REVIEWER 1

Can satellite altimetry observe coastally trapped waves on sub-monthly timescales?

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Revised version

General comments

The author has addressed all my comments satisfactorily. In response to the first reviewers comments, the EOF analysis has been replaced with a complex EOF analysis which is better suited for propagating patterns. I think this is very beneficial for the analysis and characterisation of the observed CTWs.

I have no major comments and only a few minor comments that, I think, might help to improve the clarity of some of the figures.

Specific comments

Section 3.3 Suggest to rename this section to “Radon transform and phase speed computation”

Suggestion accepted

Equation 6: Xc is not used in the subsequent equations, so why define it here?

The definition of Xc is correct in Equation 6, but should have been used consistently in Equations 7 and 9. This is now corrected, thanks for spotting the issue!

Figure 3c: move legend to the left for better visibility?

Suggestion accepted

Line 194: anti-correlated but not statistically significant, or only partly!

I rephrased as “Shelf locations north of 31 degrees S are anticorrelated with Bermagui at lag 0, although some of these correlations are not statistically significant,…”

The following suggestions might also be just personal preference but I think they could contribute to more clarity in the figures.

Figure 4: you actually only need one colorbar (or one per row) as they all have the same range.

I prefer to keep the colorbars in each subplot. In my experience, subplots are often copy-pasted or cropped in presentations or other contexts without their corresponding colorbars, which can lead to misinterpretation.

Figure 5 and 6 (upper row): I would find it more intuitive to have the distance as the y-axis (kind of mirroring the coast) and time as y-axis. Also, as in Figure 5, one colorbar might suffice.

I prefer to keep time on the y-axis to facilitate easier comparison with key references in the literature, particularly Woodham et al. (2013). Their Hovmöller plots, which I include below as a screenshot, also adopt this convention.

