Response to reviewers

Reviewer(s) 1: Robert Guza & Kilian Vos.

Given that the review itself is not structured, we respond as clearly as possible by addressing the comments within a single block rather than providing a conventional detailed point-by-point reply, which would require imposing an argumentative structure that the review itself does not provide. Moreover, several statements are presented without any supporting argument. For this reason, we will also take on part of the reviewers' role by attempting to infer the reasoning that may underlie some of their statements in order to respond as constructively as possible.

First, we would like to remind the reviewers that our study investigates spectral triad interactions between the shoreline signal and wave forcing. Our dataset and methodology are designed to address large-scale variability in shoreline position, focusing on regional to global and seasonal to interannual dynamics. This approach is fundamentally different from site-specific surveys (which we also address in other studies, such as the recent local multi-site systematic shoreline detection study by Graffin et al., 2025a), as it aims to capture broad large-scale patterns. Hence, the paper develops a substantial methodological framework that is explained in detail and presents a mechanistic explanation for the emergence of the semi-annual wave signal based on independent datasets, including a cross-comparison with an *in situ* site and large-scale regional high-resolution tide-corrected dataset (from Graffin *et al.*, 2025b). Yet, despite the conclusions being verified on these different datasets, the entire review is built on the claim that the global shoreline dataset is corrupted by tides and that, therefore, all conclusions of the paper must be invalid. This reasoning strikes us as both an overly simplistic and dangerously abrupt shortcut.

The opening paragraph of the review revives a debate that has become increasingly counterproductive. The reviewers rely on the *Matters Arising* by Warrick *et al.* (2024) which, by nature, is not a quantitative analysis but rather an opinion critique, and has been addressed in the *Reply* by *Almar et al. (2024)*. None of the arguments presented in that article (while philosophically legitimate to raise) are supported by quantitative demonstrations using the publicly available dataset. As such, they cannot reasonably be invoked as definitive evidence against subsequent studies using the dataset. Moreover, the introductory argument of this review essentially states that because the dataset has been qualitatively criticized in another paper, all conclusions of our study must therefore be invalid. In our view, this reasoning does not constitute strong evidence.

Our objective here is not to provide a detailed critique of Warrick et al. (2024). However, it is important to recognize that the logical shortcuts taken in that reply are the same as those reproduced in the present review. The key point, as emphasized by Almar *et al.* (2024), is that tides represent only one component among many that modulate water level. The waterline (used here as a dynamic proxy for shoreline position) integrates multiple contributions, including tides, wave runup, and sea-level variability. Acknowledging that one component of water-level variability can introduce noise or bias does not invalidate the entire dataset at all spatial and temporal scales. Warrick and colleagues raise a legitimate point regarding tidal influence, but the extrapolation drawn from it is unjustified. A dataset may contain noise without being unusable, provided that appropriate methodological precautions are taken. Here, to emphasize regional to climatic signals under the scope of our study, the resulting satellite-derived time series are linearly detrended and further aggregated over 3° coastal segments and 3-month intervals. One should not use this dataset for local beach study as it was not generated for this purpose (e.g. Global Climate model results for a region are not representative for a local beach and should not be used as such). This regional to seasonal spatiotemporal filtering effectively removes high-frequency variability associated with rapid total water level variations (tide, runup, storms) and local aspects (such as a regional climate models over 100 km to 1000 km footprint versus weather models of for short term aspects at the scale of a city). Although Landsat imagery has a spatial resolution of 30 m, this does not directly translate into positional accuracy. Satellite-derived shoreline positions typically achieve an accuracy of 10–15 m (Graffin et al, 2025b, Vos et al, 2023b), while errors below 5 m can be obtained under optimal conditions(Sánchez-García *et al.*, 2020; Cabezas-Rabadán *et al.*, 2020). The aggregation of multiple observations substantially enhances robustness. Assuming normally distributed errors, the minimum detectable shoreline change at the 95% confidence level is given by:

$$\Delta X_{95\%}=1.675*2^{0.5}*cRMSE*N^{-0.5}$$

where cRMSE is the centered root-mean-square error and N the number of independent samples. In this study, regional and seasonal aggregation yields N≈1000, derived from independent observations within the 500 m alongshore buffer (at 30 m resolution), combined over three monthly periods and accounting for a median spacing of 0.27° between transects. With a cRMSE of 15 m, the resulting detectable regional shoreline change is on the order of 0.5 - 1 m (Graffin et al, 2025b).

The reviewers also refer to Graffin *et al.* (2025a). Indeed, the dataset used in their study is tidally corrected and is both more recent and of higher quality than the one used here. For

precisely this reason, we performed a direct comparison between the two datasets along the west coast of North America (Fig. 1). This comparison provides a direct test of the reviewers' claim. If tidal contamination dominates the signal present in the global dataset, the coherence between the two datasets should collapse once compared with a tidally corrected product. Instead, the comparison shows a strong agreement, with correlations of R = 0.82 for the total signal and R = 0.78 for the interannual component. The fact that the interannual variability remains highly consistent between the tidally corrected shoreline dataset (HRTC) and the non-corrected global dataset (GlobC) indicates that tidal effects primarily contribute high-frequency noise rather than generating or dominating the interannual signal itself. For this reason, the comparison with Graffin et al. (2025a) ultimately strengthens rather than weakens the robustness of our analysis. We therefore struggle to understand the reviewers' statement that *"The authors cite Graffin et al. (2025) as validation of their SDS dataset, but in that paper the waterlines were indeed tidally corrected"* since the very purpose of this comparison is precisely to evaluate whether tidal contamination fundamentally alters the signal of interest. The results show that it does not. Microtidal beaches typically exhibit limited tidal bias, whereas macrotidal environments may experience stronger contamination (Vos *et al.*, 2023). Moreover, it is important to note that tidal corrections can themselves introduce uncertainties at wave-dominated microtidal sites, where the tidal signal is small relative to other sources of shoreline variability (Vos *et al.*, 2020), making the application of tide correction a non-trivial issue.
Following the same line of reasoning, the reviewers' statement that *"Nonlinear triads identified in the global dataset may be a tidal artifact in the satellite shorelines"* is largely misplaced. Our study analyzes spectral interactions within the signal. Here, the triadic relationship tested in the manuscript is:

