Review of manuscript: **High-Resolution Sea Surface Height Anomaly from Nadir Altimeters: Processing Improvements and Spectral Consistency with SWOT by Gourdeau *et al.***

This study presents improvements in the processing of high-rate (HR) along-track nadir altimetry data from Jason-2, SARAL/AltiKa and Sentinel-3A, and assesses their spectral consistency with SWOT KaRIn observations in the region south of New Caledonia, a SWOT Cal/Val site with intense mesoscale and internal tide activity. The paper addresses an important knowledge gap in the processing and interpretation of high-resolution altimetric data. By extending the observable range of nadir altimeters towards smaller scales, it helps bridge the observational gap at fine scales both for the multi-decadal period before SWOT and for the possible gap after the end of the SWOT mission. In addition, the resulting SSHA_HR dataset provides a valuable reference for intercomparison with the new SWOT observations. The detailed description of the processing and spectral methodology is appreciated and will be useful to the community. I recommend that this manuscript be considered for publication after revisions addressing the following comments:

General comments

1. The manuscript is, in some sections more than others, quite technical, which may make it difficult to follow for readers who are not specialists in nadir altimetry. Given the broad readership of *Ocean Science*, I suggest that the authors consider improving the accessibility of the manuscript, particularly in the presentation of some methodological parts and results. This does not necessarily imply reducing the level of technical detail, as some aspects would benefit from further explanation. Rather, it may be useful to reconsider which technical details are essential for understanding the study and should be included and clearly explained in the main text, and which could be moved to an appendix or supplementary material to improve the overall clarity and readability.

2. SWOT data comments:

    1. It is understandable that, as is often the case with studies using SWOT data, a new product version may become available by the time the manuscript is ready for submission, making it challenging to keep the analysis up to date. However, given the potential differences in energy levels at small spatial scales between versions 2.0.1 and 3.0.0, I would encourage the authors to assess whether the main results and conclusions remain consistent when using the newer version. Depending on the magnitude and relevance of the differences, a

comparison between the two versions could be included either in the main text or in an appendix/supplementary material.

2. SWOT also carries its own nadir altimeter, which provides data collocated in time and space with the KaRIn swath. Why was it not used as a reference alongside the other nadir data?

3. Discussion and conclusions: This section would benefit from restructuring to make it clearer to the reader, for example, by separating a summary of the main results from a discussion of their implications, limitations and outlook/future work. Specifically:
   1. The main results should be linked to the figures and tables where they are shown.

   2. References are missing, particularly in the first paragraphs.

   3. The discussion of how the results might apply or differ in other regions is missing. The SNC region is highly energetic in both mesoscale and internal tide activity, which directly affects the signal-to-noise ratio and therefore the observable wavelengths.

   4. The SWOT comparison is limited to one swath during the fast-sampling phase (March to July 2023, ie., austral autumn-winter). The authors should discuss how the results might differ in other seasons, given the internal tide seasonality shown in Section 4, and whether data from the SWOT science phase (21-day orbit), which covers all seasons, could be used to test the robustness of the comparison.

   5. A paragraph on limitations (e.g. MSS errors at small scales, the absence of an independent validation of HR vs LR) and outlook/future work (e.g. could other missions such as Sentinel-6, Jason-3 and CryoSat-2 be used?, possible to do this analysis with data from the SWOT science phase, that although better spatial coverage, lower temporal one?) would strengthen the section.

Specific comments

* Section 1:

L47-49: Please add some references to papers or mission documentation.

L60-62: Please add references.

L65-L66: "… because of the larger noise level of the SSH linked to the SSH PSD signal decrease with increasing wavenumber." This sentence is hard to follow and would benefit of some clarification. Please rephrase so it is clear that noise dominates at high wavenumbers because the signal PSD drops below the noise level.

L71-74: Please add references.

L78: It is mentioned the usefulness of using SSHA HR for comparison with SWOT data during the fast-sampling phase, with which I agree, but I am missing some mention in the manuscript of the possibility of validation of SSHA HR with other observations?

L80-81: Although mentioned later in the text, please make clear that one SWOT swath (T4) crossed the region daily during the fast-sampling phase, and that only its last four months of data are available (March-July 2023).

<u>* Section 2:</u>
L180: Please give the spatial resolution of the CNES-CLS15 MSS grid. Also, why wasn't a more recent version of the MSS used (CNES CLS 2022)?

