nature portfolio

Peer Review File

Waterline responses to climate forcing along the North American West Coast

Corresponding Author: Mr Marcan Graffin

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Mr Graffin,

Your manuscript titled "Waterline responses to climate forcing along the North American West Coast" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, a revised manuscript must:

1. Fully clarify the methods used, including the wave power formula, and explain the rationale behind dividing NAWC into subregions.

2. Fully justify the choice of Eddy Kinetic Energy as a proxy for storm track direction and clarify how uncertainty is incorporated and derived.

3. Analyze wave forcing and storm impacts and better contextualize the main findings withing the broader literature.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Authors have perfectly employed waterline extraction and tide correction techniques. The methods adopted for trend and seasonal calculations, along with EP and CP indices sounds good. The claims of the authors, regarding ENSO and PDO events with waterline position are well explained.
Following points has been raised regarding the manuscript:

01. Regarding Figure 02:
a) How the unit of wave power is translating to W/m (I suppose it is Watt per meter) from the formula mentioned, i.e., product of squared significant height and wave period?
b) What is the reference of the formula of wave power that has been used?
c) From the figure it is evident that wave power roughly ranges between -2000 to 2000 W/m. How can wave power be negative?
d) Regarding Fig. 2b, c, d and e, why the latitude in y-axis is not maintaining fixed distance, like Fig. 2a?
e) Regarding the colour palette, higher wave power and sea level is represented in reddish and bluish for low values. This representation is reversed for wave direction and waterline position. It would be better if consistency is mentioned, if there is no other reason behind choosing the palette.

02. Authors have mentioned discarding 40% of transects from non-sandy beaches. Is there any specific reason for such removal?

03. In many instances, abbreviations mentioned for the first time in the manuscript are not in expanded form. For example, EP and CP are mentioned in line 162, but the expanded form is mentioned in the later part (line 185 and 187). Also, MEI mentioned in the line 267 has not been described in the manuscript (expanded form is mentioned in Supplementary Fig. 03).

04. Regarding Figure S3, What is the unit of Wave Energy? The wave energy formula mentioned here is the same as the wave power formula mentioned in Figure 02.

05. Authors have divided NAWC into five subregions. What was the basis of that classification?

Reviewer #2 (Remarks to the Author):

Please refer to attached review

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Version 1:

Decision Letter:

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Dear Mr Graffin,

Your revised manuscript titled "Waterline responses to climate forcing along the North American West Coast" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewer 1. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Lines 121–148: The lateral boundaries of the PNW, NCA, and other study areas are not clearly defined in climatological or geomorphological terms. In Figure 1, the rationale for using a rectangular boundary is unclear. If waterlines are the primary focus, offshore and nearshore limits might be more appropriately defined by features such as the dune front or depth of closure. The geomorphic processes influencing responses to ENSO are not well articulated. Were any additional studies conducted to confirm that parameters such as beach slope, substrate lithology, or riverine sediment inputs do not significantly affect waterline positions on a seasonal scale? These factors appear to be omitted from the seasonal discussion in Figure 2.
While this version addresses the points raised in my previous review, incorporating considerations beyond statistical projections—such as relevant physical processes—could enhance the study's rigor and its appeal to an international audience

Reviewer #2 (Remarks to the Author):

I would like to commend the authors for their efforts and attention to detail in their responses to reviewers’ comments. The authors have clearly spent a lot of time addressing and integrating extensive reviewer comments into their manuscript, and I hope they feel that their efforts have resulted in a significantly improved manuscript.
Their revised manuscript they have made substantial changes, including extensively restructuring the original manuscript, incorporating additional background information and reference to relevant literature, and providing additional quantitative results. Their work addresses some long standing and highly relevant questions in the field of coastal dynamics the relevance and importance of which is now clear and well supported. The authors also now highlighted some of the key novelties of their study which, in my view, were previously understated. Overall, I find this is a manuscript of very high quality that represents a significant contribution in the field. In my view, it is ready for publication, and I am looking forward to seeing it published in due course.
Kat Konstantinou.

Point-by-point response to reviewers

We appreciate the time and effort the editor and reviewers have spent reading and reviewing our manuscript. We have responded to all of the comments and questions one by one in the letter below, and addressed/clarified any concerns raised. Please note that the reviewers' comments and questions are shown in black, and our responses are shown in blue.

The in-depth revision of our manuscript led to changes and inputs of figures in our main manuscript and supplementary materials. To better address the comments and questions of the journal's and the USGS's internal reviewers, we made changes in the order of figures and tables in the main manuscript and Supplementary, along with adding new figures/tables. The table below shows the new order of these figures/tables:

Previous label	**New label**
Figure 1	Figure 1
Figure 2	Figure 2
Figure 3	Figure 3
N/A	Figure 4
Figure 4	Figure 5
N/A	Table 1
Figure S1	Figure S1
Table S1	Table S1
N/A	Figure S2
N/A	Figure S3
N/A	Figure S4
N/A	Figure S5
N/A	Figure S6
N/A	Figure S7
Figure S3	Figure S8
Figure S4	Figure S9
Figure S5	Figure S10
Figure S6	Figure S11
Figure S7	Figure S12

Table R1: Changes made to the order of figures and tables in the main manuscript and Supplementary after the 1st round of revision

The marked-up version of the manuscript shows in ~~red~~ the deleted parts and in blue the added parts. Responses to the reviewers' comments indicate line numbers where changes have been made, these line numbers refer to the revised (unmarked) manuscript.

Reviewer #1

Authors have perfectly employed waterline extraction and tide correction techniques. The methods adopted for trend and seasonal calculations, along with EP and CP indices sounds good. The claims of the authors, regarding ENSO and PDO events with waterline position are well explained. Following points has been raised regarding the manuscript:

We thank Reviewer #1 for their supportive comment and for the relevant points raised. We hope our responses and revised manuscript respond to their questions/concerns.

01. Regarding Figure 02: a) How the unit of wave power is translating to W/m (I suppose it is Watt per meter) from the formula mentioned, i.e., product of squared significant height and wave period? b) What is the reference of the formula of wave power that has been used? c) From the figure it is evident that wave power roughly ranges between -2000 to 2000 W/m. How can wave power be negative? d) Regarding Fig. 2b, c, d and e, why the latitude in y-axis is not maintaining fixed distance, like Fig. 2a? e) Regarding the colour palette, higher wave power and sea level is represented in reddish and bluish for low values. This representation is reversed for wave direction and waterline position. It would be better if consistency is mentioned, if there is no other reason behind choosing the palette.

We thank Rewiewer #1 for raising these critical points on Figure 2. There were indeed inconsistencies in the description of the wave power formula used, its unit and how we labeled it, as well as design choices that could have been enhanced.

01-a–c) The displayed values of wave power, approximately ranging from -2000 to +2000 W/m, represent demeaned values of seasonal variability around the mean. The units are correctly W/m (watts per meter); however, the formula described in the figure caption ($H_s^2 T_p$) was incorrect. The actual formula used to generate the data is $P_{wave} = \rho g^2 H_s^2 T_p/(64\pi)$. We also have corrected the figure caption and changed to colorbar label on panel b to "Demeaned wave power (W/m)".

01-d) In Figure 2b–e, the y-axis represents the latitudes of individual transects, which are sorted by their latitude. Since the transects are not uniformly distributed along the coastline, and because the study area is not a perfectly straight, west-facing coast, the y-axis spacing is uneven. We believe that, although this representation is slightly more complicated, it provides a clearer understanding of the spatial variability in wave climate. We added horizontal solid black lines to panels b-e to highlight subregion boundaries and improve the readability of the figure.

01-e) We acknowledge the inconsistency in the color palette. To avoid confusion, we have harmonized the colormap across all panels. Higher values are now consistently represented with warm colors (reddish), and lower values with cool colors (bluish), ensuring visual consistency throughout.

02. Authors have mentioned discarding 40% of transects from non-sandy beaches. Is there any specific reason for such removal?

Sandy beaches are highly dynamic systems that experience frequent changes due to erosion and accretion over short timescales (episodic, seasonal, interannual) up to geological timescales. In contrast, rocky coasts (main type of non-sandy beach along the NAWC) change more gradually over geological timescales. Since our study focuses on the influence of seasonal to interannual climate variability on coastal change, we excluded non-sandy coasts to maintain consistency in the timescales analyzed. Furthermore, the detection methods used in this study were specifically designed to monitor sandy beaches, and non-sandy coasts exhibit much lower rates of change, which would likely result in a high noise-to-signal ratio and reduced reliability of the measurements and analysis.

03. In many instances, abbreviations mentioned for the first time in the manuscript are not in expanded form. For example, EP and CP are mentioned in line 162, but the expanded form is mentioned in the later part (line 185 and 187). Also, MEI mentioned in the line 267 has not been described in the manuscript (expanded form is mentioned in Supplementary Fig. 03).