$$f1 = f2 - f3$$

where f2 corresponds to the semi-annual component of wave forcing (derived from ERA5), f1 is the low-frequency shoreline residual obtained after removing known external forcings using the CEOF framework, and f3 is the semi-annual component extracted from the shoreline signal x(t) through wavelet transform (see Appendix for methods validity assessment).

Hence, the observed shoreline signal may be written as:

$$x(t) = s(t) + \eta(t)$$

where s(t) represents the morphodynamic shoreline component and $\eta(t)$ denotes potential contamination related to water-level variability, including tides. In this sense, the semi-annual component extracted from x(t) may indeed contain both contributions (even if already shown to not be significant at the scales considered in this study). In frequency space this simply implies:

$$X(f3) = S(f3) + H(f3)$$

where H(f3) represents the possible tidal contribution at the semiannual frequency. Therefore, the semi-annual shoreline signal could, in principle, contain a mixture of morphodynamic variability and a weak long-period tidal contribution. On this point, the reviewers raise a legitimate theoretical possibility. However, this observation alone is insufficient to invalidate the triadic interaction identified in the manuscript. The detection of a nonlinear triad does not depend solely on the existence of a spectral peak at f3; it requires a specific phase relationship between the three components, such that:

$$\phi 1 \approx \phi 2 - \phi 3$$

The legitimate question is therefore not simply whether a semi-annual component exists in the shoreline signal, but whether that component carries the correct phase relationship with the wave semi-annual signal (f2) and the low-frequency shoreline component (f1) required by the triadic condition (tested through coherence metric R; see manuscript). Hence, if the semiannual shoreline component were primarily imposed by a weak astronomical tidal constituent independent of the wave forcing, then that tidal signal would also need to systematically exhibit the appropriate phase relationship with both f2 and f1. This represents a much stronger requirement than the mere presence of a semi-annual spectral peak.

It is worth noting that the magnitude of the solar semi-annual tidal constituent (Ssa) is typically below ~4 cm globally (Lyard *et al.*, 2021; see Fig. 2). Even assuming a very low beach slope ($\beta \approx 0.01$), this would translate into a maximum horizontal shoreline displacement of approximately 4 m. This magnitude remains small relative to the typical uncertainty of satellite-derived shoreline datasets (~10–20 m) and therefore cannot dominate the observed semiannual shoreline variability as it is drawn within noise measurement.

More importantly, the reviewers' argument conflates the presence of a weak spectral component with the ability to generate structured nonlinear interactions. While tidal variability may introduce additional variance into the waterline-derived shoreline signal, it cannot by itself produce coherent nonlinear triads unless it simultaneously satisfies the required frequency and phase relationships with the other components of the system. Consequently, if the semiannual shoreline component f3 was primarily a tidal artifact, the triadic relationship f1 = f2 − f3 would imply that the low-frequency shoreline component f1 also results from an interaction involving the tidal signal itself. In other words, the reviewers' hypothesis implicitly requires that the low-frequency shoreline variability identified in the residual signal be largely tidal in origin rather than morphodynamic. This interpretation is inconsistent with the known tidal spectrum, which does not contain constituents near the corresponding biennial period. While there is variability in the 6–24 month range, the literature does not specifically highlight a significant peak at 24 months in tide-gauge data. The annual (12-month) cycle is much more common. So the signal at ~24 months we identified in the shoreline residual is strongly unlikely to originate from tide. Furthermore , if the semiannual shoreline component f3 were primarily driven by the long-period tidal constituent (Ssa), one would expect its spatial distribution to follow the known geographical pattern of Ssa tidal amplitudes (Fig. 2). The persistence of the semiannual shoreline signal in regions where Ssa amplitudes are weak, or where its magnitude does not scale with the tidal amplitude field, therefore weakens the hypothesis that the observed f3 component is primarily tidal in origin. One can compare the amplitude-weighted coherence coefficient global map (Fig. 4d; manuscript) with the one of Ssa amplitude (Fig. 2; this response) to notice this is simply not the case.

The reviewers also state that *"The present paper purports to study only micro-tidal beaches (range < 2 m), but **spring** tides in Southern California are about 2 m and 3 m+ in Oregon, and the entire US west coast is shown in Figure 4."*

In our analysis, tidal classification was derived from the global grid of mean tidal ranges provided by FES2022, interpolated onto the shoreline dataset grid. We acknowledge that this detail was not explicitly described in the manuscript and will be clarified in the revised version. However, using spring tide amplitudes to challenge the classification of microtidal environments is not an appropriate comparison. It is entirely possible for a given location to have a median tidal range below 2 m while spring tides exceed this threshold. For the purpose of the present study (which investigates regional spectral behavior across aggregated shoreline datasets) the relevant metric is the representative or mean tidal range rather than extreme spring conditions. Spring tide values are therefore not an appropriate criterion for redefining the tidal regime considered in the analysis.

