specified in the original caption. We have added this precision. The regional split is between the ACC and the subpolar Southern Ocean (defined by the MDT contour), applied uniformly regardless of season. (line 480).

22 - 376–380: can you provide the mathematical formulation of the formal error?

The formal error equation has been added to Sect. 3.4: $\sigma^2\_formal = \sigma^2\_signal - c_0^T(C + N)^{-1}c_0$, where σ^2_signal is the prescribed signal variance, C is the space-time covariance matrix of the observations, N is the noise covariance matrix, and c_0 is the covariance vector between the estimation grid point and the observation locations. (line 305).

23 - 389: (REF)

The placeholder reference has been removed. We apologise for the oversight.

24 - 393: what do you mean by "in addition to Envisat"?

This was a mistake. There are no Envisat data available in 2019. The sentence has been corrected to list only CryoSat-2, SARAL, and Sentinel-3A. (line 505).

25 - Fig 8: No need to write (top) and (bottom) in the caption if you have separate letters for the panels. Mention panel (d) in the text. Add info on the time scale on which the average is performed. Is it for the full product?

The caption has been revised: panel labels are now used exclusively, panel (d) is referenced in the text, and the averaging period (over the full period) is specified. (line 523-528).

26 - 459: Add OSISAF acronym or shortly explain what it is.

The acronym (Ocean and Sea Ice Satellite Application Facility) and the reference to the dataset have been added. Line 573.

27 - Tab 5: You don't mention in the text the change of the results when the sea-ice coverage exceeds 10%. How do you explain that most of the Pearson coefficients increase in the higher sea-ice scenario?

We do not have a fully convincing explanation for this. In some instances, such as for Myrtle B or C, the portion of the time series with sea-ice concentration > 10% is relatively short, so the higher correlation could be partly due to sampling variability. In most cases, the gain in correlation is lower than 0.1, so we are not confident it represents a definitive improvement. In the revised manuscript, Tables 4 and 5 now report metrics computed exclusively during ice-covered periods (sea-ice concentration > 10%), which is the most relevant condition for validating SLA retrievals under ice. This is to not confuse the reader.

Author Response to Reviewer #2

We thank the reviewer for their thorough and constructive review. It is my first paper and the feedback helped me improve my writing tremendously. The comments were extensive and lead to a large restructuring of the paper. The other reviewer comments lead to additional validations. Hence, the manuscript has changed considerably. We present here a high-level overview of the changes before addressing each specific comment, in order to help the reviewer navigate the revised version. Changes have been highlighted in green.

The first important change is that the dataset has been given a name: SO_SLICE.

Original manuscript structure :

- **Sect. 1** — Introduction
- **Sect. 2** — Data
  - 2.1 Satellite data / missions
  - 2.2 Open ocean satellite data / CMEMS
  - 2.3 In-situ data for validation
- **Sect. 3** — Methods
  - 3.1 Altimetry above the open ocean: SSH equation, corrections, MSS, ADT
  - 3.2 Lead detection and retracking
  - 3.3 Editing (including MERIS validation mixed in here)
- **Sect. 4** — Results
  - 4.1 Product overview mixed with CMEMS comparison and seasonal climatology
  - 4.2 Validations.
- **Sect. 5** — Discussion and conclusions

Revised manuscript structure :

- **Sect. 1** — Introduction
- **Sect. 2** — Data and standard processing
  - 2.1 Missions + range-to-SLA short paragraph. Shortened, cites references instead.
  - 2.2 Ancillary and validation data. New: introduces *all* comparison datasets upfront: C3S (instead of CMEMS), Dragomir, Auger and TGs + BPRs.
- **Sect. 3** — Sea-ice altimetry processing
  - 3.1 Lead detection, retracking, and along-track editing
  - 3.2 Ocean/lead bias correction
  - 3.3 Inter-mission harmonization
  - 3.4 Optimal Interpolation. New, detailed subsection on OI inputs
  - 3.5 Derived variables
- **Sect. 4** — Results and validation
  - 4.1 Along-track qualitative validation. New: MERIS moved here from methods
  - 4.2 Product overview. New: Seasonal cycle has been dropped + spectral analysis and comparison with C3S to quantify SO_SLICE capabilities.
  - 4.3 Validation: Inter-satellite consistency (new diagnostics), formal error, comparison with in-situ observations, comparison with other altimetry products (new).

- 4.4 Effective resolution of SO_SLICE.
- **Sect. 5** — Discussion and conclusions

Below, we address each comment individually. Reviewer comments are in bold italics.

General Comments

A dataset is created based on observations from four altimeter missions. However, consideration should be given to why the dataset was not extended to at least 2024/2025 and why Sentinel-3B was not used. Why were not all missions used up to and including 2021 (i.e. Sentinel-3A)? As a result, the dataset tends to be outdated very quickly or to have limited applications.

Sentinel-3A coverage in SO_SLICE ends in June 2021 because the dataset relies on the Sentinel-3 Processing Prototype (S3PP) retracked data from CNES/CLS, which was discontinued after this date. We chose to extend the gridded product to the end of December 2021 to end on a complete calendar year, using SARAL/AltiKa and CryoSat-2 data only for the second half of 2021.