We thank Reviewer #1 for their comment. Abbreviations are now introduced in their expanded form the first time they are mentionned in the manuscript. Also, EP, CP and MEI are clearly introduced in the revised Introduction (lines 96-109).

04. Regarding Figure S3, What is the unit of Wave Energy? The wave energy formula mentioned here is the same as the wave power formula mentioned in Figure 02.

In Figure S3, the unit was [m^2/s]. It was indeed the same formula as mentioned in former Figure 2. But Figure S3 (now S8) has been modified, now displaying winter-averaged wave power at four locations along the NAWC calculated using the formula $P_{wave} = \rho g^2 H_s^2 T_p/(64\pi)$ (in W/m).

05. Authors have divided NAWC into five subregions. What was the basis of that classification?

We thank Reviewer #1 for raising this point. The reasons behind dividing the NAWC into five subregions were indeed not fully explained in the manuscript. As mentioned in the Introduction and shown in Figure 1, the NAWC exhibits contrasting coastal hydrodynamics and varying morphological responses to ENSO (e.g., Barnard et al., 2015). To represent this spatial diversity, and to help us organize our discussion of the study results, we subdivided the coastline into regions using several criteria.

First, each subregion needed to contain enough usable transects to ensure a representative sample. Second, we aimed for subregions that were geographically recognizable and relatable to readers. For instance, the Pacific Northwest corresponds to Washington, Oregon, and the northernmost part of California, while the boundaries between Southern California, Baja California, and Baja California Sur follow administrative borders. Finally, because California spans a much larger area than the other subregions, we divided it into two subregions that are distinct in terms of coastal conditions, such as coastline orientation, wave climate, and resulting shoreline variability (as recently highlighted by Warrick et al., 2025). The revised introduction presents a deepen description of the subregions, with emphasis on their wave climate and geomorphological differences (lines 131-158).

Reviewer #2

General Comments

This work addresses some long standing and highly relevant questions in the field of coastal dynamics that has only been made possible in recent years thanks to the rapid increase in satellite-based shoreline extraction capabilities. The authors apply a recently developed method for extracting waterlines from optical satellite imagery to investigate the relationship between large scale climatic forcing and coastal response along the western coast of the US. They derive monthly timeseries of waterline positions along 7000+ 250m-spaced transects and analyse the monthly and seasonal signals alongside monthly wave power, wave direction, and sea level data. Their findings indicate spatially variable coastal response across the region that mirrors difference in incident wave power and wave direction as well as sea level. The authors hypothesise that climate modes are most likely the key drivers of a large portion of the observed variability in waterline position and perform statistical analysis of the waterline position anomalies and different expressions of ENSO. This analysis leads to only weak and mostly statistically not significant relationships.

We would like to thank Reviewer #2 for this very thorough review. We have taken due note of all the comments made by the reviewer, which have led to substantial changes to the manuscript.

RV1. Structure: The introduction does not provide the necessary background information although most of this is present in the results and discussion sections. The are also several sections in the results section that would be more appropriately included in the discussion section. Therefore, I would suggest restructuring the manuscript so that the introduction includes all the necessary background information, the results section includes the presentation of results, and the discussion section only includes a critical analysis/interpretation of the findings. Several suggestions have been made in the specific comments that follow.

This excellent point has led to an in-depth restructuring of the manuscript. Several aspect in the introduction, especially general knowledge on shoreline change, links between storminess and wave generation, and climate modes influence on regional wave climate, have been rigorously introduced to provide readers with all the relevant background to grasp the problem we are adressing and the implications of the presented study. We also removed opinionated discussion items from the Results section, and deepened our Discussion in line with the many comments raised by the Reviewer.

RV2. Methods: the methods are clear. However, it is not always clear which approaches the authors have adopted from previous works and which they are contributing to this work. Also, they do not provide much explanation as to why the authors use the Eddy kinetic wind energy as a proxy for storm track direction instead of the parameters of interest, i.e., storm wave intensity and direction. It would also be useful to provide some link between storm wave parameters of interest and the EKE.

We thank Reviewer #2 for this thoughtful comment. We have clarified our Methods section to detail which methodological components are derived from previous studies and which are novel contributions of our work. Most of these comments were related to deviations from the original approach of Bergsma et al. (2024) which we roughly reproduced. However, some aspects of this methodology were modified to allow for transferability to our study site and to handle the scale of our region of interest. We clarified that the index and thresholding method used in this study are from Bergsma et al., (2024) but that other additional processes are original inputs from this work or adapted from previous works (see lines 486-500).

EKE is widely used as a proxy for position and intensity of the storm track as it quantifies the intensity of transient atmospheric eddies, which are closely linked to mid-latitude cyclones and storm tracks and ultimately influence wave climate. Several studies support this approach in the literature:

- Chang et al. (2002) reviewed storm track measures and highlighted EKE's effectiveness in capturing storm track variability.
- Hoskins & Valdes (1990) explained how baroclinic eddies, which contribute to EKE, are fundamental to storm track formation and maintenance.
- Trenberth (1991) used EKE diagnostics to analyze storm track variability and its connection to large-scale atmospheric circulation changes.

We acknowledge that storm wave intensity and direction are also relevant parameters. However, large-scale reanalysis products like ERA5 do not provide a direct metric for "storm wave intensity." At the monthly scale, storms are smoothed in the significant wave height (Hs) and peak period (Tp) signal. It could eventually be derived from hourly wave data but this approach would be highly computationally demanding and require significant methodological adjustments. That said, we agree that this alternative approach could provide additional insight. We now state this limitation and included it as a perspective for future work in the Discussion section (lines 384-385).

RV3. Uncertainty: The authors consider various sources of error and uncertainty yet in some cases, it is not clear how uncertainty is incorporated in the results (e.g., RV43). In other cases, it is not clear how some uncertainty limits have been derived (e.g., RV20). Some further clarification would be beneficial.

We thank Reviewer #2 for highlighting these flaws in the uncertainty assessment. Since this general comment points towards specific concerns raised in RV20 and RV43, we have addressed it in detail through our responses to those specific comments.

RV4. Results: There is a general absence of quantified results which means that most conclusions need to be draw from figures. This also applies to relationships between parameters (e.g., how SLA is related to waterline anomaly) which can only be qualitatively (and subjectively) draw from figures. A major issue for me is that although the authors stress the key role of wave forcing and storm impacts as a primary driver of coastal response throughout the manuscript, they do not perform any relevant analysis. I strongly feel this is missing (please also refer to comments RV16 to RV35).

We thank Reviewer #2 for highlighting this concern regarding the need for more quantitative analysis. In response, we have revised the manuscript to incorporate additional panels in existing figures, emphasizing statistical relationships between seasonal cycles of key drivers (wave power and direction, and monthly-mean sea level) and waterline positions, as well as between climate modes and waterline position anomalies. Furthermore, we have revised Figure 2, 3 and 4 (now Figure 5), and we have added several new figures (Figure 4, S2-7) to strengthen these points. That said, we would like to emphasize that qualitative analyses —such as those presented in Figure 5— also provide valuable scientific insights, particularly when examining complex atmosphere-ocean-coast interactions that are not easily captured through statistical approaches. Nonetheless, we have carefully addressed all specific comments from RV16 to RV35 and have made substantial changes in the Results section to ensure that our findings are more robustly quantified and clearly supported throughout the manuscript.

RV5. Discussion: I do not feel the authors discuss their findings and main claims appropriately in the context of the wider literature nor do they discuss the implications of their findings. Importantly, I do not feel the analysis presented supports their main conclusions. The absence of adequate quantification of the results they discuss in combination with the fact that they have not addressed wave forcing despite stressing its importance makes the discussion weak and often, unsupported (please also refer to comments RV36 to RV41).

We thank Reviewer #2 for raising this important concern regarding the discussion and contextualization of our findings. We acknowledge that the previous version of the manuscript lacked adequate discussion in the context of the broader literature, and we also agree that the implications of our findings were not sufficiently highlighted. In response, we have significantly revised the Discussion section to address these concerns. The revised discussion now includes a more thorough analysis of our results within the context of existing literature, highlighting both the strengths and limitations of our study in comparison to previous works. We have reorganized the structure of the Discussion to follow a more logical flow, with clearer connections between our results, existing literature, and perspectives for future work. We fully addressed specific comments RV36 to RV41 and the necessary revisions have been made to strengthen the manuscript's overall argument.

RV6. Conclusions: I feel the findings presented do not provide strong evidence for the main conclusions. Please refer to comments RV42 and RV44 for specific details.