The reviewers further argue that a tidal range of 2 m combined with a beach slope of $\tan\beta = 0.04$ would produce a horizontal shoreline excursion of approximately 50 m. This represents a theoretical upper bound, not the effective error present in the satellite-derived shoreline dataset. The actual contamination of shoreline positions depends on several factors, including the tidal phase at the time of image acquisition, wave runup, setup, hydrodynamic variability, and the geometric corrections applied during shoreline extraction. Consequently, the value of 50 m corresponds to a maximum potential displacement, not the systematic error associated with the dataset itself. Hence, the argument of "worst case scenario" without any nuance is misappropriately used here.

Another reviewers' argument concerns the comparison of R^2 values between a single *in situ* transect and a spatially aggregated satellite dataset (GlobC). The GlobC shoreline dataset is designed to provide regional-scale averaged behavior, which reduces local noise and allows the identification of large-scale patterns. It is therefore not surprising that correlations between a regionally averaged satellite signal and a single local survey transect may be limited. A rigorous comparison would require multiple *in situ* measurements across a spatially coherent coastal region, averaged over the same spatial extent as the satellite dataset before comparison. Such datasets do not exist.

A more appropriate validation approach is therefore a cascade comparison. In this framework, the GlobC dataset can be compared with higher-resolution satellite datasets such as HRTC (Graffin et al., 2025), which are tide-corrected and spatially more resolved, while being themselves validated against *in situ* measurements. Such a hierarchical comparison provides a scientifically consistent pathway to evaluate the reliability of GlobC. By contrast, directly comparing a spatially averaged global dataset to a single *in situ* transect does not provide a robust assessment of its performance. To make our point crystal clear, here is an example: when comparing a spatial average computed from many shoreline points with a single *in situ* location within that region, both false positive and false negative correlations can occur. This is a direct consequence of spatial averaging and further illustrates why a cascade validation strategy is more appropriate than direct point-to-point comparisons.

Finally, the reviewers note that *"Removing tides (Vos, 2023) yields annual R^2=0.88"*. We agree that tide-corrected datasets yield higher correlations with *in situ* measurements. But this is expected. Indeed, those datasets are specifically designed for local shoreline reconstruction and validation against field surveys, and typically offer higher spatial resolution (which is great; no doubt). The objective of the GlobC dataset is different: it is intended to capture regional-scale shoreline behavior across the global coastline. At present, GlobC remains the only publicly available shoreline dataset with truly global coverage, and its value lies precisely in enabling analyses at this scale rather than reproducing local

shoreline variability at individual transects.
Concerning the argument "*Subsampling tide observations to 8 days (matching CoastSat) splatters aliased tide energy widely over frequency space*", we would argue that subsampling tidal signals at ~8-day intervals can indeed redistribute high-frequency tidal energy through aliasing. However, such aliasing typically produces broadband spectral leakage rather than coherent low-frequency oscillations. Moreover, shifting tidal energy toward the ~2-year band would require an aliasing mechanism producing a coherent low-frequency beat from high-frequency tidal constituents. In practice, irregular satellite sampling redistributes tidal energy over a broad range of frequencies rather than generating a stable oscillatory component. It therefore cannot readily explain the presence of persistent spectral peaks and phase-consistent triadic interactions at multi-month to interannual timescales, nor invalidate its detection. To ensure that we are on the same wavelength, here is a synthetic case (Fig. 3) where a pure semi-diurnal tidal signal (M2 constituent, period ≈ 12.42 h) was first generated using a high temporal resolution time series. The power spectrum of this original signal is shown in the upper panel (black) and exhibits a single narrow spectral peak at the expected tidal frequency (~1.93 cycles per day). The synthetic tidal signal was then subsampled at an interval of 8 days, approximating the temporal sampling of satellite shoreline observations (e.g., Landsat/Sentinel-2). The power spectrum of this subsampled signal is shown in the lower panel (red). Due to aliasing, the high-frequency tidal energy is redistributed into lower apparent frequencies. In this synthetic example, the aliased energy appears around ~0.06 cycles per day, corresponding to periods on the order of 10–20 days. This behaviour is consistent with the Nyquist sampling theorem for which, in our applied case, tidal constituents with periods much shorter than the sampling interval cannot be resolved and therefore fold back into lower frequencies. This aliasing process produces short-period fluctuations (weeks) and not any coherent oscillations at multi-month or interannual timescales. This is now pretty clear that this specific reviewers' argument falls short.
Finally, the statement that "*The present use of tide-contaminated data is both surprising and unacceptable*", speaks for itself and does not constitute a technical argument requiring further discussion.
Moving to the third and penultimate paragraph of the review, the reviewers advance the following argument: *"The residual (deseasonalized) signal analyzed for triads is small and contains only 7% of the original shoreline variance at Torrey Pines (line 277)."* Anyway, it is entirely consistent that the amplitude of the residual signal (which, incidentally, is not merely deseasonalized but has also been processed through the cEOF filtering framework

(see Methods section)) represents only a small fraction of the variance of the original shoreline signal. Once both the seasonal component and the linear influence of known external forcings have been removed, the remaining signal necessarily accounts for a reduced portion of the total variance. It is therefore unclear why this should constitute a methodological problem given that the residual signal retains statistically structured variability. Indeed, the residual time series exhibits significant autocorrelation and a clear spectral peak around ~26 months that rises above AR(1) red-noise expectations (Fig. 4), demonstrating that the residual signal is not dominated by random noise and remains suitable for phase-based spectral diagnostics. That being said, the reviewers do not provide any explanation for their statement. We will therefore refrain from speculating on their reasoning. As it stands, we find the argument unsupported.

