Response to Reviewer 2

We thank Reviewer 2 for their time, but we note, first and foremost, that this review contains no scientific discussion of the manuscript's content: no engagement with the methodology, the results, or the physical interpretation. In the absence of any specific scientific criticism, we therefore limit ourselves to address the general points raised.

Hence, we wish to briefly restate the structure of our study. This work rests on three independent and converging lines of evidence: (i) observational evidence of a near-biennial spectral peak (linked to triadic resonance) identified in three independent datasets (a first local surveyed *in situ,* a second tide-corrected, regional high spatial resolved from Graffin *et al.* (2025) and a global satellite-derived shoreline dataset - GlobC) after exhaustive removal of all known linear forcing contributions. (ii) The triadic resonance computed through amplitude-weighted triadic phase coherence analysis shows persistent and statistically robust phase locking over regional aggregates of the wave-shoreline system. (iii) Regarding one of the three datasets (GlobC) which is global and coarse (generated in 2020), a noisy dataset is not an invalid one. Individual transect-level uncertainty is explicitly acknowledged in the present version of the manuscript. However, noise does not preclude the detection of coherent regional and slow-varying seasonal signals when appropriate statistical aggregation is applied (i.e., reducing individual sampling uncertainty (~ 10 – 15 m RMSE) to a minimum detectable change of ~ 0.5 - 1 m; see response to Reviewer 1). This aggregation strategy is standard in geophysical signal analysis, and the statistical significance of all results is assessed rigorously throughout the manuscript.

On journal scope: The decision to send this manuscript for review rests with the editor, who deemed it appropriate for *Nonlinear Processes in Geophysics*. A paper demonstrating nonlinear triadic resonance as a mechanism generating interannual variability in a geophysical system is precisely within the scope of this journal, which explicitly covers nonlinear interactions, phase coupling, and emergent dynamics across Earth sciences. We trust the editor's judgment on this matter.

On data processing transparency: The manuscript provides a detailed description of all data (publicly available) processing steps, including the iterative cross-EOF removal, Monte Carlo convergence tests, synthetic noise validation, and wavelet-based signal reconstruction. Suggesting that readers of NPG (a journal whose readership is well-versed in nonlinear dynamics and signal analysis) cannot evaluate these methods seems unwarranted. More fundamentally, the objective of the study is not to process data for its own sake, but to

extract dynamical relationships from observations. The identification of phase-consistent triadic interactions, their spatial robustness, and their linkage to external forcing constitute a dynamical result, not a purely data-driven one. We would also note that the ability to fairly evaluate a manuscript depends not on the journal, but on the reviewer who, in good conscience, agrees to assess it. By accepting review assignments, reviewers implicitly acknowledged their competence to do so.

On the significance of the result: This study's objective is not to reproduce the full satellite-derived shoreline variability at each location, but rather to identify and quantify how large-scale wave-shoreline resonance leaves a detectable imprint on shoreline position at specific timescales. Shoreline position is also influenced by a wide range of local processes, including direct climate forcing, complex alongshore and cross-shore hydro-sedimentary dynamics, and human interventions, none of which are explicitly represented in our framework. Against this background, identifying a mechanism that accounts for ~15% of regional interannual shoreline variance in the satellite dataset (and independently validated at 7% in long-term *in situ* measurements) represents a physically meaningful and non-trivial contribution to our understanding of shoreline dynamics. The significance of this result does not depend on one's methodological background, but on the physical and statistical evidence presented.

On the theoretical model: As stated explicitly in the manuscript, the forced-oscillator model is a conceptual toy model. Its sole purpose is to demonstrate that the near-biennial frequency emerges naturally from the quadratic interaction term $x_1 x_2$ using observed signals, without prescribing any interannual forcing. It carries no predictive intent and does not constitute the basis of our conclusions. The core results rest entirely on the observational evidence and the in situ validation at Torrey Pines, not on this toy model used only within the discussion section. Finally, while triadic interactions are indeed expected in quadratic systems, their detection in real-world, noisy, and non-stationary geophysical signals, as well as their attribution to specific physical drivers, is not trivial and requires dedicated analysis.

We therefore respectfully consider that the concerns raised do not question the scientific validity of the manuscript, but rather reflect a difference in perspective regarding its positioning.