Sentinel-3B was not included because the S3PP retracking was never applied to it, and incorporating data processed with a different retracker would introduce inconsistencies in the along-track SLA inputs.

We acknowledge that the current temporal extent is a limitation. Extending SO_SLICE beyond 2021 will require using an alternative source of consistently retracked Sentinel-3A/B data over sea-ice leads. Our plans to address this are detailed in our responses below. That said, we believe the dataset in its current form fills a significant observational gap in the subpolar Southern Ocean. No comparable gridded sea level record exists in terms of temporal coverage. The closest study, Dragomir 2024, does not validate their dataset extensively. Therefore, we consider SO_SLICE to be of considerable value to the community despite its temporal scope.

Regarding Sentinel-3, the dedicated sea-ice thematic data products provided by ESA (LAN_SI) were not utilized, even though they are specifically optimized for sea-ice applications due to their enhanced waveform binning and associated technical improvements. The manuscript should clarify why these products were excluded from the processing chain. A reprocessing effort that incorporates Sentinel-3B, as well as the ESA LAN_SI thematic datasets, is strongly recommended to ensure that the dataset fully benefits from the most suitable altimetry products currently (and freely) available. If such an update is not planned, the authors should provide a detailed justification and a thorough discussion of the differences, similarities and potential advantages or disadvantages of the Sentinel-3 products used in this study compared with the publicly available ESA LAN_SI sea-ice-thematic data.

This is a fair point. We chose to use the S3PP database rather than the ESA LAN_SI thematic products for two main reasons.

First, this work builds directly on the processing chains developed by Auger et al. (2025) and Veillard et al. (2024), both of which rely on the S3PP experimental tables. The primary focus of our study was to extend the existing product backward in time by processing Envisat data and to merge all missions into a consistent gridded dataset, rather than to revisit the Sentinel-3 processing itself.

Second, and more importantly, we aimed to maintain processing continuity across all missions in SO_SLICE. This required applying the same lead/ocean classifier (described in Poisson et al., 2018) and the same retracking strategy to all SAR-mode data. Working from S3PP L1b waveforms allowed us to do so, whereas starting from the L2 LAN_SI products, which come with their own classification and retracking already applied, would not have permitted this unified approach.

Regarding waveform quality, we note that the S3PP processing also applies zero-padding and Hamming windowing to the SAR waveforms, so the resulting waveforms should be comparable to those in the LAN_SI products. A short paragraph clarifying this choice has been added to Section 2.1 (lines 116-122). However, we did not expand on the pros and cons of each dataset as we think this is enough of a justification.

We acknowledge that our approach has limitations: S3PP processing was not applied to Sentinel-3B, and the Sentinel-3A record ends in mid-2021. Going forward, we plan to transition to the EUMETSAT Sentinel-3 F06 reprocessing baseline once it becomes available, which will feature the same classifiers as S3PP and cover both Sentinel-3A and Sentinel-3B. By 2028, Sentinel-3C data may also be incorporated.

In general, the term "assimilation" is used in the document. From my understanding, assimilation is the process by which observational data (e.g., sensor data, measurements) are systematically incorporated into a running numerical model in order to update its state. This term should only be used in connection with numerical simulations, which is not the case here, as this is a data set based purely on observations. Please rephrase the relevant passages.

The reviewer is right. All instances of "assimilation" have been replaced with "interpolation" or "mapping" as appropriate.

The work very often lacks clear scientific statements and precision. Abbreviations are frequently introduced, and satellite mission names are spelled differently.

We have carefully revised the manuscript for consistency. "CryoSat-2" is now spelled consistently throughout (previously alternating with "Cryosat-2"), and "SARAL/AltiKa" is used at first mention, shortened to "SARAL" thereafter. We've also tuned down the use of abbreviations.

The plots generally have poor print quality (although this may also be related to the creation of the preprint). In addition, the same parameters are often presented in different units (m vs. cm).

We put considerable care into producing figures at the correct resolution, but the Copernicus preprint rendering degraded them. All figures have been regenerated at 300 dpi with consistent font sizes. Units are now standardized to meters throughout. The only exception is in Fig. 7 and tables 4 and 5 where we present RMSD/RMSE values. For visualization purposes it was easier to keep the metrics in centimeters for the figure, and it felt more natural to keep the RMSE in centimeters for the tables.

In addition, the term "along-track" and as a stand-alone description, "along-tracks," is unclear. Along-track describes, for example, observations along a track, as opposed to gridded data. It describes how the data is available, either as along-track observations/elevation/heights, etc., or as gridded observations/heights. "Along-

tracks" does not sound scientific. Sometimes the text contains "along-tracks" or just "along-track". Please be more precise here.

All instances of "along-tracks" used as a noun have been replaced with "along-track observations," "along-track data," or "along-track measurements."

For the validation, it would be helpful to clarify why the datasets introduced earlier in the manuscript were not included in the comparison. Instead, the study relies on gridded CMEMS data, which is not specifically designed for sea-ice applications. Providing some context for this choice would strengthen the overall transparency and interpretation of the results.

There was no reason not to include them. We have expanded on the validation.

Instead of CMEMS, we now use a C3S dataset as we needed daily data and it was easier to download. This can be found in Sect. 4.2.