The reviewers further argue that the number of cycles is too small, noting that for the *in situ* case, approximately nine cycles of a ~2 year oscillation are contained within the 20-year record. However, no reference is provided to support the claim that this number of cycles is insufficient. The argument therefore appears to rely primarily on opinion rather than established methodological criteria. Conversely, in (geophysical) time-series analysis, the detectability of an oscillatory mode is not determined solely by the number of observed cycles but rather by the effective degrees of freedom of the spectral estimate, which depend on record length, background noise structure, and signal-to-noise ratio (e.g., Torrence et Compo, 1998; Vaughan, Bailey et Smith, 2011; Ghil *et al.*, 2002). Consequently, a limited number of cycles does not in itself invalidate the identification of a coherent oscillatory mode. In our case, the interannual component can be reliably detected despite the limited number of cycles. As a proof, we performed several robustness tests directly on the residual shoreline signal at Torrey Pines (Fig. 5). First, splitting the record into two halves shows that the interannual spectral peak remains present in both segments, although with reduced amplitude due to the shorter records. Second, a "leave-24-months-out" analysis demonstrates that the spectral peak remains stable even when removing segments comparable to one oscillation period. Third, wavelet analysis indicates that energy in the ~20–30 month band persists over substantial portions of the record rather than being confined to a single transient episode. Finally, block-bootstrap resampling yields a median peak period of 25.6 months with a 99% interval of 18–36 months, confirming that the detected variability robustly falls within the interannual band. To conclude, the triad analysis does not rely on a simple spectral peak detection but on phase-coherent triadic interactions. Its diagnostic therefore depends not only on the number of cycles but also on the phase relationships between the interacting components. In that context, less than 10 cycles seem entirely sufficient for detecting coherent phase coupling in the system.

On the reviewers comment that *" Bispectral analysis, widely used to study triad interactions, but not here, shows that nonstationary (transient) triads with low degrees of freedom are statistically noisy"*, we acknowledge that classical bispectral analysis is a widely used tool for diagnosing nonlinear triadic interactions in stationary signals. However, in this study, the present shoreline–wave system is explicitly nonstationary, with transient and time-localized spectral components. For this reason, the main methodology adopted in the manuscript relies on a wavelet-based phase-coupling framework, which is specifically designed to diagnose localized interactions across a large number of coastal records using a unified metric. Statistical significance is assessed directly through the amplitude-weighted phase-coherence metric Rw and associated permutation tests, which evaluate whether the phase relation $\phi1 - \phi2 - \phi3$ is maintained beyond chance fluctuations.

Nevertheless, as an independent consistency check, we applied a targeted cross-wavelet bicoherence analysis to the Torrey Pines record (Fig. 6). This analysis yields high bicoherence values (b^2 = 0.83–0.93, depending on the temporal smoothing window [5-9 months]) at the expected frequency combination T1 = 4.94 months, T2 = 6.08 months, and T3 = 26.30 months, consistent with the subtractive triadic relation $f1 - f2 = f3$ identified in the manuscript. This provides an independent spectral confirmation of the same interaction detected by the Rw framework.

We note, however, that a full surrogate-based significance map for the cross-wavelet bicoherence is poorly constrained in this specific near-degenerate configuration. Because $f1 \approx f2$ and $f3 << f1$, the admissible frequency space satisfying $f3 = f1 - f2$ within the narrow interannual band is extremely limited under the present wavelet parameterization. As a result, the null distribution of frequency-resolved bicoherence values becomes unstable and highly sensitive to sampling. For this reason, we regard the cross-wavelet bicoherence as a supporting diagnostic, whereas the formal statistical assessment of the triadic interaction is provided by the amplitude-weighted phase-coherence metric Rw and its permutation test.

Finally, the reviewers question the aggregation of shoreline observations to a 30 km spatial resolution, arguing that a single reach may include beaches with different orientations and behaviors. This is true. However, this remark appears to overlook the objective of the dataset and the scale of the analysis performed in the manuscript. The purpose of the spatial aggregation is precisely to reduce local noise and small-scale variability, which are known to affect shoreline extraction from satellite imagery. Individual shoreline points extracted at ~100 m spacing can be strongly influenced by local morphological variability, short-term hydrodynamics, and image-specific artefacts. Aggregating observations over larger spatial

units therefore improves the signal-to-noise ratio and allows the identification of regional-scale shoreline dynamics, which is the focus of this study.
It is also important to note that the shoreline dataset used here is not a raw transect-scale product. The underlying observations are first distributed along the coast with an average spatial merging of approximately 0.27°, and are subsequently averaged over 3° coastal segments in order to obtain consistent large-scale shoreline time series. The additional aggregation step applied here therefore operates on an already spatially smoothed dataset and should be understood primarily as a pragmatic regionalization of neighbouring coastal segments. To evaluate this, we performed a sensitivity test by repeating the aggregation using different orientation thresholds (±10°, ±15°, and ±20°). These tests produce slightly different spatial segmentations but yield aggregated shoreline time series that are nearly identical (Fig. 7). The resulting signals exhibit a median correlation of 0.99–1.00 between configurations, with unchanged variance and dominant spectral periods. This indicates that the aggregation procedure primarily acts as a spatial grouping of neighbouring coastal segments rather than modifying the underlying shoreline signal; as it is, already spatially averaged. The robustness of the detected spectral interactions across geographically diverse coastal regions therefore does not depend on the nominal aggregation parameters. The 30 km scale therefore represents an intermediate choice allowing consistent global coverage while maintaining sufficient statistical stability for spectral analyses.