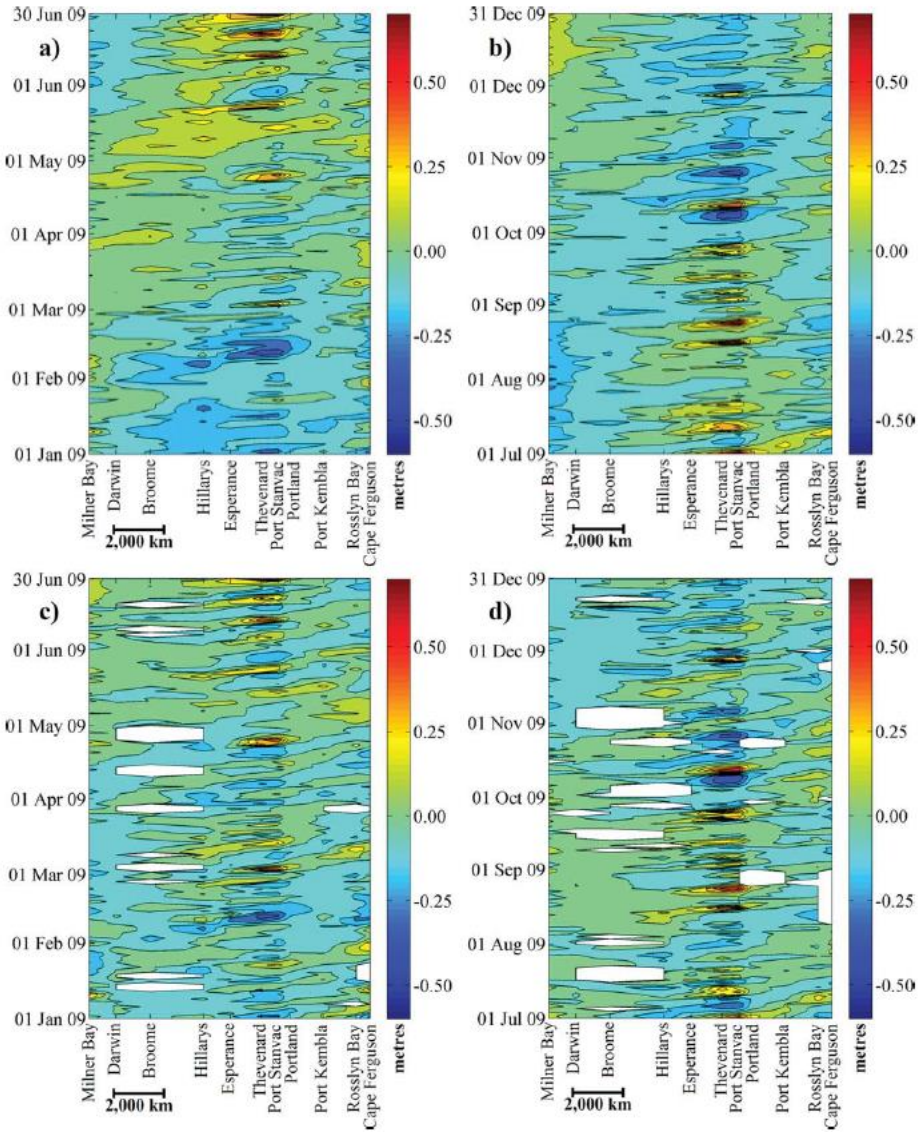


Figure 6, lower row: Would it be more intuitive to show the phase in degrees? And, particularly for the right panel, how about keeping the phase between 0-360 (thinking degrees here) to even more emphasize the oscillations?

While this wrapped representation can visually emphasize oscillatory behavior, this is already clear from the Hovmöller diagram. My analysis in the text related to the lower row relies on the unwrapped phase to compute key physical quantities such as wavelength and period. Unwrapping the phase avoids artificial discontinuities and allows for accurate spatial and temporal derivatives, which are essential for quantifying wave propagation characteristics.

Figure 7: same as for Figure 4.

See previous comments

REVIEWER 2

The revised version of the manuscript shows clear improvements compared to the original submission. The clarifications introduced by the authors significantly enhance the readability of the text and the methodological explanations are now much easier to follow. A notable strength of the revision is the integration and discussion of the Complex Empirical Orthogonal Function analysis. This addition provides a more robust assessment of the spatio-temporal characteristics of the coastally trapped waves and demonstrate the propagating nature of the detected signal. Overall, the manuscript is well-constructed, and I recommend publication after the addressing of the following concerns.

Introduction :

Please better explain why accurately detecting CTWs is crucial for describing and predicting coastal circulation, upwelling processes, and associated ecosystem impacts. This aspect is currently mentioned in the conclusion but should also be highlighted earlier to motivate the study.

The following lines are added to the introduction: CTWs can interact strongly with local circulation patterns, inducing alongshore currents and significant water mass displacement (Bailey et al., 2022). By causing vertical displacements of the pycnocline, CTWs affect coastal upwelling patterns and thereby influence nearshore productivity (e.g., Echevin et al., 2014). Monitoring and understanding this phenomenon can therefore be useful for improving predictions of biological productivity in coastal systems (Körner et al., 2024).

Data :

- You add that the model is eddy-resolving, but a resolution of 10 km makes the model eddy-permitting in the study region.

The statement is taken from the related references and it is likely related to the “near-global” coverage. However, to avoid misunderstandings, I have removed “eddy-resolving”.

SWOT Coverage and Sampling :

- Please indicate where the SWOT swaths intersect the continental shelf in your study region. Adding this information directly in Figure 1 would greatly help interpretation.

We are using data from the 21-day science orbit phase of the SWOT mission. During this period, SWOT provides near-global coverage of the ocean between approximately 78°N and 78°S. As a result, nearly all of our study locations are intersected by SWOT swaths. While the spatial coverage is extensive, the primary limitation of SWOT during this phase is temporal: each location is revisited only once every 21 days. Therefore, the constraint is not spatial coverage, but rather the temporal resolution of the observations.

- In Figure 2 (Coffs Harbour), does the time series correspond to a location directly sampled by SWOT? If so, it would be useful to compare results with a location not sampled by SWOT to assess sensitivity to swath coverage.