L184: "On the other hand, it avoids interpolating the along-track data onto a fixed track." Please could you clarify and give more details on this points.

L184-186: The authors note that MSS errors may affect the uncharted S3A tracks at 30-80 km wavelength. This is precisely the range of the S3A observable wavelength (Table 3) and of the SWOT–S3A comparison (Fig. 5). Could the authors please comment on this?

L195: Specific handbook references could be given, and also included in reference list. (Including documentation links in L119 and L121, …).

L231: The detailed description of the wavenumber spectrum computation is very much appreciated. Only a few points might be missing:

(i) Could the authors comment on possible spatial aliasing in the spectral estimates?

(ii) Please clarify whether the 500 km segments are further split into sub-segments within the scipy.signal.welch function and, if so, their length and overlap.

(iii) How sensitive are the results to these methodological choices? For example, would a different window (e.g. Hann instead of Tukey) change the reported spectral slopes, given that steep spectra are prone to spectral leakage?

L239: “The spectral uncertainty is estimated for a 95% significance level based on a chi-squared test.” This is mentioned here, but is not shown in any of the spectral plots as an envelope of the average spectra. Please include it.

L282-283: “The classical 70-250 km mesoscale wavelength range has been revisited to limit the contamination of mesoscale signals by internal tides and mesoscale slopes are calculated over the 100-250 km wavelength range.”. Since internal tide signatures are reported up to ~150 km elsewhere in the manuscript, the 100-250 km range may still be partly affected by internal tides, at least in summer. Could the authors comment on this, or test how sensitive the slopes are to the lower bound of the fitting range (e.g. 120-250 or 150-250 km)?

L286-287: “It sets the minimum size of dynamical structures that altimetry would statistically be able to observe with a signal-to-noise ratio greater than 1”. The SNR = 1 threshold is a common convention (e.g. Dufau et al., 2016), but at this wavelength noise and signal have equal variance, so individual structures near the OWL are not reliably detectable in single passes. The OWL describes a statistical (spectrally averaged) property rather than the size of structures that can be observed individually. Please clarify this in the text, and consider mentioning how the OWL would change with a stricter threshold (e.g. if used a higher SNR threshold (>1), this would increase the OWL). The relative improvement of HR over LR does not depend on the threshold, but statements on absolute resolution, such as the “twice the diameter of an eddy” interpretation, do.

<u>* Section 3:</u>
L292-293: “We look at the averaged spectra within the regional box (shown in Fig. 1a).” The regional spectra are averages over the box in Fig. 1a, but the number of tracks, segments retained (after the 75% valid-data criterion) and spectra averaged is not reported per mission, only as a range (1000–2200). Please provide these numbers. The authors should also discuss whether the different spatial coverage, track orientation and time periods of each mission within the box could contribute to the differences between their spectra.

L299-300: “For wavelengths below 70 km, the differences between SSHA_LR and SSHA_HR_EDIT data are particularly well marked for J2.” Please quantify these

differences, e.g. as a PSD ratio at selected wavelengths or with reference to the error levels in Table 3.

L300: “The filtering step (SSHA_HR_FLT) modifies the shape of the spectra for wavelengths up to 50-80 km”. Since the filtering step is described as outlier removal (Section 2.2.2), please explain how it changes the spectra out to 50–80 km. Is this due to the removed points and the subsequent gap filling?

* Section 4:
L340-353: Many of these details should be given earlier in the manuscript.

L345:“variability in SSH up to 10 cm rms (Fig. 4a)”. It is not clear how this value is derived from Fig. 4a. Is this the correct figure reference? Please guide the reader to better follow the results discussed.

L347-355: Please clarify details on how the model spectra were retrieved for fig. 4. e.g., model data interpolated onto SWOT S3A tracks? If so, how? Same spectral methodology described in L231-237 applied too?

L355-356: “Bendinger et al. (2025) illustrate how SSH wavenumber spectra in tidal beam propagation 355 direction differs from the ones in along-track direction because of strong anisotropy in the internal tide signature.” The authors note that the altimetric tracks are not aligned with the internal tide beam and refer to Bendinger et al. (2025) for the anisotropy of the internal tide SSH signature. Since J2 (66° inclination) and SRL/S3A (sun-synchronous) tracks cross the south-westward tidal beam at different angles, could part of the differences between the mission spectra (Fig. 3) come from track orientation rather than noise or processing alone? It would be useful to discuss this, Could there also be an impact from whether it is an ascending or descending track as they would cross the beam differently? The model sampling already done along S3A tracks (Fig. 4) could be extended to J2 and SRL tracks. The 2D SWOT data would also allow direction-dependent spectra to be computed from observations, providing an observational test of the anisotropy shown in Bendinger et al. (2025) (if the across-track dimension not too small for this). If relevant, this could be mentioned in the Discussion as a perspective.