In addition to the C3S comparison, we now include comparisons with Auger et al. (2022a)/Veillard et al. (2024) and Dragomir et al. (2024) in Sect. 4.3.5, (~line 590) with time series south of 66°S, pointwise correlation and RMSD maps, and power spectrum density (PSD) comparisons.

The comparison with C3S is meant to show SO_SLICE gain information in the subpolar Southern Ocean and that the open ocean processing we use can be trusted.

Geostrophic velocity components are provided in the data product. These variables are not mentioned or presented in any greater detail in the explanations. It is not clear why these were calculated or included at all.

We computed the geostrophic currents so that users do not have to derive them from the ADT fields themselves. The idea was to improve the ease of use of the product, just as on the Copernicus Marine Datastore you can download SLA/ADT/geostrophic currents. We added a sentence in Sect. 3.5 (line 300) to clarify that these variables are provided for user convenience but are not analyzed further in this study. We believe it is still useful to include them in the distributed dataset.

The paper is very difficult to read, as some facts are often repeated or not clearly separated, e.g., possible causes for deviations or differences between datasets used in the validation. Explanations of how the altimetry observations are edited are mixed with descriptions about imaging systems such as MERIS, which are more part of the validation. A validation with a recent independent altimetry dataset appears without any context. I would suggest a restructuring.

I am sorry about that. We have restructured substantially in order to improve the readability of the paper and avoid repetitions. Sect. 2 now contains all data descriptions. Sect. 3, the methods section, focuses on sea-ice-specific processing only. The MERIS comparison has been moved to Sect. 4.1. All comparison datasets are now introduced in Sect. 2.2 before they appear in the results.

At the same time, some parts of the paper could be shortened considerably. I don't think it's necessary to explain the entire story of satellite altimetry data processing. There is already a lot of literature on this subject that can be cited.

Our aim was to write a paper that any physical oceanographer could read easily and understand how altimetry works and how SO_SLICE circumvents the issue of sea ice. But that was perhaps poorly executed. We dropped the long “classic altimetry” section, but kept a short paragraph in Sect. 2.1 (lines 124-135) so that all key information can be found within the paper. Section 3 now focuses entirely on the sea-ice-specific methodology.

Specific Comments

Line 20: “under sea ice cover”: You don’t observe SLAs under the sea ice. You detect leads and interpolate the SLAs to retrieve information under the sea ice. Here, it appears as if satellite altimetry could be used to look beneath the sea ice. Please rephrase.

Indeed, we rephrased to clarify that SLA is measured in lead locations (line 14).

Line 30: The mention of “coverage in 2023” feels a bit misleading, since the dataset presented here extends to 2021. You may want to rephrase or clarify this to avoid creating confusion for the reader.

We agree. The sentence has been removed (line 30).

Line 32: “characterized” à investigated?

We switch from poorly characterized to uncharacterized, but we can replace by not investigated if the reviewer still feels it is better (line 31).

Line 34: “is” to are

We intended “is” to refer to “a key variable” (singular). I believe it is correct but may be wrong as English is not my first language. Kept as is (line 35).

Line 35: This relationship is confusing. SLAs refer to a mathematical reference surface and not to a physical one. Geostrophic currents are derived based on a physical reference (i.e. geoid).

We rephrased to state that SLAs can be used to derive anomalies in the ocean geostrophic currents. It was easier to formulate it this way than introducing the geoid or MDT at this stage. (line 36).

Line 37: How can satellite altimetry be used to provide insights into the ocean vertical structure, in particular by using SLAs? I think the sentence must be rephrased. + Line 37: Same as the comment above: How would you obtain information about structural ocean changes if only the sea surface can be spotted. Satellite altimetry is a geometric observation technique in which the sum of the effects acting on the surface height is always observed.

We agree these statements were too strong. Altimetry alone cannot resolve the vertical structure. Rephrased to state that SLA captures the combined effects of steric and manometric contributions to sea level. (line 37 – 39).

Line 46: “[…] from space.” This sentence does not fit to the sentence in Line 40 “However, this technique has remained [..]” . There must be a logical connection,

perhaps through additional information about what needs to be done to enable satellite altimetry to be used in the ice-covered ocean.

We rewrote the paragraph, explaining why traditional altimetry fails in ice-covered regions, what would be needed, and how the community got there (line 42 – 44).

Line 46: "open-water hole", a lead is not a hole. This does not sound scientific. + Line 47: "extremely reflective", what does this mean? This is not scientific wording.

Very fair remarks. Leads are now defined as "fractures between the ice floes" and described their return as "highly specular" (line 44-45).

Line 51: Check the brackets.

Removed.

Line 52: MSS is Mean Sea Surface not Mean Sea State. Here it is correct.

The original text was correct (Mean Sea Surface). No change needed.