In conclusion, if there are legitimate scientific concerns, we remain open to addressing them. However, we believe that criticism based solely on the fact that our work precedes, differs from, or offers an alternative perspective to that of the reviewers does not, in itself, constitute a valid basis for rejection.

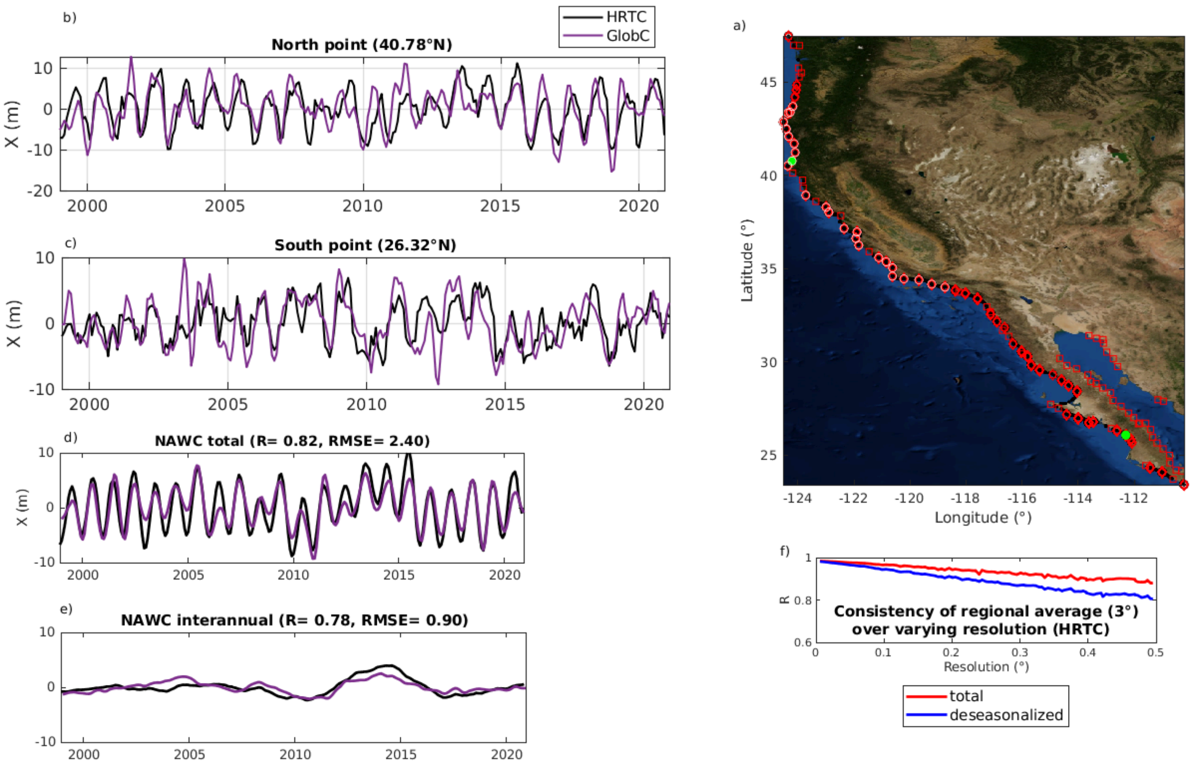

Figure 1: **Figure adapted from Almar et al. (in review).** *Comparison of our global coarse (GlobC) dataset with the tide-corrected shoreline dataset (HRTC). This dataset covers the western coast of North America (NAWC) - see a). black and red dot stands for HRTC and GlobC points respectively, matching locations are shown as stars. North and South locations taken for comparison in b) and c) appear as green stars. White circles show the locations taken for the regional average shown in d) total signal, and e) interannual signal. f) shows the result of correlating regional (3°) average with full resolution of HRTC with same average with reduced resolution (GlobC uses 0.27° median resolution).*