L369: “unbiased spectra are presented on Fig. 3d". Unbiased spectra not shown in fig 3e?

L379: Please define the specific months used to define the seasons for the analysis.

L385: the authors refer to the slope of k-4 (QG) observed in fig. 4, but only k-2 and k-3.6 are shown. Please consider which slopes would better help the reader. Also please try to better guide the reader with the slope references, e.g. k-11/3 and k-3.6 reference slopes.

L395-402: Please consider moving some of the details to the introduction and/or the discussion.

L406: Pleas indicate these 40 and 70 km peaks with for e.g. vertical lines in fig. 4.

L407-408: The two peaks at 70 and 40 km are attributed to internal tides, although the authors note that their "significance could be questionable", and the supporting comparison with a non-tidal simulation is "not shown". Please add confidence intervals to Fig. 4 so that the significance of the peaks can be assessed, and consider showing the non-tidal comparison (e.g. in a supplement).

* Section 5:
L427: "The 2km SWOT product is the "optimal" product in terms of signal/noise to analyse meso and submesoscale dynamics in the open ocean." Why? Could the authors please further develop this point and give references where necessary. Is this point on the "optimal" product still true for v3.0 of the 2km and 250 m data?

L451: Clarify that SWOT data is available only during that last period between March and July.
Figure 1: Would be useful to have a legend in fig. 1a, and colorer labels where necessary.

L455: In L373-374 mentioned why focus on S3A, but it would be helpful to further clarify here how this is best for the SWOT comparison. More importantly, the period of the S3A spectrum in Fig. 5 is not stated. If it is based on the 2016–2021 record, the comparison mixes different years and seasons (the SWOT data cover March-July 2023), although Section 4 shows that S3A slopes vary with season.

L465: Below 0.03 cpkm (i.e. higher wavenumber values), isn't the S3A demonised spectra still a bit noisy?

* Conclusions:
L472: "(SSHA_LR). Their effective resolution is limited to spatial scales with wavelengths > 100 km". Isn't this inconsistent with what you mention in L61 in the introduction "When comparing spectra from different altimetric technologies at

scales of 15-200 km, a strong gap was identified 60 between the 40-70 km effective resolution of the low resolution 1 Hz"?. Also, could be relevant to include references to other studies that defined effective resolution from SSHA_LR too.

L479: "the HR products provide more accurate data even at scales as large as ~100 km compared to LR data". This statement is based only on spectral error levels and observable wavelengths (Table 3), which measure noise relative to signal rather than accuracy against an independent reference. Maybe could rephrase it to lower noise or higher signal-to-noise ratio. Also, it would be useful to reference the respective figures and tables for the statements made in this section.
L485: "A mesoscale slope around -3.7 extends down to 60 km wavelength." Inconsistent with L370 : "For all satellite missions, mesoscale slope extends down to 80 km wavelength.". Please clarify.

L505-508: It could also be mention that this HR dataset can maybe benefit the grid L4 altimetric products? I.e., how the HR processing could also feed into operational products (DUACS/CMEMS)?

Figures 3-5: Would be helpful to also add the corresponding wavelength on the upper or lower axis of the figures, to help the reader, specially when specific wavelengths are mentioned in the results.

Figure 5: Although it could be deduced, please distinguish the 2 k slopes so they are not the same

Technical corrections

1. Review use of uppercase, e.g. L24 "Surface Quasi-Geostrophic", L426 "Unsmooth", …

2. Following the journal's instructions: "Ensure that your figures' colour schemes allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator and revise the colour schemes accordingly". Please review this specially for figure 1.

3. L25: k-2 —> k^{-2}

4. L51: KaRIN —> KaRIn.

5. L80: Inconsistency in abbreviations of CalVal and AdAc throughout the manuscript.

6. L294-L300: Please review paragraph spacing/separation.

7. L296: Fig. 3abc —> Fig 3a-c or a, b, c.

8. Table 3: For clarity, Would help to have sub columns for LR/HR_EDIT/HR_FLT. Also, please remind the reader of the abbreviations in the caption. Also missing units here : 100-250 km.