We thank Reviewer #2 for raising this concern. We understand that the lack of strong evidence in the previous version of the manuscript may have affected the robustness of the conclusions. In response, we have significantly revised the Results and Discussion sections to provide more concrete evidence supporting our conclusions. By including additional analyses (e.g., signal-to-noise analysis for ENSO impact on waterline change, and correlations to quantify the strength of linear links between drivers and waterline position), clarifying key relationships, and enhancing the contextualization of our findings in the literature, we believe the revised manuscript now offers a more compelling argument. We also totally rephrased the Conclusion section to ensure it accurately reflects the revised content, and specifically in relation to the points raised in comments RV42 and RV44. These revisions strengthen the connection between our results and the broader conclusions drawn from this study.

Specific Comments

Introduction

RV7. L36-51: I understand where the authors are going with this paragraph. However, I feel there is some confusion in the terminology with respect to coastal change indicators (see next comment below). There is some well-established literature on the topic that is not considered here, so I would suggest revising the use of the term 'indicators' within the framework of established literature.

We thank Reviewer #2 for this comment. We respond to this comment bellow comment RV8.

RV8. L38-40: You are referring to coastal change indicators and although waterline is one such indicator according to (Boak & Turner, 2005), dune erosion and beach slope are beach characteristics rather than coastal change indicators. The dune toe, vegetation line, any elevation-based beach contour for example can be coastal indicators, and therefore, changes in these can be considered as coastal change indicators. I would suggest rephrasing these two sentences to consistently refer to either coastal change indicators or beach parameters commonly monitored.

We thank Reviewer #2 for this comment. We understand the distinction made between coastal change indicators and beach characteristics. While we argue that beach slope, which typically flattens during erosional events and steepens during recovery, can serve as an indicator of coastal change, we acknowledge that it is more accurately described as a beach characteristic. We have revised the corresponding lines, now mentioning only coastal indicators (lines 48-50).

RV9. L49: By 'one-line' are you referring to 2D, linear indicators as opposed to area (e.g., subaerial beach) or volume metrics?

Reviewer #2 is right, we wanted to refer to linear indicators (i.e., 2D) as opposed to area/volume metrics. The sentence is now phrased as "generally through linear indicators (i.e. 2D)" (line 59) in the revised manuscript.

RV10. L50-51: I am not sure I understand this. SDS is based on the delineation of the land/water interface. That really is the definition of the (instantaneous) waterline.

The land/sea interface can be ambiguous due to the dynamic nature of coastal sea level and hydrodynamic processes. In our manuscript, we distinguish between the shoreline, which refers to a morphological state and is typically defined as the intersection between coastal topography and a fixed elevation (such as a tidal datum), and the waterline, which is influenced by both hydrodynamic and morphodynamic factors. We have rephrased this sentence for improved clarity (lines 59-62).

RV11. L60-64: please provide some relevant bibliography.

This part of the introduction have been substantially revised to reflect on the large body of literature studying shoreline variability (lines 70-86). It no longer opposes long-term and shorter terms shoreline variability, but rather expose the various scales and mechanisms of change that can exhibit shoreline position.

RV12. L71-L77: I feel the reader would benefit from a bit more explanation and detail as to i) what these effects are on the wave climate, and ii) Why and how are coasts and coastal ecosystems affected. This can then provide the motivation for this study. At the moment I feel this paragraph is too general and vague to introduce the reader to the topic and the value of this investigation. As a result, the motivation is not clear.

We thank Reviewer #2 for this suggestion. We have expanded this section to to clarify how ENSO variability impacts wave climate and, in turn, coastal regions and ecosystems. We now introduce the distinct effects of EP and CP El Niño events on storm tracks and swell generation (Capotondi et al. 2015; Yeh et al. 2018; Taschetto et al. 2020; McKenna and Karemperidou 2024). We also clarify how these wave climate variations influence coastal dynamics: stronger and more frequent winter swells during EP El Niños result in enhanced coastal erosion (Allan and Komar 2006; Barnard et al. 2017; Boucharel et al. 2021), while CP El Niños tend to reduce storm activity in the eastern Pacific and are associated with less marked coastal reponse. These distinctions now emphasize more clearly the need for a comprehensive investigation of how ENSO diversity shapes coastal change patterns along the North American West Coast (lines 87-114).

RV13. L79: Why is this relevant? Almar 2023 may have investigated the influence of ENSO as the authors are, but other studies (apart from Almar 2023) exist that have performed regional analyses at higher spatial scales (e.g., Vos et al., 2023; Castelle et al., 2024; Warrick et al., 2025). Please provide a few words to also recognise that other large-scale studies have been performed at finer spatial scales and justify your choice of scale.

This is a good point. We have completed it with references to other large-scale studies, such as Vos et al. (2023), Castelle et al. (2024), and Warrick et al. (2025), which perform analyses at finer spatial scales. (see revised text in lines 115–120).

RV14. Figure 1: Could you please indicate on the map where Long Beach, Monterey, Torrey Pines, and Playa el Suspiro are. Also, I feel this figure should appear later on in the results sections.

We thank Reviewer #2 for this comment. We have updated Figure 1 to indicate the locations of Long Beach (also relevant for Figure S3), Monterey, Torrey Pines, and Playa el Suspiro on the map (Figure 1, panel a). As Figure 1 shows the study area and presents the spatial diversity of significant wave height and sea-level variability along the NAWC, we believe its place is in the Introduction, even if it displays the pie charts of waterline position long-term trends which are indeed a result (but not further analyzed nor described in this study).

RV15. L94-L98: Could you please provide some indicative figures (e.g., winter/summer Hs, Tp, etc)?

Certainly, we detailed the description of regional coastal hydrodynamics (wave and sea-level) mean state and seasonal variability in the description of the subregions (lines 131-158).

Results

RV16. General: You provide acronyms for the four main regions but do not define them specifically. In the text, you sometimes use the acronym and sometimes a description (e.g., southern California) but it is not always clear which are you are referring to. It would be beneficial to link the two (perhaps in the Figure 1 legend?) to increase clarity.

We agree that the five subregions described in this study were not properly introduced. We added one paragraph of description for each in the Introduction (lines 131-158).

RV17. General: You mention that changes in storm tracks and storminess are a critical factor behind the coastal responses, but you provide not quantification of these and how they vary across the NAWC. Yet you do prove some very nice and detailed figures illustrating the impact of ENSO on SLA (-8.0 cm – 8.0 cm) and discuss the waterline response to that. I am perplexed as to this disparity. If your findings indicate the coastal response to 16 cm SLA is more pronounced than its response to increased storminess and changes in storm tracks, please provide some supporting evidence.

We thank Reviewer #2 for raising this concern. Designing Figure 4 was challenging, as we aimed to present multiple datasets on the same figure without making it too dense. We have now added contours of EKE 200 hPa for fixed values (100 to 250 m^2/s^2, with 25 m^2/s^2 intervals) to provide a clearer representation of storm track behavior across different ENSO phases. This clearly shows clearly, both qualitatively and quantitatively, how storm tracks intensify and shift southwards during El Niño phases, associated by a wave power increase and rising sea-level. As for the SLA impact, we moved this figure to the Supplementary and replaced it by a similar figure showing anomalies of wave power instead.

RV18. L101: Do you mean you average 125m either side of each transect?

No, in this sentence we referred to the 250m-spacing of the transects. We rephrased it in this way to remove any ambiguity (lines 166-167).

RV19. L103: Good choice! It is great thinking that we have data since 1984 but in reality, the data we have prior to 2000 is so sparse it severely limits our analysis capability.

We appreciate that Reviewer #2 notes this point.

RV20. L115: How was the 0.5m/year threshold calculated?

We are not sure we understand the question. If the question is about why the choice of 0.5 m/year as a threshold to distinguish stable and non-stable beaches, we followed the methodology outlined by Luijendijk et al. (2018), who used the same threshold to define stability of a sandy beach, based on a reflection of the detection error and the study period.

RV21. L126: It is not clear to me at least how the comparison between seasonal and overall signal was performed.

We thank Reviewer #2 for raising this point. To clarify, at each transect, we calculated the correlation and p-value between the seasonal cycle (which was repeated to match the length of the raw signal) and the raw (monthly-sampled) signal from which the seasonal cycle was derived. This allowed us to assess the statistical significance of the seasonal component in relation to the overall variability of the raw signal. This has been slightly rephrased in the revised manuscript (lines 196-198).

RV22. Figure 2: This is a great plot that demonstrates the variability in the wave climate along the NAWC verry nicely. However, the latitude scales in (a) and (b-e) do not match so it is difficult to make direct comparisons. Please align the two y-axis. Also, please clarify 'Cycle range' in (a) refers to waterline excursion.