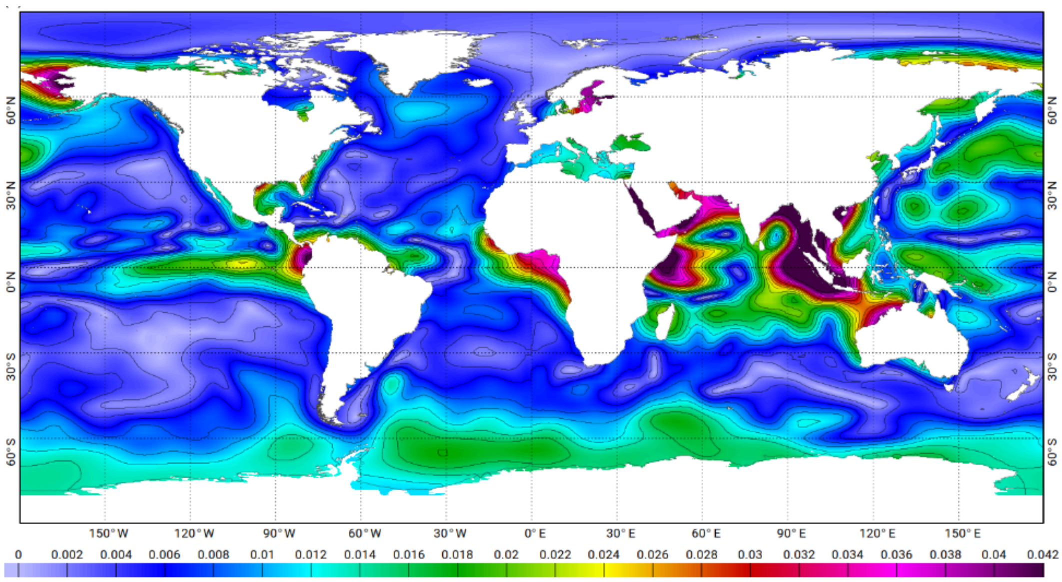

Figure 4. Maps of amplitude in metres of Sa **(a)** and Ssa **(b)** ocean signals estimated from GLORYS2v1 reanalysis. GLORYS2v1 products are free of atmospheric surface pressure effects (i.e. they are not taken into account in the NEMO model forcing and are corrected for in the assimilated SSH data). Consequently, they are comparable to IB-corrected sea level (at Sa and Ssa frequencies) in altimetry and tide gauge observations.

Figure 2: *Map of Ssa amplitude (cm) estimated from GloryS2v1 reanalysis. Figure extracted from Lyard et al. (2021).*

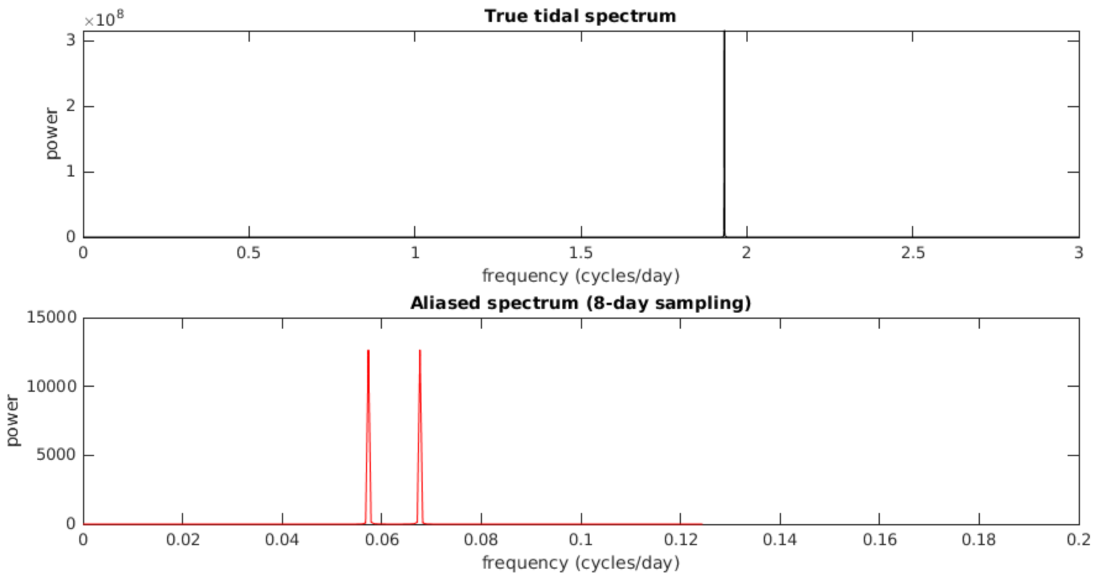

Figure 3: *Synthetic demonstration of tidal aliasing under ~8-day satellite sampling. The upper panel shows the spectrum of a pure semi-diurnal tidal signal (M2). The lower panel shows the spectrum after subsampling at 8 days, illustrating that aliasing redistributes tidal energy toward short periods (weeks) rather than producing coherent oscillations at multi-month or interannual timescales.*

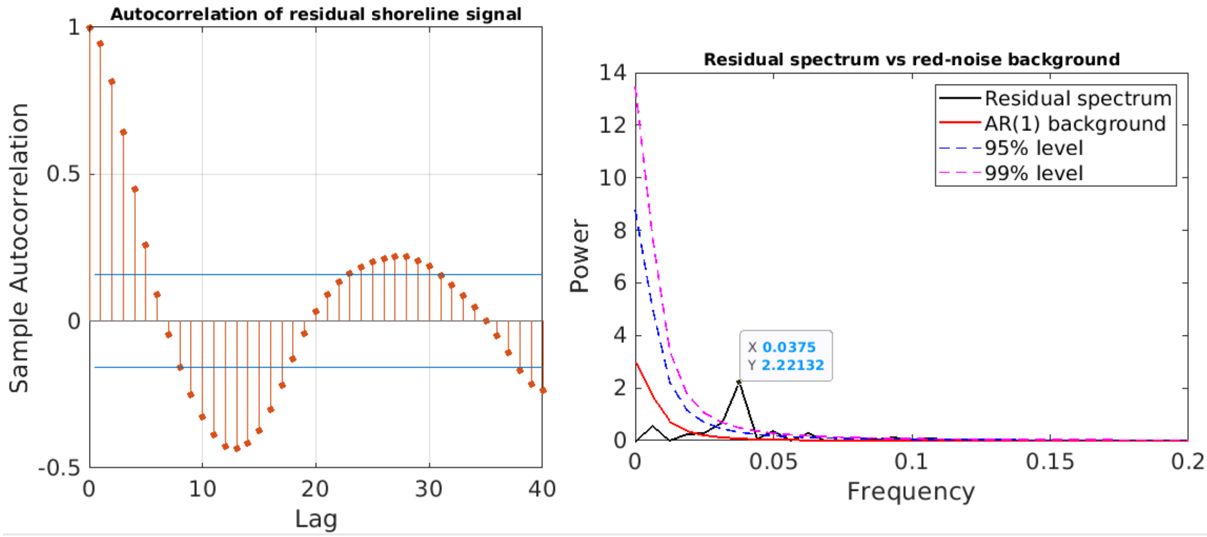

Figure 4: *Statistical structure of the residual shoreline signal at Torrey Pines after removal of seasonal variability and linear forcing components. Left: autocorrelation function showing significant temporal structure beyond the confidence limits of white noise. Right: power spectrum of the residual signal compared to an AR(1) red-noise background. A clear spectral peak near ~0.037 cycles/month (~27 months) rises above the red-noise expectation, indicating that the residual variability is not dominated by random noise despite representing a small fraction of the total shoreline variance*