Line 54: There is also important work Rose from, S.K.; Andersen, O.B.; Passaro, M.; Ludwigsen, C.A.; Schwatke, C. Arctic Ocean Sea Level Record from the Complete Radar Altimetry Era: 1991–2018. Remote Sens. 2019, 11, 1672. https://doi.org/10.3390/rs11141672 and Doglioni, F., Ricker, R., Rabe, B., Barth, A., Troupin, C., and Kanzow, T.: Sea surface height anomaly and geostrophic current velocity from altimetry measurements over the Arctic Ocean (2011–2020), Earth Syst. Sci. Data, 15, 225–263, https://doi.org/10.5194/essd-15-225-2023, 2023.The authors created datasets that include altimeter observations from nearly 30 years. Please add this references.

Both references have been added to the introduction. (line 57 – 59).

Line 55: from the missions ERS-1/-2 and Envisat to quantify…

Rephrased. (line 51-52).

Line 56: utilized laser altimeter observations from ICESat to map the Arctic dynamic ocean topography

Rephrased. (line 52-53).

Line 54–60: Please check for more missing literature.

We added Rose et al. (2019) and Doglioni et al. (2023) and reviewed the paragraph. (line 50 – 59).

Line 70: Please comment on the fact why you haven't used Auger et al. 2022b and Auger et al., 2023 and Veillard et al., 2024 as a comparison dataset for your validation activities. What is your innovation in context to these references? This should be somewhere highlighted. Why should users use your dataset and not a dataset presented in the references?

The primary innovation of SO_SLICE lies in its temporal coverage. By reprocessing Envisat data, we extend the subpolar Southern Ocean sea level record from 6 years (Auger et al., 2022; 2013–2019) and 8 years (Veillard et al., 2024; 2013–2021) to 19 continuous years

(2003–2021). This two to three fold xincrease in record length is critical for studying interannual-to-decadal sea level variability in the Southern Ocean, where long time series are needed to separate forced trends from natural modes of variability such as SAM and ENSO teleconnections.

For the 2013–2021 period, SO_SLICE uses the same input missions as Veillard et al. (2024) and Auger et al. (2022b), with minor differences in processing. We have now included a direct comparison with the Auger et al. (2022b) gridded product in a new Section 4.3.5. We did not include Veillard et al. (2024) as a separate comparison dataset, as it would be largely redundant given the overlap in input data and processing, and the Auger et al. product is more widely used in the community.

Beyond temporal extension, a second key contribution of this work is the in-depth validation and characterization of the gridded product. Existing polar altimetry datasets in the Southern Ocean have not been accompanied by a comprehensive assessment of their effective resolution, error budget, and comparison against independent observations (tide gauges, bottom pressure recorders, GRACE). SO_SLICE provides this characterization, which we believe is essential for users to employ the dataset with confidence.

Line 96: The Ka-Band does not allow for a higher sampling rate. AltiKa allows for a higher sampling rate.

Yes that is true. We've rephrased it to only talk about the satellite footprint now. (line 97).

Line 96: 8 km → diameter?

Corrected to "8->9 km in diameter." (line 102).

Line 99: SARIn is not entirely used on land ice. Please check the CryoSat-2 acquisition mode maps.

Specified that SARIn also includes parts of the coastal Southern Ocean. A reference of the acquisition mask is given. (line 107 - 109).

Line 102: If there is a long repeat orbit, the tracks are spatially quite narrow and dense, indicating a high spatial but low temporal resolution. Please rephrase.

We had mixed this up. Rephrased. (line 112 - 114).

Line 103: I don't understand the sentence with "contain artefacts". Why is a long repeat orbit responsible for unphysical meridional stripes? Please also check the brackets behind Auger et al. 2022.

We mentioned this because in the single-mission CryoSat-2 dataset of Armitage et al. (2018), an alternation of positive and negative meridional stripes is visible. Auger et al. (2022a, their Fig. 6) show that multi-mission merging attenuates the pattern. We think this is due to poor temporal sampling: within the same month, neighbouring meridional regions might be sampled at the beginning or end of the month, leading to different average values if there is strong oceanic variability. However, because we are not fully confident in this explanation, we removed the sentence.

Line 105-106: What is the name of the dataset from the French space agency? Which data is used here? What do you mean by "specifically designed"?

We use the CNES Sentinel-3 Processing Prototype (S3PP). We said specifically designed for leads processing because zero-padding and Hamming windowing are applied to the waveforms. It is the same dataset used by Prandi et al. (2021) for the Arctic and Auger et al. (2022a) for the Southern Ocean.

Figure 1: Why does Sentinel-3A suddenly stops and is not available after 30.06.2021. Why don't you use Sentinel-3B to densify your observations (see general comments)?

We answered that in the general comments.

Section 2.2: As far as I understand you also interpolate via DUACS, but you also compare a DUACS processed dataset? Can you please explain the differences here and why CMEMS is an independent dataset?

We added this clarification: "Compared to our product, it uses the same interpolation methodology but does not include observations within the sea ice cover." So we use the DUACS procedure but not the same input data. (line 146 – 151).

Line 121: What do you mean by 100 days globally and 60 days at high latitudes? What defines high latitude against the background that "globally" includes also high latitudes. Why do the days change?

We had mixed this up. Vinogradova et al. (2007) show that bottom pressure and SLA are highly correlated at short timescales (< 30 days) globally. At high latitudes (south of 60°S), this correlation holds for longer timescales, due to the ocean being more barotropic. The original text said 100 days but the relevant figure in the paper only shows this for the 20–60 days band. This has been corrected and moved to Sect. 4.3.4. (line 543-544).