We thank Reviewer #2 for the positive feedback on Figure 2. About the alignment of the latitude scales in panels (a) and (b–e), we acknowledge the concern. As noted in our response to Reviewer #1, the y-axis in panels (b–e) represents the latitudes of individual transects, which are sorted by latitude. Since the transects are not uniformly distributed along the coastline, and because the study area is not a perfectly straight, west-facing coast, the y-axis spacing is uneven. Aligning the y-axes to match panel (a) would require to average data over latitude ranges or distorting the relative size of the transects, which could undermine the spatial representation. We believe the current layout better reflects the spatial variability of the displayed metrics, even if it is slightly less legible. We added horizontal black lines between subregions on panels b-e to help readers situating the presented seaonal cycles. We changed 'Cycle range' into 'Seasonal waterline excursion".

RV23. L145-150: I can see in Figure 2a that there is a significant drop in wave power south of 34°N accompanied by an equally significant difference in wave direction (Figure 2c) and a noticeable shift in the timing and magnitude of negative SLA (Figure 2d). But the switch in the timing of the erosion/accretion cycle seems to happen much further south, below 28°N and does not look like it could be associated with the timing of the SL seasonal cycle that switches much further north, at least as shown in Figure 2. Are you referring to some previous work that has linked the two? Also, it seems to me that below 25°N the shoreline orientation is more southward (Fig2a) and the prevailing wave direction (Fig2c) so wave incidence is more oblique. There also seems to be a definite shift in wave power seasonality with lower wave power during the winter months and higher during the summer months. So very plausibly, the shift in the waterline circle may reflect the aforementioned conditions rather than a 7-25cm SLA.

This is a fair point that deserves some explanations from our part. Southern SCA and BCS coastlines share similar characteristics : they are roughly southwest oriented, have a weak and no spatially-coherent wave power seasonal cycle, similar wave direction and sea-level seasonal cycles. However they exhibit very different waterline position seasonal cycles, southern SCA is very impacted by its local geomorphic characteristics (*Warrick et al., 2025*) and therefore show no regional consistency in its waterline position, making it hard to associate its regional variability with any drivers. However BCS waterline positions are spatially very consistent at the seasonal scale, and this seasonal pattern is more correlated to fluctuations of the sea-level (see new panel f on Figure 2) than to wave power/direction variability. Variations of $\sim$ 20 cm in the sea-level induce passive waterline change of a few meters (2.5-3 m considering the median estimated slope along the NAWC $tan\beta \sim 0.07$, see Figure S12), which is already relatively large regarding the 9 m median seasonal excursion in BCS, and to which can also be expected non-passive changes, adding further weight to the influence of sea-level variations on the waterline position. However, this discussion has no place in the Results section and has been moved to the Discussion (lines 369-386)

RV24. L162-163 & Fig3: I will return to my earlier comment (RV12) that not enough information on the behaviour and influence of ENSO on the wave climate has been provided and as a result. Please include the relevant background info in the introduction.

We thank Reviewer #2 for this comment. This has been added to the Introduction section (lines 87-114).

RV25. L163: The PDO is not introduced until L176. Please introduce in the first instance. Also, please introduce it in the introduction and discuss interactions with ENSO.

We no longer refer to PDO in the Results section, nor in the Introduction. We now only reflect on the potential influence of PDO and waterline variability along the NAWC in the Discussion section, with proper description of the phenomenon (lines 427-435).

RV26. L178-180: I am not sure I understand why you characterise your 'time coverage' as limited. Are you referring to the 1997-2021 period? If yes, I am guessing existing knowledge indicates the PDO has larger cycles? Again, this is not at all clear primarily because the background information is missing in the introduction.

Similarly to the previous response, we shifted this reflection on PDO impacts to the Discussion. We were indeed mentioning that our time coverage (1997-2022) was relatively short compared to PDO return period, making analysis complicated. The 20-30 years return period of the PDO is now clearly described (lines 427-435).

RV27. L174-182: Perhaps I am missing something, but I feel this is more of a discussion than the presentation of your results. Because of the lack of adequate information on the behaviour of ENSO and its influence on the regional wave climates, and (complete absence of info on) the PDO, these statements are not supported here. I am also not sure I can see the notably less pronounced influence of ENSO on PNW and NCA. For example, despite the admittedly much greater variability in the waterline anomaly in PNW, both PNW and NCA plots show a drop in the 2010 event followed by a steady increase up to 2016 and another large drop in 2016. This behaviour is very closely mirrored by CP in panel (g), at least visually. Some quantitative results are necessary.

We thank Reviewer #2 for this insightful comment. We agree that the initial presentation of this analysis lacked clarity, especially regarding the lack of introduction of climate modes and their influence on regional wave climates. We intended to highlight that waterline changes in the PNW and NCA appeared to follow long-term patterns, distinct from the more event-driven erosional responses observed in SCA, BC, and BCS during El Niño events. You are correct that the impact of El Niño events, particularly in 2010 and 2016, is evident in the waterline anomaly plots for the PNW and NCA, but these event-driven changes seem to be superimposed on longer-term variability patterns. This is likely why the waterline anomalies in these regions did not show a substantial dip below zero despite the occurrence of these El Niño events. To reflect on ENSO-driven changes we modified our methodology (Figure 4 and 5) to now consider changes of waterline position anomalies between summer and winter, allowing us to quantify ENSO related changes despite the longer-terms waterline position anomaly variability observed along the PNW and NCA. To address the concern regarding the absence of quantitative results, we have added to Figure 3 a heatmap showing the correlations between climate modes and regional waterline anomalies. This provides a clearer, more quantitative understanding of the relationships between the climate modes and the observed regional variability. We have revised the corresponding paragraph in the manuscript (lines 265-269) to more accurately describe our results, and further discuss the implications in the Discussion section (lines 395-400).

RV28. L188 The PMM has also not been introduced or discussed thus far. Please provide the necessary background information on the relevant climatic patterns in your introduction.

The PMM is now only mentioned in the Discussion as an existing mode of climate variability in the Pacific (lines 427-429)

RV29. L183-190 Whether a correlation is positive or negative has no effect on its strength or statistical significance. Here you characterise a correlation as 'weak yet positive'. I imagine 'anti-correlations' refer to negative correlations? Also, you are characterising similar values differently, sometimes referring to correlations in the order of 0.4 as weak (e.g., L196) and sometimes not. Lastly, please clarify if you are reporting R (Pearson or Spearman?) values and what level of statistical significance you are considering prior to reporting your findings.

We thank Reviewer #2 for raising this point. We have removed 'yet positive' from the sentence, turned 'anti-correlations' to 'negative correlations' and standardized our terminology: correlations are now consistently referred to as strong ($0.7 < |R| < 1$), moderate ($0.3 < |R| < 0.7$) or weak ($|R| < 0.3$). We also clarified that the reported correlations are Pearson correlation coefficients (lines 228-229) and significance levels are now described when correlation are mentioned. In figures, correlation with a 1% and 5% significance levels are now marked with "**" or "*", respectively. Figure S2 from the previous draft of the Supplementary has been removed. Panel g in Figure 3 shows the Pearson correlation between climate modes and regionally-averaged waterline anomalies between 1997-2022.

RV30. L196-2001: How do the two phases of ENSO relate to or are expressed in CP and EP? This should also be mentioned in the introduction.

It's a good point, and further supports the need to clarify the description of climate modes in the introduction to the paper. This question was partially answered by the definition of El Niño/La Niña phases in the Methods, while we acknowledge it was not the best place for it. The revised Introduction now introduced clearly the climate modes and indices used to quantify their intensity. We added a long description of ENSO diversity within the

EP/CP continuum (lines 101-109)

RV31. L202-220: (please also refer to comment RV17). You mention that changes in storm tracks and storminess are a critical factor behind the coastal responses, but you provide not quantification of these and how they vary across the NAWC. The discussion you have included may be correct, but you provide the reader with no evidence, other than the waterline response shown in Fig4, of the forcing conditions during these events and how they change. You use the EKE as a proxy for quantifying changes in storminess and show the contours on Fig4, but you provide no quantification of how these relate to '...significant changes in coastal wave extremes.' Please provide some information and a figure to demonstrate the spatial distribution as they should be at least if not more important than the SLA shown in Fig4.

We thank Reviewer #2 for pointing this out. As explained in the response to RV17, we revised Figure 4 (now Figure 5) to include contours of EKE with fixed values for each ENSO phase. This now clearly shows that storm tracks intensify and shift southward during El Niño, leading to increased storm activity, both offshore and along coastal areas, from Southern California to Southern Baja California, associated with increased wave power. We rewrote the description of Figure 5 (former Figure 4) accordingly (334-354)

RV32. L204: typo: remove full stop after the word 'phase'.

Good catch. Thank you for raising it.

RV33. L202-228: A large part of this section discusses the influence of ENSO on the wave climate. This should be part of your introduction, not your results. This is what will set the scene for your waterline analysis.

We thank Reviewer #2 for this comment. Most elements of this section have been incorporated to the Introduction (lines 87-114), and we rewrote this part of the Results section to add some quantitative figures (lines 312-320).

