Spatio-Temporal Changes in China's Mainland Shorelines Over 30 Years Using Landsat Time Series Data (1990–2019)

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Reply to comments:

We sincerely appreciate the reviewers' thorough review and constructive feedback. Your insightful comments have been instrumental in enhancing our manuscript. In response to the suggestions, we have provided an item-by-item response follows, with the original feedback highlighted in italics for clarity.

There are three major modifications in the revised manuscript: (1) Change from "Accuracy verification" to "Consistency test": we have revised all relevant statements regarding the comparison with the reference datasets, and added a discussion on the limitations of this consistency test. (2) Forces driving shoreline change: The analysis of shoreline change drivers has been moved from the Results section to the Discussion section, and we have also expanded the discussion to address the limitations and implications of using shoreline length as a metric. (3) More comprehensive discussion: Sections 5.1 and 5.3 of the original manuscript have been integrated into Section 5.4, providing a more comprehensive discussion of the advantages and limitations of the proposed method and dataset.

Additionally, we have made minor revisions to several statements, updated figures, and added new references to enhance the manuscript's reliability.

Once again, we are particularly grateful for your careful reading and constructive comments. Thank you for taking the time to enhance our work.

Anonymous Referee #1: *This research produced the first 30-year spatio-temporal change analysis of China's mainland shoreline based on the time series data of Landsat images from 1990 to 2019 obtained from GEE platform. The datasets are widely covered and complete. The experiments and result analysis are interesting and sufficient. The discussions and conclusions are effective. However, there are some points needs to be clarified before accepted.*

Response:

Thank you for your thorough review of our manuscript. We appreciate your recognition of the completeness of our datasets, as well as the effectiveness of our discussions and conclusions. We have carefully considered your comments and have made the clarifications and revisions to address the points you raised. And we have responded to your points one by one below.

Major points:

1.In the shoreline extraction step, the classic threshold segmentation method Otsu algorithm is used to segment the grayscale MNDWI images into water bodies and non-water bodies. And then, to extract water body, pixels corresponding to lakes and reservoirs are removed by geographic distribution and area sizes. In Line 175 of the manuscript, this study employed an area parameter and select the largest water body for each object to effectively eliminate interference caused by terrestrial water bodies. As well known, Otsu algorithm is only a thresholding algorithm, which only can segment image to isolate label points. However, to extract water bodies, connected segmentation regions are necessary. The thresholding algorithm and the post-processing step is too simple. We doubt the accuracies and robustness of the extracted shoreline.

Response:

Thank you for your valuable feedback. We understand your concern about the proposed method. To address this, we would like to clarify and provide additional context on the method we employed. The Otsu algorithm was applied to determinate thresholds primarily as an initial step to segment the MNDWI images into water and non-water pixels based on grayscale values. This provided a basic separation of the two classes, which is essential for subsequent processing. The water area obtained at this time includes connected marine regions, as well as small isolated water bodies such as lakes and reservoirs within the land area. We used ArcGIS 10.4 software to convert binary images into vector and remove isolated water bodies by area. Finally, convert the marine polygons into lines and perform smoothing processing. We recognize that this approach may seem overly simplistic, and we appreciate your concern regarding the robustness and accuracy of the shoreline extraction. To ensure the reliability of our results, we compared the extracted shorelines with other products. Our findings indicated that the approach can achieve results with high consistency with other shoreline products based on remote sensing images, though we acknowledge that there are potential areas for improvement. In the revised manuscript, we have added a discussion (Section 5.4) about these limitations and potential future improvements in the shoreline extraction section. We hope this clarification addresses your concerns, and we are open to any further suggestions you might have to improve the robustness and accuracy of our methodology.

Minor points:

1.In Line 190, the symbols in equations (2) and (3) are not defined, such as , , .

Response:

Thank you for pointing out the omission of symbol definitions in equations (2) and (3). We apologize for this oversight and appreciate your attention to detail. We have revised the

manuscript to include clear definitions for all the symbols used in equations (2) and (3) (Lines 200-202).

2.In Line 205, What are EPR and LPR? We can find the full name in Abstract part but not in the method.

Response:

Thank you for highlighting the need for clarity regarding the terms EPR and LPR. To address this issue, we have revised the manuscript to include the full names and definitions of EPR (End Point Rate) and LPR (Linear Regression Rate) when they are first introduced in the methodology section, specifically on Line 213.