Line 123: Where are these locations? Maybe you can link Figure 9?

We now reference Figure 9 (now 10) and Table S1 in Sect. 2.2.3. The table gives the exact locations. (line 161 -162).

Line 129: What do you mean by "key geophysical parameters" All physical parameters are important variables. Please rephrase. You write parameter(s), but list only SSH.

We agree it was poorly formulated. We wanted to also mention SWH as a key parameter, but it would have been more confusing. The sentence has been removed.

Figure 2 caption: Mean Sea State → Mean Sea Surface.

Corrected.

Figure 2 caption: "Range" is not a reference height. SSH is the difference between the height of the satellite above a reference surface and the distance between the phase center of the altimeter and the sea surface. Please be more precise here and rephrase.

Rephrased to clarify that range is a distance and not a height. (line 175).

Section 3.1: Since the article describes a data set in the ice-covered ocean and represents an advanced data set, it is not necessary to explain the entire processing of the altimetry data. There are numerous publications that do this. In my opinion, section 3.1 can be shortened considerably.

Our aim was accessibility for physical oceanographers, but it was poorly executed. We dropped the long section, replacing it with a short paragraph in Sect. 2.1 (line 124-135). Section 3 now focuses on sea-ice-specific methodology.

Line 147 – 148: Do you mean LRM data? What does that mean in the case of Sentinel-3A, since Sentinel-3A observes exclusively in SAR mode? I'm lost here. Why MLE4 for SAR missions? Could you please clarify.

Even If S3A only observes in SAR mode, over the open ocean its SAR waveforms are here converted to pseudo LRM waveforms on which we can apply MLE4. This was not properly explained and we modified that in section 3.1 (line 194-197).

Line 151: SSH (abbreviation should be enough).

SSH is now defined at first mention in the introduction and use throughout the paper.

Line 159: signal → effects?

We dropped the detailed corrections description. Table 2 lists all corrections; the reader is referred to Fu and Cazenave (2001) for information on the geophysical corrections. (line 124-135).

Line 161: MSS → Mean Sea Surface.

Corrected.

Line 165: What is the name of the MSS used?

The MSS (CNES_CLS22 Hybrid) is now specified in the text and Table 2.

Lines 167–174: Line 167: Without direct context, you provide information about the ADT. Why? Is it ensured that the SLA and MDT refer to the same MSS? Please provide further details.

Line 172: Now you use a different MSS, are you also computing different SLA? Please explain.

Line 173: Which geoid are you using here: GOCO06S or GOCO06C? The geoid model is not cited.

We have grouped the description of distributed variables into Sect. 3.5. We acknowledge the inconsistency raised by the reviewer. Our along-track SLA is referenced to the CNES_CLS Hybrid MSS, which offers significantly better accuracy than the standard CNES_CLS MSS in polar regions. I originally chose these MSS given that this work is funded by CNES. However, no MDT has been computed from this Hybrid MSS. Since all results presented in the paper use the SLA field, this inconsistency does not affect any figure or conclusion.

Nonetheless, we wish to distribute an ADT field for user convenience. The distributed ADT is obtained by adding the MDT of Jousset (2023), computed from the CNES_CLS22 MSS and the GOCO06s geoid (Kvas et al., 2021), to our Hybrid-MSS-referenced SLA. There is therefore a mismatch between the MSS used for the SLA and the MSS underlying the MDT. Sect. 3.5 now states this explicitly and warns users accordingly. (line 292-296).

We considered dropping the ADT from the distributed variables altogether, but this would likely lead users to combine our SLA with an arbitrary MDT, a less controlled outcome. For the next version of SO_SLICE, we plan to switch to the DTU21 MSS, for which a consistent MDT is available and that performs well in polar regions.

Geostrophic equation: Why do you need a vertical unit vector? What does it contain? You provide only horizontal velocities. Please explain and rephrase.

We need a vertical vector for the vectorial product. It is the unit vector with 0 in the horizontal directions and 1 in the vertical direction. I specified now that the gradient operator is the horizontal gradient operator. It is the condensed way to represent the geostrophic velocities I believe. However, it might be clearer to just split u and v geostrophic. (line 300-302).

Line 180: "in this study fly above sea ice" This is not scientific wording. Please rephrase.

We've dropped this sentence, which was the introduction to methods. The new introduction can be found in lines 178-184.

Line 180: Why should altimeters not work above sea ice? Please rephrase the sentence.

We've dropped this sentence as well, which was the introduction to methods. The new introduction can be found in lines 178-184.

Line 181: get → receive

We've dropped the associated sentence as well, which was the introduction to methods. The new introduction can be found in lines 178-184.

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Lines 182–184: "reflects the radar wave extremely well" is not scientific.

We now define leads as quasi-specular reflectors and introduce the backscattering coefficient. We kept "smooth" qualified as "very smooth at the scale of the radar wavelength," which we believe helps visualize what is happening. (lines 186-189).

Line 186: Why is it important to know how the classifier is validated? It would be more interesting to know briefly how the classifier works since this is an important step in the sea ice altimetry processing chain. Please add some more info here.