RV34. L216: you have already introduced EP and CP; there is no need to repeat here.

Right. We thank Reviewer #2 for raising this point. The repetition has been removed.

RV35. L220-241: this is more of a discussion section.

We thank Reviewer #2 for pointing out the ambiguity in the structure of the paper. This paragraph indeed combined factual observations with subjective interpretations and discussion points. We revised the paragraph to focus on the factual results only and moved the interpretive elements to the discussion section, where they fit more appropriately (lines 407–426).

Discussion

RV36. General: I am missing a discussion on how your findings compare with the findings of previous works. I am not an expert in your area, however, I am aware of at least on publication you also cite (Vos 2023). How do your results compare with theirs? Why do you think there are differences if these exists? Are your findings in line with current understanding of how this stretch of coastline has evolved? If not, what new have we learnt?

The revised Discussion now discuss our results in the context of existing litterature. The 3 first paragraphs (lines 359-406) mention common observations made with other studies, both locally and regionally. Globally our observations align with existing literature but with analysis performed at an intermediate scale, between local studies and continental ones, refining our understanding of the spatial diversity of seasons- and ENSO-related waterline change.

RV37. General: I am also missing a discussion on the implications of your findings. Are there any observed trends in ENSO? If yes, how do you think the NAWC might be affected in the future?

We thank Reviewer #2 for raising this point. We added a paragraph on the implication of understanding climate-driven to improve our prediction skills in terms of coastal vulnerability, including a mention to the potential influence of climate change on climate modes, further complication our understanding of climate-related morphodynamics (lines 427-445).

RV38. L265-270: Here you partially address my comments RV17 and RV31. Yet throughout your manuscript you refer to the influence of ENSO on storminess. I have a few comments relating to this: I. Several studies have demonstrated storm links between ENSO and wave conditions as you also indicate (e.g., L265-266). Do you have an explanation why in your case you did not find any? II. Could you please indicate the location of ERA5 node you used for the comparison shown in Figure S3? III. The absence of statistically significant correlations between wave power and ENSO (in one location) does not imply the absence of a relationship between wave power and waterline response.

We thank Reviewer #2 for these 3 points. We think there is a misunderstanding here because our point was not robustly stated. While we agree that there are links between ENSO and wave conditions, these links are not systematically linear. We revised Figure S3 to show the winter response of wave power to ENSO for the 4 locations shown in Figure 1 (same ERA5 node, for convenience). Among the four sites, only Long Beach (PNW) shows a non-significant correlation between MEI and winter-averaged wave power. However, we make two observations on the correlations:

1. The response to MEI variations is not significant (p-value < 0.05) for the four sites if the two main ENSO events (MEI > 1.5: 1997/1998 and 2015/2016) are removed. This suggests that wave power is mostly sensitive to strong El Niños, at least for the three southern sites (there is seemingly a different behaviour along the PNW).
2. The strong La Niña event of 1998/1999 is associated with the highest wave power between 1997-2022 at Long Beach (documented by Komar et al. (2001) for the PNW), which runs counter to the simple narrative of a linear link between ENSO and wave power.

Also, we did not mean to imply a lack of relationship between wave power and waterline response. The paragraph have been rephrased to incorporate the aforementioned discussion, and to remove potential ambiguity on links between wave and waterline variability (lines 417-426).

RV39. I am not surprised you find weak and mostly not statistically significant relationships between ENSO and coastal response as your finding also agrees with previous work. However, with this you have, at least partially, disproved your hypothesis as stated in L155-158. You stress the key role of wave forcing and storm impacts as a primary driver of coastal response throughout your manuscript, yet you do not perform any relevant analysis. I strongly feel this is missing.

We acknowledge that the weak linear relationships we found between ENSO and coastal response are consistent with previous studies. However, we believe this does not disprove our hypothesis, but rather highlights a lack of linear relationship. In the revised manuscript, we now show direct correlations between climate modes (including ENSO) and regionally averaged waterline responses (Figure 3). While the correlations are statistically significant, they are generally weak ($|R| < 0.48$), which is, again, expected given the complexity of the relationships between these climate modes and coastal responses. We added Figure 4 in the main manuscript to show that El Niño (and to a smaller extent, La Niña) lead to changes of waterline position anomalies that are visible despite the noisiness of waterline position data (due to spatial heterogeneity of and SDW limited accuracy). Additionally, we have updated Figure 5 to clearly illustrate the increase in storm track activity and its southward shift during El Niño events. Overall, we maintain that these results are valuable for understanding the broader dynamics at play. The revised manuscript includes these clarifications to provide a more nuanced interpretation of our findings.

RV40. L267: This is the first mention of MEI. Please include and explain earlier in the manuscript.

We thank Reviewer #2 for pointing this out. MEI is now introduced earlier in the Introduction alongside the other climate modes considered in this study (lines 96–99).

RV41. L293-297: you mention the importance of the combined impact of elevated water level and storm impact, yet miss the opportunity to demonstrate this at monthly, seasonal, or annual timeframes. Though it is possible you find equally weak relationships due to the inherent noisiness of SDS and (potentially) the quality of the wave data, I feel that without this analysis.

This is a good point that supports the general remark on the lack of statistics quantifying links between drivers and shoreline change in this paper. As mentioned in the response to previous comments, statistical links between climate mode and shoreline change raise limitations that we partially confront and/or discuss in the revised version of the manuscript (see responses to comments R31 and R39). For the seasonal aspect, we added to Figure 2 the statistical links between coastal hydrodynamics and shoreline change at the seasonal scale, we also added the description of these results (lines 228-236). Regarding the storm impact, we found it rather complicating to translate it into quantitative analysis beyond the display of ENSO-induced storm track variability in Figure 5 as storm influence can be local and distant, especially when considering waves. In this revised manuscript our analysis relies more heavily on wave power as a direct driver of coastal change rather than storms.

Conclusions

RV42. L318-321: I am perplexed at this statement as your findings indicate weak and mostly not statistically significant relationships between climatic patterns and coastal response and you have not shown any quantitative results relating wave energy to coastal response. Including a plot of wave power in Fig2b, in my view is not sufficient to support this conclusion.

We thank Reviewer #2 for this comment, in line with comments above. We totally rewrote our conclusion, which is now more precise and directly points towards key results supported by correlations, a SNR anaylsis and composites of the different ENSO phases.

RV43. L456-474: Given the accuracy of the method as reported by Bergma 2024 (10m), would these results not indicate that most of the observed variability is within the uncertainty f the method, i.e., noise?

This is a relevant point that deserves more explanation. A priori, SDW/SDS noise is not time-dependent. Therefore, by aggregating values using monthly resampling and estimating the seasonal cycle based on over 20 years of monthly-sampled data, the magnitude of SDS noise is minimized in the estimates of the seasonal cycle (similar reasoning behind the analysis of Warrick et al. (2025)). However, for demonstration purposes, we have added to Figure S9 the validation of the seasonal cycle for 4 profiles at Torrey Pines, CA, and at one profile at Ensenada, Mexico. The low error (standard error σ = 3.2-6.9 m, below the $\sim$ 10 m SDS noise) between the estimated and observed seasonal cycle further supports our approach (lines 543-549).

RV44. L332-335: you have shown the 200hPa isobars in Fig4 and explain you are using EKE as a proxy for changes in storm track but do not perform any analysis to support this statement. As a matter of fact, the southern tip of the Baja peninsula shows the largest negative (erosion) anomaly in Fig4a, d, and e and this does not seem to relate to changes in storm track.

We thank Reviewer #2 for pointing this out. This is now properly described and justified in the Introduction section, drawing on relevant literature (lines 94-106). Regarding Figure 5 (former Figure 4), we acknowledge that the use of the 25th percentile contour for storm tracks can be misleading. The example of the southernmost tip of Baja California, which falls outside this contour yet experiences significant erosion, illustrates this issue and raise doubts about our hypothesis that storm tracks, by influencing regional wave climate, can be a proxy of waterline change. Initially, the 25th percentile contour was chosen because it provided a simple and clear way to represent the shift in storm track position without overcrowding the figure. In the revised version of the figure, we have replaced the percentile-based contour with several specific EKE value contours, which more accurately highlight the southward shift of storm tracks during El Niño events. This addition includes quantitative values to better explain why regions like the southern tip of Baja California are exposed to storm tracks despite not being within the 25th percentile contour. Also offshore storms lead to the generation of waves offshore, which can expose a given coastal area to an enhanced wave climate without directly experiencing the storm.

Methods

RV45. L337: typo: 'methods'

Good catch ! We corrected this.

RV46. L338: typo: replace 'it consists in a…' with: 'it consists of a…' however, the use of this expression is not a good use of the English language in this case.

We rephrased this sentence to : "it involves combining different spectral bands to produce SCoWI images" (lines 483-485).