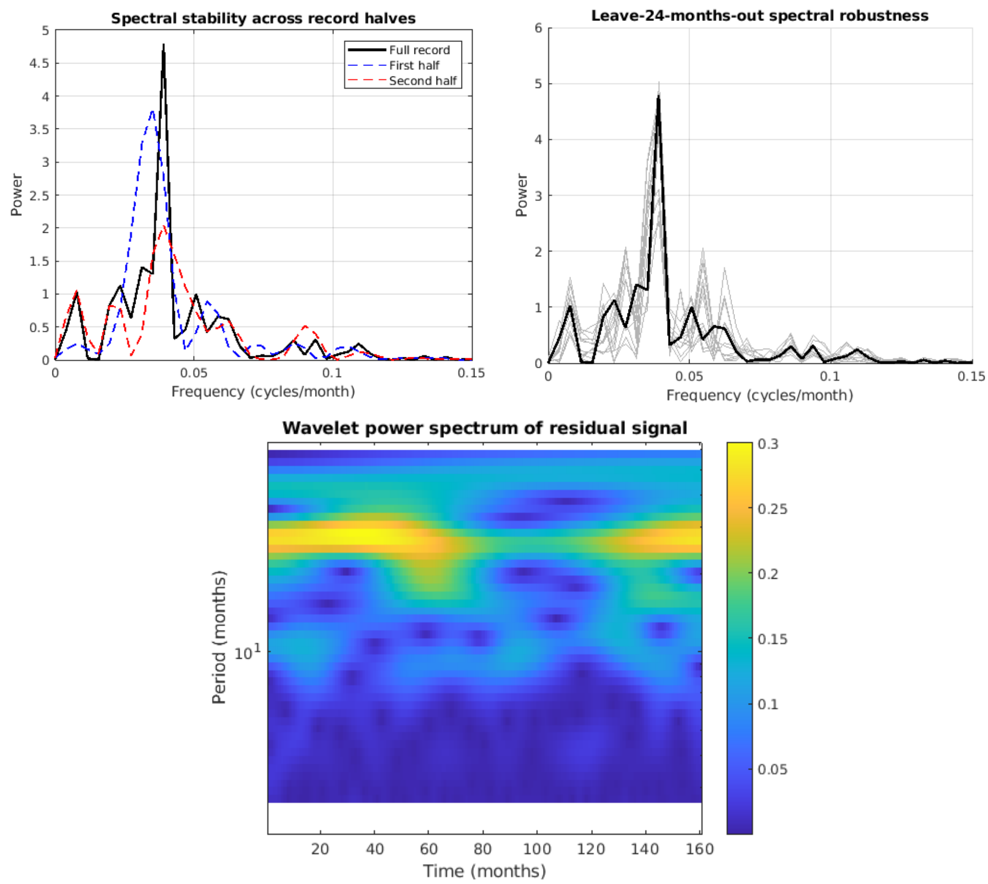

Figure 5: *Robustness of the interannual component detected in the residual shoreline signal. Top left: spectral estimate for the full record and for the two record halves, showing that the interannual peak remains present despite reduced record length. Top right: leave-24-months-out test demonstrating that the spectral peak remains stable when removing segments comparable to one oscillation period. Bottom: wavelet power spectrum showing that energy in the ~20–30 month band persists over substantial portions of the record rather than being confined to a single transient episode. Together, these diagnostics indicate that the interannual mode is not controlled by a small number of isolated cycles despite the limited record length.*