The reviewer is right. We added that the classifier is a neural network (Poisson et al., 2018) that categorizes each waveform into one of sixteen classes according to its shape. Lead echoes have their dedicated class. (lines 190-193).

Line 197: Why is there an offset? It's written above there should be no offset? Can you explain this sentence a bit more.

Poisson et al. (2018) argue their Adaptive retracker "guarantees, by construction, continuity between open ocean and ice-covered regions." We interpreted this as no bias. However, they show along-track data with a y-axis of ±1 m, making centimetre-level offsets invisible. When we examined the same tracks and recomputed SSH, lead data was consistently a few centimetres above open-ocean data. This could be due to differences in geophysical corrections or retracker limitations over specular surfaces. In the Southern Ocean, we consistently find a residual bias with the Adaptive retracker, though smaller than with an empirical retracker. We therefore correct it. We did not want to enter into a detailed discussion of Poisson et al.'s claims as it is outside the scope of this paper. The text now reads: "Theoretically, the Adaptive retracker should allow merging without an inter-surface bias (Poisson et al., 2018). In practice, we find that a residual offset of a few centimetres remains, which is estimated and removed." (lines 200-202).

Table 2: Is this MSS used for generating ADT heights or SLAs? In the caption it's written for SLA, but what MSS are you using for generating ADT heights?

This is the MSS for SLA computation. The caption now specifies this. ADT is discussed in Sect. 3.5. We've talked about this in a previous comment.

Line 202-203: A reference would support this hypothesis. Otherwise, some more explanations or additional tests are needed. Please comment.

Line 204: Please add a reference (no reliable SSB…)

From our understanding, neglecting SSB over leads is something all polar altimetric products do because we cannot get a reliable SWH from lead waveforms. SSB is derived from SWH and wind speed (Tran et al., 2010), but SWH is not reliably retrieved from specular waveforms (Poisson et al., 2018). Sea ice inhibits wave propagation (Squire, 2020). All existing products neglect SSB under ice (Armitage et al., 2016; Auger et al., 2022a). We acknowledge this has flaws and is a source of bias. We expand on this in section 3.1. (lines 210-216).

Line 206 – 211: I don't fully understand this procedure. How do you deal with the very small number of lead observations compared to a large number of observations from the open ocean? How do you deal with along-track data that consists only of lead observations? It would help a lot to restructure this text passage.

We had forgotten the word "open" in front of "ocean" — the iterative editing only applies to open-ocean data, which is numerous. The procedure is now three numbered steps. Step 1: empirical threshold (all data). Step 2: iterative Lanczos filter (open ocean only). Step 3: hooking filter (LRM leads only). Together with the new pipeline figure (Fig. 3), we believe this is now clearer. (line 218-230).

Line 215: You refer to Fig.3b?

We meant Fig. 3a. Corrected.

Line 216 – 218: "Being too conservative […] sea level data à It seems the second part of the sentence is missing. One would expect "and being too optimistic… "Please rephrase or add the counterpart of the sentence.

Line 218: " the example along-track" à you mean the example pass? Something is missing here.

Line 216- 222: In general, I don't understand the entire paragraph. I have many questions here. It starts with "As in all filtering" and ends with "highest uncertainties". What is the "all filtering" and what are the highest uncertainties? What can we expect here? Concerning along-tracks. It's along-track data. Along-tracks is not scientific wording. Please change this. What are the repetitive patterns you are talking about? Maybe you can rephrase this text passage.

This overall paragraph has been rewritten and this specific part has been removed as we found it more confusing than anything. We now simply talk about statistics on the number of points kept vs filtered out. (line 231-239). The MERIS part of this paragraph is now touched on in section 4.1, more precisely in lines 317-323.

Line 224: Why do you switch the topic to MERIS? What does this have to do with data editing? I see this part more under data or briefly touched upon with a reference to how MERIS works. What is the resolution of MERIS compared to the spatial extend of the leads you want to detect? Can MERIS be used as a reliable comparison source for this task here or can it be that you detect leads which cannot be spotted with MERIS. Please discuss this.

We had two goals: show what is fed to the OI and qualitatively show we extract data from leads. We agree it was poorly placed and does not have to do anything with data editing. The MERIS comparison is now in Sect. 4.1. MERIS resolution (~390 m) is not enough to accurately detect leads from which we can recover signal (minimum 100 meters wide)., this is more to show qualitatively that some leads that we detect with the waveform can also be found with MERIS (if they are big enough). We use this paragraph to also discuss some limits of our approach. (lines 317-337).

Line 228: What are "colder" colors?

In my mind white was "colder", which is indeed not straightforward when the opposite color is black. We rephrased to just using highly or poorly reflective surfaces to describe the figure. (line 327).

Line 232: Drinkwater et al. 1991 is an important reference. But, I'm doubting this can be 1:1 used in this context to derive height offsets. Maybe there are some further references?

Yes, I don't think we can't reliably state that it is a 1:1 relationship. We've gone away from giving definite figures and are much more qualitative (lines 330-332). From what we read, no other study have quantified precisely these biases.

Line 232: Point à Observation?

This has been modified in the paragraph (points to observations) in section 4.1.