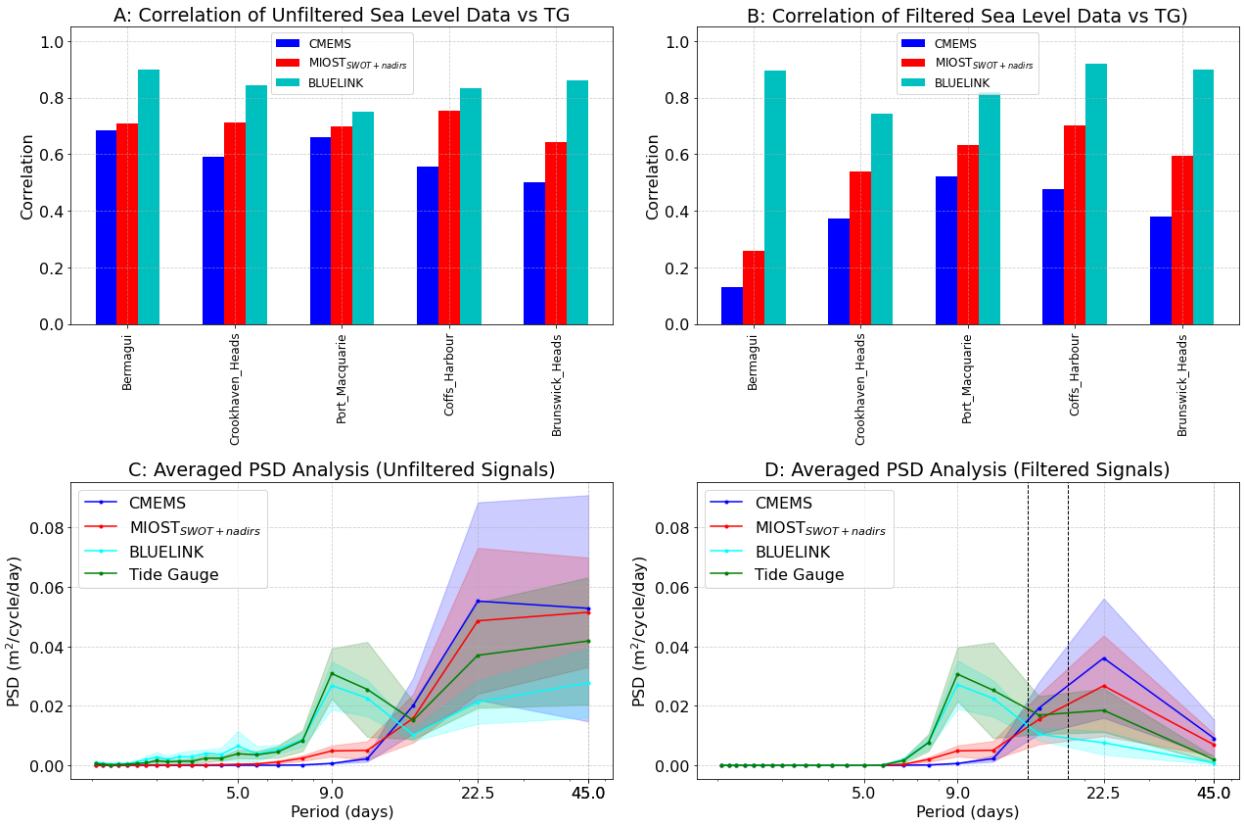
Please see the answer to the previous comment

Spectral Content

- The CMEMS and MIOST time series appear to show virtually no energy at periods shorter than 20 days. How do you reconcile this with the periods of 14 days (MIOSTSWOT+nadirs) and 18 days (CMEMS) obtained from the CEOF computation?

I respectfully disagree with the statement that there is “virtually no energy” at periods shorter than 20 days. To clarify this point, I have produced a special version of Figure 3D for your benefit, in which I have added vertical lines at 14 and 18 days.

As shown in this version, both datasets do exhibit spectral energy at these periods. Therefore, I do not see a contradiction that requires reconciliation.



-Both the spectra and Figure 3 show a peak around 22.5 days. This feature is not currently discussed. Please clarify whether this peak corresponds to CTWs or to another process.

The apparent peak around 22.5 days is not a physical feature, but rather an artifact introduced by the filtering process. Specifically, when generating the PSD of the filtered signals, the last discretized period value before the cutoff is 22.5 days. This can give the impression of a peak at that location.

To clarify, this feature does not correspond to coastally trapped waves (CTWs) or any other physical process. This can be appreciated in Figure 3C, which shows the PSD of the unfiltered signals: no peak is visible around 22.5 days in that panel. The presence of the feature in the filtered PSD is purely a consequence of the bandpass filter design and the discretization of the frequency axis.

-In Figure 4, the correlation with Bermagui does not appear to be significant for any of the products, even though Figure 8 shows correlations exceeding 0.8. Could you provide an explanation for this discrepancy?

There is no Figure 8 in the manuscript, but I assume the reviewer is referring to Figures 3A and 3B, where the correlations are presented. Figure 4 shows the correlation of the filtered signals, which corresponds to the analysis in Figure 3B. As shown there, the only dataset with a strong correlation at the closest shelf point is BLUELINK, which is consistent with what is seen in Figure 4. Neither CMEMS nor MIOST$_{SWOT+nadirs}$ shows correlations exceeding 0.8, not even in the full signal analysis shown in Figure 3A. Therefore, I do not see a discrepancy between the figures.

-Line 213: you state that the first CEOF mode explains 71% of the total variance of the filtered signal. Please clarify whether this refers only to the filtered signal or to the total variance.

I changed “total variance” to “variance of the filtered signal”

CEOF modes : I agree with your answer and is not necessary to add the analysis of modes higher than one in the manuscript.

Conclusions I am globally satisfied by the answers given by the authors to my first review and I recommend the publication after minor corrections.