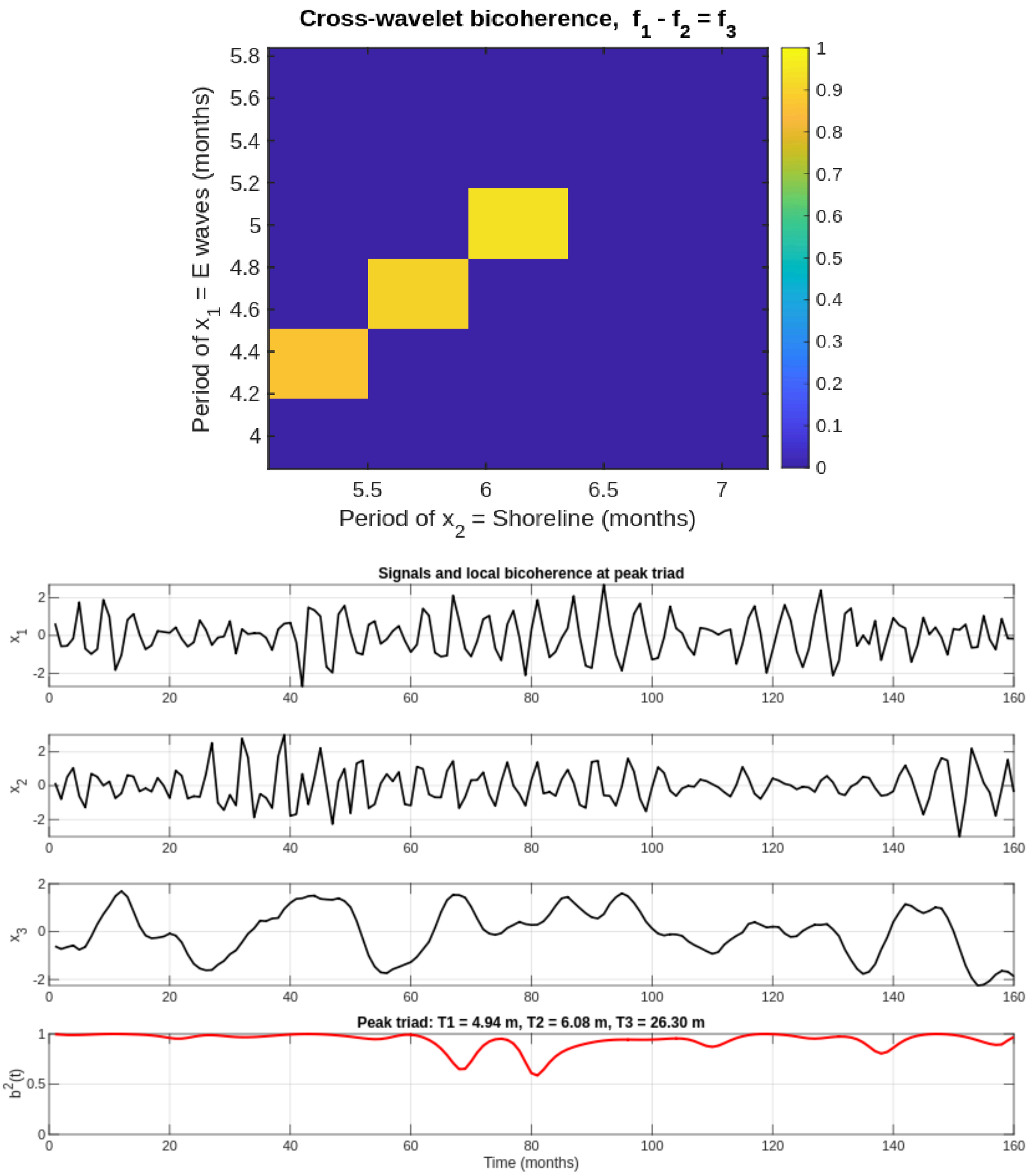

Figure 6: *Wavelet bicoherence analysis of the triadic interaction at Torrey Pines. Top: Cross-wavelet bicoherence* $b^2(f_1, f_2)$ *between the semi-annual wave energy component (*x_1*) and the semi-annual shoreline component (*x_2*), restricted to frequency combinations whose difference* $f_3 = f_1 - f_2$ *falls within the interannual band (18–36 months). The peak bicoherence* b^2*=0.83–0.93 (robust across smoothing parameters of 5–9 months) occurs at* T_1*=4.94 months and* T_2*=6.08 months, yielding a difference frequency* T_3*=26.30 months consistent with the near-biennial mode identified in the residual shoreline signal. Bottom: normalized time series of* x_1 *(semi-annual wave energy),* x_2 *(semi-annual shoreline), and* x_3 *(residual shoreline signal), together with the local bicoherence* $b^2(t)$ *at the peak triad. The local bicoherence remains consistently elevated throughout the record, confirming that the triadic interaction is not confined to an isolated episode but reflects a persistent nonlinear coupling between the semi-annual components of wave forcing and shoreline response.*

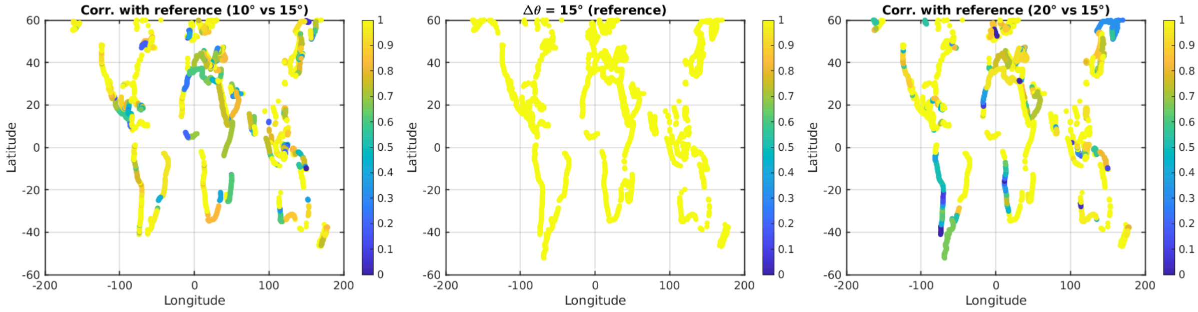

Figure 7: *Sensitivity of the shoreline aggregation procedure to the orientation threshold. Panels show the correlation coefficient r between shoreline time series aggregated using orientation thresholds of ±10° and ±20° relative to the nominal ±15° configuration. The correlation is computed for each coastal segment over the full time series. Values close to 1 indicate that the aggregated shoreline signals are nearly identical despite changes in the aggregation parameter. The median correlation across all locations ranges from 0.99 to 1.00, indicating that the aggregation step primarily acts as a spatial grouping of neighbouring coastal segments and does not significantly modify the underlying shoreline dynamics.*

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