Line 234: You write can be removed, but I guess it is removed?

We can remove the hooking effect when it occurs in the along-track direction, and it is done indeed. We now explain the hooking procedure in section 3.2 (lines 224-230) and discuss the limits in section 4.1 (lines 333-335).

Line 235: Why cannot it be removed in the cross-track direction? Please add more information.

In the along-track direction, it is possible to mitigate this issue. We apply a rolling filter that retains only the waveform with the highest backscatter coefficient within each window. When a lead enters the satellite footprint, passes through nadir, and exits on the other side, this filter effectively selects the nadir signature. However, if a lead appears briefly at the edge of the footprint without ever reaching nadir, as happens when it lies in the cross-track direction, and is associated with a strong backscatter coefficient, there is no reliable way to determine that the lead was never at nadir. Consequently, such off-nadir reflections cannot be removed in the cross-track direction, because we cannot distinguish them from true nadir returns. We have not added this explanation yet in the paragraph because we thought it was maybe too much information. We can add this the reviewer deems it essential. We reference to Poisson et al in Section 4.1, and Armitage and Davidson, 2014, Quartly et al. 2019 in section 3.1

Line 237-238: Is it like this? I think this sentence is very confrontational. How can you justify using the most recent methods? In the field of classification, for example, there are several other, even newer algorithms that can be used, but only one, Poisson et al., 2018, is listed. I would suggest rephrasing this last sentence.

We agree that this was definitely an overstatement. The original phrasing was likely motivated by the fact that our processing chain relies on the same algorithms as recent polar altimetry products (Auger et al., Veillard et al.), but the sentence did not convey this accurately. We have removed it from the revised manuscript.

Figure 3 caption: The caption is really long and explains MERIS. I think this text should be in the main document and not in the caption.

+

Figure 3 caption: "Trajectory of the along-track shown in A." What and where is A? After "along-track" something is missing. Maybe observations?

MERIS description moved to main text in section 4.1. Caption is now purely descriptive. Panel references are corrected. (lines 340-344).

Line 256: What is this "debiasing step"? Peakiness was never introduced.

Peakiness is now defined in Sect. 3.2: "the ratio of the maximum waveform power to the mean power (Laxon, 1994), a measure of surface specularity." The peakiness-dependent correction for Envisat is described in (lines 253-258).

Line 263: What is the mean state value in this context? How is it computed? Is it the same like the mean inter-satellite offsets?

The mean state of CryoSat-2 is the temporal and spatial mean of its SLA (relative to the MSS), south of 50°S. The methodology is now described in lines 260-263.

Line 271: What are the "input correlation scale files". A brief summary would be very useful, since this is a very important point.

We expanded Sect. 3.4 to explain the three OI inputs: correlation scales (control smoothness and resolved scales), signal variance (expected SLA amplitude), and noise variance (trust in individual observations). The fields are in the supplementary information. (lines 266-285).

Line 279: There is no mission included with a native temporal sampling of 10 days. See Table 2. Please explain.

This statement was extremely false. We went too fast and wanted to (incorrectly) reasoned that combining three missions with approximately 30-day repeat cycles would yield an effective 10-day sampling, which is not accurate at all. We've rephrased this statement in lines 281-285.

Line 280: How are the daily files interpolated?

They are produced by the Optimal Interpolation, using all observations within the space-time correlation window.

Line 283: SLA

Corrected to now use only the acronym once it has been defined.

Line 284: CMEMS

Corrected to now use only the acronym once it has been defined.

Line 286: Why not include Jason-2?

Jason-2 does not cover the ocean south of 66°S. To maintain consistency between the domains north and south of 66°S, we chose to exclude Jason-2 from both. However, there are actually not that many regions south of 66°S in the Southern Ocean so it might be a good idea to include Jason-2 in the future, for both open ocean and lead waveforms.

Line 294: reveals → reveal

Corrected (line 358).

Line 304: Do you mean Fig.4 e?

Yes. However, we ended dropping this panel from Figure 4 (now Figure 5).

Line 307: Could you give some more information? How is the climatology computed and what is the Armitage et al., 2018 -180 cm MDT contour?

We have now dropped this paragraph and the climatology comparison from the paper.

We use an MDT contour to delineate the ACC region from the subpolar Southern Ocean. In the original manuscript, we relied on the MDT from Armitage et al. (2018), using the same contour as Auger et al. (2022b) to define the subpolar domain. In the revised version, we have switched to MDT_CNES/CLS22 (Jousset, 2023) for consistency with the SO_SLICE reference surfaces. The new contour value of −1.05 m was determined empirically to closely match the previous delineation. This is described in lines 350–351.

Line 310: Check brackets of Auger et al.,2022b. What was described in Auger et al.,2022b? Could you please add some short sentences highlighting the main findings.

We have now dropped this paragraph and the climatology comparison from the paper. But in a nutshell, we find the same seasonal cycle in the two studies.

Figure 4: In general, the printing quality of the figures is poor. Maybe this is related to the fact of a preprint. But it is hard to spot details. The term "product" is somewhat unfortunate, as CMEMS is also a product for example. A separate term would be preferable for better understanding.

We've talked about the printing quality of the figures in the general comments. The product is now called "SO_SLICE" in the caption. (lines 381-383).