3.The labels of subfigures are confusion. In most of Figures, the labels of subfigures are denoted as (a), (b), (c), (d). But in Figure 7 and Figure 10, the labels of subfigures are denoted as a, b, c. In Figure 14, A, B, C are used.

Response:

Thank you for bringing this inconsistency to our attention regarding the labeling of subfigures across the figures in the manuscript. We agree that consistent labeling is crucial for clarity and readability. To address this issue, we have standardized the labeling of subfigures throughout the manuscript. We have revised figures to use the same labeling format: (a), (b), (c), (d) (Lines 288, 369 and 459).

4.In Line 170, "grayscale MNDWI binary images" should be "grayscale MNDWI images" we think.

Response:

Thank you for catching that error. We have revised Line 175 to correct the "grayscale MNDWI binary images" to "grayscale MNDWI images" in the revised manuscript.

Anonymous Referee #2:

General comments

I thank the authors for their paper presenting a new method for mapping coastal change using satellite remote sensing data over large spatial extents. The approach presented here is scientifically sound, and I believe it will be of interest to readers of Earth System Science Data with an interest in large-scale coastal mapping. However, I believe there are several major and minor areas where the paper should be improved prior to publication.

My primary critism of the manuscript is its reliance on validating modelled remote sensing-based coastal change results against other modelled remote sensing-based coastline datasets. This particularly applies to the use of GSV and Coastline_ECS, which are both also Landsat-based shoreline mapping datasets. This comparison does not effectively verify the accuracy of the data being presented in this study: it serves more as a test of "consistency" with previous approaches (with consistency not necessarily being a good thing if these previous datasets were inaccurate themselves) rather than a "validation". I feel the paper would strongly benefit from additional validation comparing the results here to real-world validation data (e.g. beach surveys etc) at least a number of coastal sites, providing additional confidence that this study is indeed producing accurate results and not simply re-producing (potentially inaccurate) existing datasets. In addition, caveats and limitations of comparing modelled results against other modelled datasets should be discussed in detail in the paper.

The paper also uses shoreline length as a key metric for comparing coastal change over time. Shoreline length is a notoriously problematic metric, being essentially unmeasurable and scale-dependent due to the "Coastline paradox", and highly influenced by noise which can be variable over time or between different satellite sensors (e.g. Landsat 5 vs Landsat 8). While I would strongly advice the authors choose another metric for comparing coastal change over time, if they wish to continue using shoreline length the limitations of this metric should be discussed and documented clearly in the paper.

Finally, the current Discussion section feels very brief and poorly referenced. I believe a significant amount of material currently contained in the Results section could be moved to discussion, and the existing Discussion material could cite and discuss existing literature in more detail. I have also suggested a number of areas below where limitations and caveats of the proposed method could be discussed to allow readers to gain a more informed understanding of the advantages and limitations of the approach.

Response:

Thank you for your detailed and insightful comments. We appreciate the opportunity to improve our manuscript based on your feedback.

Additional validation using real-world validation data: We agree that the comparison in Section 4.1 serves more as a test of "consistency" with previous approaches rather than a "validation". Therefore, we have revised the section title and the wording of the relevant content. Due to confidentiality reasons, we are unable to obtain and publicly display the measured data of the coastline for accuracy verification. On the other hand, the measured coastline data from the management department is difficult to match our results in terms of definition and resolution. Our results come from remote sensing images with a resolution of 30 meters, while the measured coastline is derived from high-precision surveying work. We use tidal station data to

select the images closest to the peak moment, but we cannot guarantee that the satellite's transit time will be exactly at the peak moment. And the measured coastline is selected based on the maximum high tide line within thirty years. This leads to a large difference between the two in the mudflat. Furthermore, the coastline of some aquaculture areas does not match the actual high tide line due to management reasons. Therefore, we still adopt consistency testing with the reference dataset instead of using actual data for accuracy verification. In addition, we also paid attention to visual inspection with the original Landsat images and higher resolution images when extracting the shoreline production dataset. We also add a section (Section 5.1) discussing the caveats and limitations of relying on modelled datasets for validation in the revised manuscript.

Shoreline length as a key metric: While we acknowledge that shoreline length may not be an ideal metric in all contexts, we argue that the methodological steps taken—particularly the use of the HANTS and the emphasis on large-scale trends—significantly reduce the uncertainties associated with this metric. Nevertheless, it is important to consider these limitations when interpreting the results, and we have carefully documented the potential pitfalls in this regard in Section 5.2.3 in the revised manuscript. To address the inherent noise and variability introduced by different satellite sensors, we applied the HANTS to smooth the time series data and reduce the impact of short-term fluctuations and sensor-specific inconsistencies. This method effectively filters out much of the random noise, enabling a more consistent and robust comparison of shoreline length over time. Furthermore, by focusing on regional-scale shoreline trends rather than small-scale local variations, the analysis mitigates the sensitivity to the scale dependency emphasized by the coastline paradox. Future studies could benefit from exploring additional metrics, such as shoreline position or area change, to complement the findings presented here.