RV47. L341: Who validate the application to the Landsat series? If I am not mistaken, Bergsma 2024 mention that validation of the application of their method to the Landsat series imagery would constitute further work.

This is an important point, we thank Reviewer #2 for pointing it out. As the transferability of the method to Landsat imagery has not yet been validated in the literature, we have revised Figure S9 to include separate validation metrics of the satellite-derived data at Torrey Pines (validation data spanning between 2001-2017, so only slightly overlapping with the Sentinel-2 mission) and at Ensenada, Mexico. These are shown as separated standard errors: σ_{S2} for Sentinel-2 and $\sigma_{Landsat}$ for Landsat, showing strong agreement between the performances of satellite-derived waterline estimations from Sentinel-2 and Landsat missions. We now describe this transferability check in the Methods section of the revised manuscript (lines 542-548).

RV48. L345: I believe Bergsma 2024 used the cirrus band and cloud mask in S2 to filter out cloud-contaminated images. If the SCoWI method is part of your work (or of others) please clarify and/or reference accordingly)

We thank Reviewer #2 for this comment. Bergsma et al. (2024) used the S2 cirrus band to mask the detected clouds within images, not to discard entire images from the waterline detection process. Our approach differs slightly, as we apply a filtering method to both Sentinel-2 and Landsat images to remove cloud-contaminated images entirely. This filter is part of our work and has now been mentionned in the Methods section (lines 492-493) and further described in the Supplementary Materials (Figure S11).

RV49. L347: Highlighting the poor use of the expression: 'thanks to…' in this case.

We thank Reviewer #2 for pointing out this poor phrasing. We totally rephrased this sentence, as detailed in the response to your next comment.

RV50. L347-348: One thing is defining the threshold using a refined Otsu approach, and another is deriving the waterline contour using a matching squares algorithm. This is not clear in the text at the moment.

We thank Reviewer #2 for this comment. The process of detecting the waterline from a SCoWI image involves two key steps: first, the definition of the threshold using a refined Otsu approach, and second, the delineation of the waterline contour based on this threshold using a marching squares algorithm. We have rephrased the sentence to clearly differentiate these two steps (lines 493-496).

RV51. L349: GHSSH not defined

Good catch ! GHSSH was indeed not associated with a reference. We fixed this issue (lines 497-498).

RV52. L348-350: Please be consistent with the use of tense. E.g., '..waterlines are then…' and '…constituted…'. Please check consistency throughout the manuscript.

This is an good point, thank you. We revised the manuscript to ensure consistency in tense usage throughout the text.

RV53. L355: typos: 'the time series…' & 'positions'. Please check the use of singular instead of plural when referring to waterline positions throughout the manuscript.

We thank Reviewer #2 for pointing this out. We corrected this typo and ensured consistent use of singular and plural forms throughout the manuscript.

RV54. L345-359: Please rephrase this sentence. I know what you are saying is that at each time step, you applied a tidal correction based on the estimated tidal elevation and the mean transect slope using linear translation, but this section is poorly written and rather convoluted.

We rephrased the paragraph to clarify the description of the tidal correction process (lines 503-509). The term "Tide-induced horizontal displacement of the waterline" which was unnecessarily wordy, has been replaced by "Tide correction term" for improved readability and conciseness.

RV55. L359: several studies have demonstrated that including run-up corrections is critical at low gradient sites. Figrue S7 suggests that a large proportion of the NAWC sites are dissipative, yet you are not including runup corrections. Given this area is rich in data both measured and high-quality modelled data, could you provide some justification/explanation of your choice? How do you think this might (or not) influence your results?

This is an interesting remark. Our approach is justified by 2 main reasons. 1) Wave runup corrections for satellite-derived data are usually wave setup corrections (as solving the runup phase it out of reach) derived from empirical wave setup correction formula like *Stockdon et al. (2006)*. While this type of formula have locally proven good results in reducing errors associated with remotely sensed shoreline positions, several recent studies (e.g., *Toomey et al., 2024*) point out that the systematic use of empirical formula to estimate wave setup over large scale can lead to erroneous results, especially when wave setup/runup is calculated from coarse datasets like the one used in our study. 2) Wave setup-induced variations of the sea-level at the coast are influenced by climate patterns such as the seasonal cycle and interannual variability of wave climate and are therefore part of the climate-driven waterline variability we are exploring in this study. This relates to point RV10 on distinguishing between SDW and SDS: non-astronomical coastal water level fluctuations are an integral part of the SDW signal we analyze. And (this is a second order argument) we also note that wave setup corrections made in several studies (e.g., *Vos et al., 2023b*; *Graffin et al., 2023*) do not affect significantly standard error, which is the most relevant metric to minimize when studying waterline/shoreline variability.

RV56. L363-366: It is good to see that someone has considered the differences between satellite missions!

We appreciate that Reviewer #2 find this methodological point relevant !

RV57. L388: Most studies use 100-150m-spaced transects (Mao et al., 2021; Castelle et al., 2022; Vos et al., 2023), and this is still often not dense enough especially for small pocket beaches. Could you please explain why you chose to use a wider transect spacing especially as the NAWC coastline also includes many pocket beaches?

The choice of a 250 m transect spacing was a deliberate trade-off between achieving a sufficiently detailed resolution and managing the computational cost of a study at this continental scale. While we acknowledge that a 250 m spacing might under-represent small pocket beaches—many of which are indeed present along the NAWC coastline—we considered that capturing regional-scale patterns of shoreline change was the primary objective of this study.

RV58. L402: '… the average value of the signal…' please clarify what you are referring to for the benefit of the reader.

We rephrased the corresponding sentence : "as the average of the monthly-sampled detrended waterline positions, noted $\overline{X^d_{wm}}$" (line 556-557).

RV59. L403: please clarify what Xw and Xtrend are for the benefit of the reader.

We rephrased the corresponding sentence : "where X_w is the waterline position time series, X_{trend} the linear component of waterline change derived from X_w, and $X^d_w = X_w - X_{trend}$ the detrended waterline position. N_m is ..." (lines 558-560).

RV60. L421-423: If storm track and storm wave intensity are parameters of interest as stated in L89-90 but also throughout the manuscript, why not use some more direct measure of these? If not, please provide some link between them and the EKE.

See response to RV2.

Supplementary

RV61. Figure S3: please indicate the location of the Columbia River Littoral Cell somewhere.

Thank you for this suggestion. We have updated Figure 1 to indicate the location of Long Beach, which is part of the Columbia River Littoral Cell. Additionally, in Figure S3 (now Figure S8), we replaced the mention of the Columbia River Littoral Cell with Long Beach for consistency and clarity. This should help readers better locate the site.

RV62. Figure S5: please provide a legend for the colour scale used in b-e. What does the colour represent?

Good catch! We added a color legend to Figure S5 (now Figure S10) to clarify the values and meaning of the color scale, which represents the density of points in panels (b–e). This addition ensures the figure is self-explanatory.

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Point-by-point response to reviewers

We appreciate the time and effort the editor and reviewers have spent reading and reviewing our revised manuscript. In this document, we address/clarify the remaining points raised by Reviewer #1. Please note that the reviewers' comments and questions are shown in black, and our responses are shown in blue.

Reviewer #1

Lines 121–148: The lateral boundaries of the PNW, NCA, and other study areas are not clearly defined in climatological or geomorphological terms. In Figure 1, the rationale for using a rectangular boundary is unclear. If waterlines are the primary focus, offshore and nearshore limits might be more appropriately defined by features such as the dune front or depth of closure. The geomorphic processes influencing responses to ENSO are not well articulated. Were any additional studies conducted to confirm that parameters such as beach slope, substrate lithology, or riverine sediment inputs do not significantly affect waterline positions on a seasonal scale? These factors appear to be omitted from the seasonal discussion in Figure 2. While this version addresses the points raised in my previous review, incorporating considerations beyond statistical projections—such as relevant physical processes—could enhance the study's rigor and its appeal to an international audience.

We thank Reviewer #1 for these general comments.