Figure 5 caption: See comment Line 307.

We dropped this paragraph and the associated figure.

Figure 6: In Figure 1 the application of CryoSat-2 data starts in 2010-07. Why is the time axis limited to end-of 2010. The transition of Envisat nominal phase to Envisat extended phase would be quite interesting, since the orbit of Envisat changes dramatically.

The time axis has been extended to show the full CryoSat-2 record. There is not a long overlap with Envisat nominal phase so it is hard to say if there is a change or not with the CryoSat-2 comparison. However, when plotting the timeseries of average value or variance there a no changes between the two phases. (Figure 7).

Line 347: Which DUACS dataset? I thought the optimal interpolation was also done by DUACS? What are you comparing here? CMEMS vs. your product?

We meant the CMEMS dataset, now C3S. All references to "DUACS" as a comparison product replaced with "C3S." Within this section (4.3.2) we are comparing SO_SLICE vs along-track data. But the argument here is that SO_SLICE compares well with C3S in the ACC region, so if the comparison metrics in the ACC region and in the subpolar Southern Ocean matches, this is indirectly further validates SO_SLICE and the lead processing.

Line 345: RMSEs → RMSE values

Corrected. (line 475).

Figure 7 caption: Figure 7 caption: "The with…" à remove with

We meant "the white dotted line", forgot the "e." Corrected. (line 482).

Line 380: SLA

Corrected. (line 492).

Line 382: "Panel a" → Fig8. a?

Corrected. To Figure 9a.

Line 389: REF??

Sorry about that. Removed.

Line 390: "and its orbit results in non-uniform sampling with temporal gaps" à What do you mean by this?

This is illustrated in Fig. 9b: CryoSat-2's long-repeat orbit means uneven daily spatial coverage with gaps between tracks. It was discussed in a previous comment as well (comment on line 103). We discuss this briefly in lines 499-505.

Lines 390–392: Please add a reference.

This is something we show with Fig. 9 and discuss in lines 499-505. We did not include a reference for now but can include the ones mentioned in the answer to the comment on line 103.

Line 445-446: "correlation of 0.52". I wouldn't say this is "well captured". It's a correlation of around 50%, which is more moderate.

The reviewer has a point. While we would argue these correlations are statistically significant, "well captured" was too strong for r = 0.52. We now describe them as "statistically significant". Line 546.

Figure 9 caption: The caption is very long. Maybe it can be shorten or the plots shown are limited to the highlights?

Figure 9 is now Figure 10. The caption has been shortened (lines 571-576).

Section 4.2.5: Why is this comparison product (Dragomir et al., 2024) not mentioned in the introduction? This section seems somewhat out of context, as a very detailed comparison with CMEMS was made previously. This could somehow be combined, as this is also a comparison with a recent altimetry product. This is a completely independent validation, which is particularly interesting and should be given more attention.

We agree it appeared without sufficient context. Dragomir et al. (2024) is now introduced in the introduction and in Sect. 2.2. The comparison section (Sect. 4.3.5) now includes both Dragomir et al. and Auger et al./Veillard et al., providing a more comprehensive intercomparison. (lines 593-610).

Line 476: SLA

Corrected to now use only the acronym once it has been defined.

Line 480: SSB

Corrected to now use only the acronym once it has been defined.

Line 485: "some mesoscale activity". What do you mean by "some"?

This was definitely too vague. We've now introduced a quantification of energy across scales with the PSD analysis in section 4.2. The paragraph in the discussion now refers to it. (line 665-667).

Line 487: What is the DUACS reference product? CMEMS?

We have now switched to C3S and use C3S instead of DUACS throughout the paper.

Line 490: Delete "then."

Corrected.

Line 491: What exactly do you mean by "independent"? You use gridding strategies from DUACS.

We were talking about the individual satellite missions used in SO_SLICE. We've rewritten this part of the paragraph and use the term "Despite differences in instrumentation and orbits" instead of independent. We highlight that despite those disparities, the mission are coherent on scales larger than 300km. (lines 671-677).

Line 492: What do you mean by "global processing strategy"? You don't process globally.

We meant the processing chain in its entirety. However, we've dropped this sentence from the paragraph. (lines 669-677).

Line 494: comparable to what?

Comparable between open-ocean along-track data and lead along-track data. Clarified in line 681.

Line 496: Add a new line after Southern Ocean.

Done.

Line 501: Once again, it is very vague to compare correlations based on two completely different regions and data sets.

The reviewer is right, we were perhaps reaching a bit with this comparison. The comparison with Armitage et al. (2016) Arctic correlations has been removed. The only downside is there are no comprehensive altimetry/in-situ comparison in the Southern Ocean. A comparison with one BPR and one tide gauge was done by Veillard et al. The in situ validation now stands on its own (lines 687-697).

Line 508: ":"?

We have removed this sentence in paragraph lines 687-697.

Line 516: "considered" as "a"

As was to be added after considered but we've removed this sentence altogether.

Line 521: RMSE

Corrected.

Line 527: a "," is missing.

Added in line 723.

Line 528: Which other observing systems?

Added in brackets (in-situ and other satellite observations) line 724.