Enhancing the discussion section: We agree that the discussion section could be expanded and better referenced. We revised this section to move relevant material about forces driving shoreline change from the results section, provide a more comprehensive discussion of our findings in the context of existing literature, and address the limitations and caveats of our method in greater detail. Specifically, we have added Section 5.1 and Section 5.2.3 to discuss the limitations of consistency test and shoreline length as a key metric. We have reorganized Section 5.1 and Section5.3 of the original manuscript and added a discussion on tidal effects. Section 5.4 of the revised manuscript provides a more comprehensive discussion on the advantages and limitations of the proposed method and dataset. In addition, we have highlighted the newly added references in red font in the revised manuscript.

We believe these revisions will address your concerns and strengthen the manuscript. If there are any specific suggestions or additional comments you would like us to consider, please let us know. Thank you once again for your constructive feedback.

Specific comments

Lines 130: As the authors recognise, accounting for tide in large-scale coastal remote sensing analyses is critical. However, the current manuscript does not provide sufficient detail about how Landsat imagery was filtered by tide. In particular, "based on high tide times" should be replaced with specifics about how these high tide images were selected (e.g. tide height threshold? top X percent of tides etc?).

Response:

Thank you for pointing out the need for more specific details regarding how Landsat imagery was filtered by tide in the manuscript. We have revised Lines 121-123 of the manuscript. For the mudflat area greatly affected by the tide, we screened the Landsat image closest to the high tide according to the high tide time of the tide station and the imaging time of the satellite image.

Lines 130: In additional, more detail should be provided about how point tide gauge locations were mapped to continuous coverage satellite imagery. Were tide heights interpolated to each image, or assigned based on the closest tide gauge? How did the authors ensure that tides observed at this small number of locations (17) were representative and applicable to satellite imagery away from these gauges, particularly in areas of complex tide dynamics or in areas located far from the nearest tide gauge? This ideally would include some discussion around alternative approaches used for accounting for tide in complex coastal environments (e.g. the use of global ocean tide modelling; Vos et al. 2019, Bishop-Taylor et al. 2021).

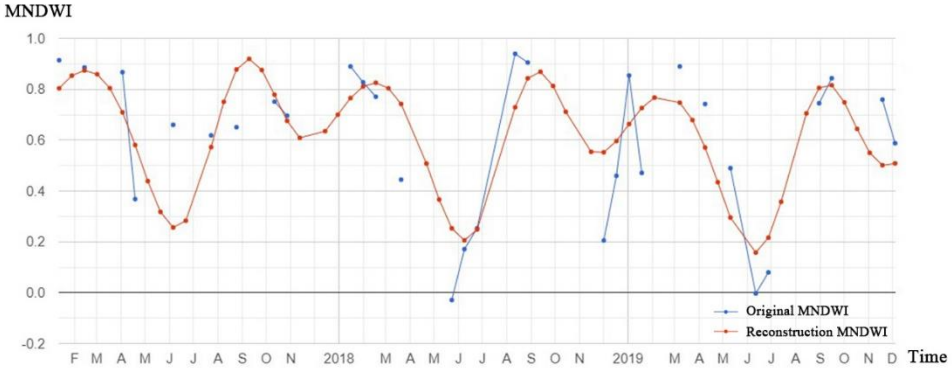
Response:

Thank you for this important observation on the method. We believe that the bedrock shoreline and artificial shoreline are not significantly affected by the tide, so we only consider the mudflat area that is greatly affected by the tide when matching the tide stations. In this study, we screened the Landsat image closest to the high tide according to the high tide time of the tide station and the imaging time of the satellite image. While this approach effectively captures key shoreline characteristics in areas with significant tidal coverage, the limited distribution of tide stations means that the results may not fully represent tidal conditions across the entire study area, particularly in regions with complex tidal dynamics or varying intertidal slope gradients. And in reality, we cannot guarantee that the satellite's transit time will be exactly at the peak moment. Therefore, our shoreline results differ from the maximum or average high tide line as the coastline, which only represents the water edge line close to the high tide level from remote sensing images. This dataset already meets the needs of analyzing the change trends and characteristics of large-scale regions, while avoiding the sensitivity of standard coastlines as confidential data. We have discussed this issue in Section 5.4 of the revised manuscript.