As mentioned in our previous responses, the subdivision of the coastline into subregions was based on a combination of geophysical and practical considerations. These include wave climate considerations, as well as the need to ensure a relatively balanced number of transects per subregion. We have slighlty deepened the description of the subregions to mention additional geomorphological and/or climatological characteristics of each subregion. For example : "Also, the PNW is situated along the Cascadia Subduction Zone, exposing the area to hazards associated with active tectonics". Regarding the rectangular boundaries shown in Figure 1, they are intended solely to delineate the subregions and should not be interpreted as defining the offshore or nearshore extents of the coastal system. This is now outlined in the caption of the figure. We agree with Reviewer #1 that depth of closure and dune front are appropriate coastal features to define the boundaries of the beach system. Flatter beaches are expected to be more sensitive to seasonal and interannual sea-level fluctuations compared to steeper ones. Similarly, lithology might play a role in the extent of the morphological response to a given forcing. However there is no evidence, to our knowledge, that these factors alter the timing of morphological response to the forcing, or at least not consistently at the regional (and at larger) scale, which is the focus of the study. Riverine sediment supply is more complex: while liquid discharge often shows a seasonal pattern, the solid discharge that impacts coastal morphology tends to be dominated by episodic flood events with multi-year return periods (*Warrick et al., 2022*; *Barnard & Warrick, 2010*). Moreover, the influence of fluvial sediment input varies spatially, largely depending on proximity to river mouths. We expanded the Discussion section to discuss the influence sediment supply on waterline variability :

"Additionally, an overlooked aspect of climate-driven waterline variability is the influence of riverine sediment supply. While the influence of riverine sediment supply on waterline variability is undoubtedly an important factor, its detailed assessment remains largely out of reach with current tools and data, especially at the seasonal scale. Solid sediment input that drives coastal morphological changes is typically linked to infrequent, localized, high-magnitude flood events with multi-year return periods (*Barnard & Warrick, 2010*; *Warrick et al., 2023*)."

Reviewer #2

I would like to commend the authors for their efforts and attention to detail in their responses to reviewers' comments. The authors have clearly spent a lot of time addressing and integrating extensive reviewer comments into their manuscript, and I hope they feel that their efforts have resulted in a significantly improved manuscript.

Their revised manuscript they have made substantial changes, including extensively restructuring the original manuscript, incorporating additional background information and reference to relevant literature, and providing additional quantitative results. Their work addresses some long standing and highly relevant questions in the field of coastal dynamics the relevance and importance of which is now clear and well supported. The authors also now highlighted some of the key novelties of their study which, in my view, were previously understated. Overall, I find this is a manuscript of very high quality that represents a significant contribution in the field. In my view, it is ready for publication, and I am looking forward to seeing it published in due course.

Kat Konstantinou.

We thank Kat Konstantinou for this feedback as well as for the very constructive and complete review she provided. We believe it significantly helped us improving the quality of the manuscript.

Review of the manuscript titled *Waterline responses to climate forcing along the North American West Coast*

SUMMARY & GENERAL COMMENTS

This work addresses some long standing and highly relevant questions in the field of coastal dynamics that has only been made possible in recent years thanks to the rapid increase in satellite-based shoreline extraction capabilities. The authors apply a recently developed method for extracting waterlines from optical satellite imagery to investigate the relationship between large scale climatic forcing and coastal response along the western coast of the US. They derive monthly timeseries of waterline positions along 7000+ ~250m-spaced transects and analyse the monthly and seasonal signals alongside monthly wave power, wave direction, and sea level data. Their findings indicate spatially variable coastal response across the region that mirrors difference in incident wave power and wave direction as well as sea level. The authors hypothesise that climate modes are most likely the key drivers of a large portion of the observed variability in waterline position and perform statistical analysis of the waterline position anomalies and different expressions of ENSO. This analysis leads to only weak and mostly statistically not significant relationships.

The manuscript could benefit from a stronger statement as to the nuance of the work and findings presented, however, my main comments are as follow:

RV1. **Structure**: The introduction does not provide the necessary background information although most of this is present in the results and discussion sections. The are also several sections in the results section that would be more appropriately included in the discussion section. Therefore, I would suggest restructuring the manuscript so that the introduction includes all the necessary background information, the results section includes the presentation of results, and the discussion section only includes a critical analysis/interpretation of the findings. Several suggestions have been made in the specific comments that follow.

RV2. **Methods**: the methods are clear. However, it is not always clear which approaches the authors have adopted from previous works and which they are contributing to this work. Also, they do not provide much explanation as to why the authors use the Eddy kinetic wind energy as a proxy for storm track direction instead of the parameters of interest, i.e., storm wave intensity and direction. It would also be useful to provide some link between storm wave parameters of interest and the EKE.

RV3. **Uncertainty**: The authors consider various sources of error and uncertainty yet in some cases, it is not clear how uncertainty is incorporated in the results (e.g., RV43). In other cases, it is not clear how some uncertainty limits have been derived (e.g., RV20). Some further clarification would be beneficial.

RV4. **Results**: There is a general absence of quantified results which means that most conclusions need to be draw from figures. This also applies to relationships between parameters (e.g., how SLA is related to waterline anomaly) which can only be qualitatively (and subjectively) draw from figures. A major issue for me is that although the authors stress the key role of wave forcing and storm impacts as a primary driver of coastal response throughout the manuscript, they do not perform any relevant analysis. I strongly feel this is missing (please also refer to comments RV16 to RV35).

RV5. **Discussion**: I do not feel the authors discuss their findings and main claims appropriately in the context of the wider literature nor do they discuss the implications of their findings. Importantly, I do not feel the analysis presented supports their main conclusions. The absence of adequate quantification of the results they discuss in combination with the fact that they have not addressed wave forcing despite stressing its importance makes the discussion weak and often, unsupported (please also refer to comments RV36 to RV41).

RV6. **Conclusions**: I feel the findings resented do not provide strong evidence for the main conclusions. Please refer to comments RV42 and RV44 for specific details.

SPECIFIC COMMENTS

1. Introduction

RV7. L36-51: I understand where the authors are going with this paragraph. However, I feel there is some confusion in the terminology with respect to coastal change indicators (see next comment below). There is some well-established literature on the topic that is not considered here, so I would suggest revising the use of the term 'indicators' within the framework of established literature.

RV8. L38-40: You are referring to coastal change indicators and although waterline is one such indicator according to (Boak & Turner, 2005), dune erosion and beach slope are beach characteristics rather than coastal change indicators. The dune toe, vegetation line, any elevation-based beach contour for example can be coastal indicators, and therefore, changes in these can be considered as coastal change indicators. I would suggest rephrasing these two sentences to consistently refer to either coastal change indicators or beach parameters commonly monitored.

RV9. L49: By 'one-line' are you referring to 2D, linear indicators as opposed to area (e.g., subaerial beach) or volume metrics?

RV10. L50-51: I am not sure I understand this. SDS is based on the delineation of the land/water interface. That really is the definition of the (instantaneous) waterline.

RV11. L60-64: please provide some relevant bibliography

RV12. L71-L77: I feel the reader would benefit from a bit more explanation and detail as to i) what these effects are on the wave climate, and ii) Why and how are coasts and coastal ecosystems affected. This can then provide the motivation for this study. At the moment I feel this paragraph is too general and vague to introduce the reader to the topic and the value of this investigation. As a result, the motivation is not clear.

RV13. L79: Why is this relevant? Almar 2023 may have investigated the influence of ENSO as the authors are, but other studies (apart from Almar 2023) exist that have performed regional analyses at higher spatial scales (e.g., Vos et al., 2023; Castelle et al., 2024; Warrick et al., 2025). Please provide a few words to also recognise that other large-scale studies have been performed at finer spatial scales and justify your choice of scale.

RV14. Figure 1: Could you please indicate on the map where Long Beach, Monterey, Torrey Pines, and Playa el Suspiro are. Also, I feel this figure should appear later on in the results sections.

RV15. L94-L98: Could you please provide some indicative figures (e.g., winter/summer Hs, Tp, etc)?

2. Results

RV16. General: You provide acronyms for the four main regions but do not define them specifically. In the text, you sometimes use the acronym and sometimes a description (e.g., southern California) but it is not always clear which are you are referring to. It would be beneficial to link the two (perhaps in the Figure 1 legend?) to increase clarity.

RV17. General: You mention that changes in storm tracks and storminess are a critical factor behind the coastal responses, but you provide not quantification of these and how they vary across the NAWC. Yet you do prove some very nice and detailed figures illustrating the impact of ENSO on SLA (-8.0 cm – 8.0 cm) and discuss the waterline response to that. I am perplexed as to this disparity. If your findings indicate the coastal response to 16 cm SLA is more pronounced than its response to increased storminess and changes in storm tracks, please provide some supporting evidence.

RV18. L101: Do you mean you average 125m either side of each transect?

RV19. L103: Good choice! It is great thinking that we have data since 1984 but in reality, the data we have prior to 2000 is so sparse it severely limits our analysis capability.

RV20. L115: How was the 0.5m/year threshold calculated?

RV21. L126: It is not clear to me at least how the comparison between seasonal and overall signal was performed.

RV22. Figure 2: This is a great plot that demonstrates the variability in the wave climate along the NAWC verry nicely. However, the latitude scales in (a) and (b-e) do not match so it is difficult to make direct comparisons. Please align the two y-axis. Also, please clarify 'Cycle range' in (a) refers to waterline excursion.