Line 160: The HANTS method presented here sounds very promising for a tool for handling noisy/sparse remote sensing time series. So that readers can appreciate how this approach works, please provide an additional figure demonstrating the HANTS approach being applied to several example pixels from this study (e.g. showing the effect of smoothing and gap filling).

Response:

Thank you for your positive feedback on the HANTS method. We have added an additional figure (Figure 4) to the manuscript that illustrates the application of the HANTS method to an example pixel from our study area. This figure will show:



Line 172: Was OTSU thresholding applied to each annual timestep individually, producing different thresholds for each year? Or was a consistent threshold derived and applied across the entire time series?

Response:

Thank you for raising this important question about the application of the Otsu thresholding method in our analysis. In our study, Otsu thresholding was applied to each annual timestep individually. This approach allows the threshold to be dynamically adjusted based on the specific characteristics of the MNDWI images for each year, accounting for potential variations in environmental conditions, sensor characteristics, and other factors that could influence the pixel intensity distributions over time. We revised Lines 176-177 to clarify this point in the manuscript. The updated text will explicitly state that the Otsu thresholding was performed separately for each year, leading to potentially different thresholds being used for each annual timestep.

Line 190: Did this offset calculation include directionality? (e.g. bias on the inland or seaward directions)

Response:

We have revised Line 196 to clarify that the offset calculation did not account for directionality. The revised manuscript stated that the offset was calculated without distinguishing between inland or seaward biases.

Section 4.3.2: This section currently goes into a little too much locally-specific detail for a journal with global readership - would suggest simplifying it and removing some of the current content. In addition, some of this material feels like it would more appropriately belong in Discussion instead of Results.

Response:

Thank you for your feedback regarding Section 4.3.2 of the original manuscript. We agree that the content should be adjusted to better suit a global readership and to maintain the appropriate focus for each section of the manuscript. We have moved Section 4.3 of the original manuscript to Section 5.2 of the revised manuscript and added a discussion on the shoreline length as a key metric.

Line 398: What is a "reconstruction" based threshold? Please clarify or use this term more consistently throughout the manuscript.

Response:

Thank you for your comment. We apologize for the misunderstanding caused by our expression in the original manuscript. Here, "reconstruction" refers to the reconstruction of the MNDWI time series. We have revised the sentence on Line 463 of the revised manuscript to provide a clearer explanation of our method.

Line 450: Based on the current validation, I don't think the statement "more accurate shoreline data compared to previous global shoreline datasets" can be justified, given that those global datasets were themselves used as a point of truth in the validation. Perhaps this could be justified if results of this study and those global datasets could all be compared to real-world, independent validation data.

Response:

Thank you for pointing out this critical issue. You're correct that using global datasets as reference datasets in the validation process makes it difficult to claim that our dataset is "more accurate" without independent validation. Therefore, we have removed this statement in the revised manuscript and re discussed the advantages and limitations of the proposed method and

dataset in Section 5.4.

Technical corrections
Line 405: Should this read "20 yearly observations"?

Response:
We have modified the sentence to express the long-term advantage of Landsat data (Line 470).

Data comments
*The transect point and line features are currently split into 7 individual features ("*_1.shp", "*_2.shp" etc). These would be much easier to use if these individual files were combined, so that users could analyse them at once without having to combine them manually first.*

Response:
Thank you for your feedback on the file structure for the transect point and line features. Combining the individual files into a single file would indeed streamline the process for users and facilitate easier analysis. We merged the individual shapefiles into a single unified shapefile for each feature type (points and lines) and updated the data (https://www.scidb.cn/en/anonymous/aXFNclkz). This will eliminate the need for users to manually combine the files.

Similiarly, the shoreline datasets would be easier to use if all years were combined into a single shapefile with a "year" attribute column.

Response:
We appreciate your suggestion to combine the shoreline datasets into a single shapefile with a "year" attribute column. However, we believe that keeping the datasets separate for each year provides greater flexibility and clarity in the analysis. Combining all years into a single file might complicate data handling and increase the risk of errors when managing large datasets. We are committed to supporting users in their analysis and will continue to provide clear guidance and resources for working with the separate annual files. If you have any further questions or need assistance, please do not hesitate to reach out. Thank you for your understanding.