RV23. L145-150: I can see in Figure 2a that there is a significant drop in wave power south of 34°N accompanied by an equally significant difference in wave direction (Figure 2c) and a noticeable shift in the timing and magnitude of negative SLA (Figure 2d). But the switch in the timing of the erosion/accretion cycle seems to happen much further south, below 28°N and does not look like it could be associated with the timing of the SL seasonal cycle that switches much further north, at least as shown in Figure 2. Are you referring to some previous work that has linked the two? Also, it seems to me that below ~25°N the shoreline orientation is more southward (Fig2a) and the prevailing wave direction (Fig2c) so wave incidence is more oblique. There also seems to be a definite shift in wave power seasonality with lower wave power during the winter months and higher during the summer months. So very plausibly, the shift in the waterline circle may reflect the aforementioned conditions rather than a 7-25cm SLA.

RV24. L162-163 & Fig3: I will return to my earlier comment (RV12) that not enough information on the behaviour and influence of ENSO on the wave climate has been provided and as a result. Please include the relevant background info in the introduction.

RV25. L163: The PDO is not introduced until L176. Please introduce in the first instance. Also, please introduce it in the introduction and discuss interactions with ENSO.

RV26. L178-180: I am not sure I understand why you characterise your 'time coverage' as limited. Are you referring to the 1997-2021 period? If yes, I am guessing existing knowledge indicates the

PDO has larger cycles? Again, this is not at all clear primarily because the background information is missing in the introduction.

RV27. L174-182: Perhaps I am missing something, but I feel this is more of a discussion than the presentation of your results. Because of the lack of adequate information on the behaviour of ENSO and its influence on the regional wave climates, and (complete absence of info on) the PDO, these statements are not supported here. I am also not sure I can see the notably less pronounced influence of ENSO on PNW and NCA. For example, despite the admittedly much greater variability in the waterline anomaly in PNW, both PNW and NCA plots show a drop in the 2010 event followed by a steady increase up to 2016 and another large drop in 2016. This behaviour is very closely mirrored by CP in panel (g), at least visually. Some quantitative results are necessary.

RV28. L188 The PMM has also not been introduced or discussed thus far. Please provide the necessary background information on the relevant climatic patterns in your introduction

RV29. L183-190 Whether a correlation is positive or negative has no effect on its strength or statistical significance. Here you characterise a correlation as 'weak yet positive'. I imagine 'anti-correlations' refer to negative correlations? Also, you are characterising similar values differently, sometimes referring to correlations in the order of 0.4 as weak (e.g., L196) and sometimes not. Lastly, please clarify if you are reporting R (Pearson or Spearman?) values and what level of statistical significance you are considering prior to reporting your findings.

RV30. L196-2001: How do the two phases of ENSO relate to or are expressed in CP and EP? This should also be mentioned in the introduction.

RV31. L202-220: (please also refer to comment RV17). You mention that changes in storm tracks and storminess are a critical factor behind the coastal responses, but you provide not quantification of these and how they vary across the NAWC. The discussion you have included may be correct, but you provide the reader with no evidence, other than the waterline response shown in Fig4, of the forcing conditions during these events and how they change. You use the EKE as a proxy for quantifying changes in storminess and show the contours on Fig4, but you provide no quantification of how these relate to '…significant changes in coastal wave extremes.' Please provide some information and a figure to demonstrate the spatial distribution as they should be at least if not more important than the SLA shown in Fig4.

RV32. L204: typo: remove full stop after the word 'phase'.

RV33. L202-228: A large part of this section discusses the influence of ENSO on the wave climate. This should be part of your introduction, not your results. This is what will set the scene for your waterline analysis.

RV34. L216: you have already introduced EP and CP; there is no need to repeat here.

RV35. L220-241: this is more of a discussion section.

3. Discussion

RV36. General: I am missing a discussion on how your findings compare with the findings of previous works. I am not an expert in your area, however, I am aware of at least on publication you also cite (Vos 2023). How do your results compare with theirs? Why do you think there are differences if these exists? Are your findings in line with current understanding of how this stretch of coastline has evolved? If not, what new have we learnt?

RV37. General: I am also missing a discussion on the implications of your findings. Are there any observed trends in ENSO? If yes, how do you think the NAWC might be affected in the future?

RV38. L265-270: Here you partially address my comments RV17 and RV31. Yet throughout your manuscript you refer to the influence of ENSO on storminess. I have a few comments relating to this:

I. Several studies have demonstrated storm links between ENSO and wave conditions as you also indicate (e.g., L265-266). Do you have an explanation why in your case you did not find any?

II. Could you please indicate the location of ERA5 node you used for the comparison shown in Figure S3?

III. The absence of statistically significant correlations between wave power and ENSO (in one location) does not imply the absence of a relationship between wave power and waterline response.

RV39. I am not surprised you find weak and mostly not statistically significant relationships between ENSO and coastal response as your finding also agrees with previous work. However, with this you have, at least partially, disproved your hypothesis as stated in L155-158. You stress the key role of wave forcing and storm impacts as a primary driver of coastal response throughout your manuscript, yet you do not perform any relevant analysis. I strongly feel this is missing.

RV40. L267: This is the first mention of MEI. Please include and explain earlier in the manuscript.

RV41. L293-297: you mention the importance of the combined impact of elevated water level and storm impact, yet miss the opportunity to demonstrate this at monthly, seasonal, or annual timeframes. Though it is possible you find equally weak relationships due to the inherent noisiness of SDS and (potentially) the quality of the wave data, I feel that without this analysis

4. Conclusions

RV42. L318-321: I am perplexed at this statement as your findings indicate weak and mostly not statistically significant relationships between climatic patterns and coastal response and you have not shown any quantitative results relating wave energy to coastal response. Including a plot of wave power in Fig2b, in my view is not sufficient to support this conclusion.

RV43. L324-325: Given the accuracy of the method as reported by Bergma 2024 (~10m), would these results not indicate that most of the observed variability is within the uncertainty f the method, i.e., noise?

RV44. L332-335: you have shown the 200hPa isobars in Fig4 and explain you are using EKE as a proxy for changes in storm track but do not perform any analysis to support this statement. As a matter of fact, the southern tip of the Baja peninsula shows the largest negative (erosion) anomaly in Fig4a, d, and e and this does not seem to relate to changes in storm track.

5. Methods

RV45. L337: typo: 'method**s'**

RV46. L338: typo: replace ‘it consists in a…’ with: ‘it consists **of** a…’ however, the use of this expression is not a good use of the English language in this case.

RV47. L341: Who validate the application to the Landsat series? If I am not mistaken, Bergsma 2024 mention that validation of the application of their method to the Landsat series imagery would constitute further work ….

RV48. L345: I believe Bergsma 2024 used the cirrus band and cloud mask in S2 to filter out cloud-contaminated images. If the SCoWI method is part of your work (or of others) please clarify and/or reference accordingly)

RV49. L347: Highlighting the poor use of the expression: ‘thanks to…’ in this case.

RV50. L347-348: One thing is defining the threshold using a refined Otsu approach, and another is deriving the waterline contour using a matching squares algorithm. This is not clear in the text at the moment.

RV51. L349: GHSSH not defined

RV52. L348-350: Please be consistent with the use of tense. E.g., ‘..waterlines are then…’ and ‘…constituted…’. Please check consistency throughout the manuscript.

RV53. L355: typos: ‘the time series…’ & ‘position**s**’. Please check the use of singular instead of plural when referring to waterline positions throughout the manuscript.

RV54. L345-359: Please rephrase this sentence. I know what you are saying is that at each time step, you applied a tidal correction based on the estimated tidal elevation and the mean transect slope using linear translation, but this section is poorly written and rather convoluted.

RV55. L359: several studies have demonstrated that including run-up corrections is critical at low gradient sites. Figrue S7 suggests that a large proportion of the NAWC sites are dissipative, yet you are not including runup corrections. Given this area is rich in data both measured and high-quality modelled data, could you provide some justification/explanation of your choice? How do you think this might (or not) influence your results?

RV56. L363-366: It is good to see that someone has considered the differences between satellite missions!

RV57. L388: Most studies use 100-150m-spaced transects (Mao et al., 2021; Castelle et al., 2022; Vos et al., 2023), and this is still often not dense enough especially for small pocket beaches. Could you please explain why you chose to use a wider transect spacing especially as the NAWC coastline also includes many pocket beaches?

RV58. L402: ‘… the average value of the signal…’ please clarify what you are referring to for the benefit of the reader.

RV59. L403: please clarify what Xw and Xtrend are for the benefit of the reader.

RV60. L421-423: If storm track and storm wave intensity are parameters of interest as stated in L89-90 but also throughout the manuscript, why not use some more direct measure of these? If not, please provide some link between them and the EKE.

6. Supplementary information

RV61. Figure S3: please indicate the location of the Columbia River Littoral Cell somewhere.

RV62. Figure S5: please provide a legend for the colour scale used in b-e. What does the colour represent?

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