Response to Reviewers

Dear Editor and Reviewers,

We sincerely thank you for the time and effort devoted to reviewing our manuscript entitled:
"Early Identification of Reservoir-Bank Landslides in Deeply Incised Mountain Canyon Areas with Interferometric Baseline Optimization."

We are deeply grateful for your constructive and insightful comments, which have been invaluable in improving the clarity, scientific rigor, and overall quality of the manuscript. In response to your suggestions, we have carefully revised the manuscript and addressed all comments point by point. Corresponding modifications in the text have been clearly highlighted in red for your convenience.

The main revisions include restructuring the Introduction to better highlight research background and innovations, refining the Abstract to emphasize scientific insights, improving methodological descriptions, adding a comprehensive workflow diagram, justifying key parameters and thresholds, supplementing time-lag analyses, enhancing figure clarity and captions, and standardizing equation formatting according to journal requirements.

Below, we provide our detailed, point-by-point responses to each reviewer's comments.

Response to Reviewer 1

Comment 1: Further clarify the innovativeness of the manuscript by explicitly listing the contributions in bullet points in the introduction.

Response: We thank you for this important suggestion. We have strengthened the final paragraph of the Introduction section by explicitly listing three main contributions of this study in bullet points:
(1) Proposed a vegetation-adaptive Weighted Coherence Threshold Method (WCTM) that establishes a vegetation-coherence coupling model to dynamically adjust coherence thresholds, effectively mitigating the impact of seasonal vegetation decorrelation on interferogram quality;
(2) Integrated ERA5 high-resolution meteorological reanalysis data with tropospheric delay modeling to significantly reduce atmospheric delay errors in complex terrain, thereby improving deformation inversion accuracy;
(3) Achieved high-precision early identification of reservoir landslides in the deeply incised alpine gorge area of the Baihetan reservoir region, demonstrating a 22% increase in identification rate compared to conventional methods and validating the method's applicability and effectiveness under extreme topographic conditions.
The modified text has been highlighted in red in the Introduction.

Comment 2: Generally, all variables and Greek letters should be italicized, while constants should not. Vectors and matrices should be boldfaced. Please ensure all equations in the manuscript comply with these formatting standards.

Response: We sincerely apologize for this oversight and thank you for bringing it to our attention. In the revised manuscript, we have systematically reviewed and corrected all mathematical equations, variables, and Greek letters throughout the text. We have ensured that all variables and Greek letters are italicized, constants remain upright, and all vector and matrix symbols are boldfaced, bringing them into full compliance with standard academic publishing format requirements.

Comment 3: Is the method proposed in the manuscript specifically targeted at reservoir bank landslides in deeply incised valleys? Given that "underwater color difference" studies have identified color variations between underwater and terrestrial environments, it is necessary to clarify whether this method can be generalized to other scenarios.

Response: We thank you for this insightful comment. While this study indeed uses reservoir bank landslides in deeply incised valleys as both the application context and validation scenario due to its representative and challenging nature, the core innovation of our method addresses the universal challenge of "radar interferometric decorrelation caused by seasonal vegetation changes." Therefore, its application potential extends beyond reservoir landslides. As you astutely observed, the technical framework (vegetation dynamics-aware interferometric optimization + atmospheric correction) possesses good generalizability and can be extended to other vegetation-affected scenarios such as monitoring of geological hazards in mountainous areas (e.g., landslides, collapses) and stability monitoring of engineering structures in areas with seasonal vegetation variations. We have added corresponding clarification in the Discussion section (end of Section 5.1.3), with the additions highlighted in red.

Comment 4: The authors may add future work prospects in the Conclusion section.

Response: We thank you for this suggestion. We fully agree that this enhances the completeness and forward-looking nature of our research. We have added a separate paragraph at the end of the Conclusion section (Section 5.2) outlining prospects for future work, primarily including: further optimization of the vegetation-coherence model, application of the method to more diverse geographical environments for validation, and exploration of its integration with emerging technologies such as machine learning. These additions have been highlighted in red in the manuscript.

Response to Reviewer 2

Comment 1: Although the introduction mentions the limitations of existing methods, it is necessary to further elaborate on the innovations and reflect the importance of the research problem.

Response: We greatly appreciate this suggestion. We fully agree with your perspective and have strengthened the final paragraph of the introduction. The revised text not only outlines the research methodology but also explicitly highlights the three core innovations and their significance in bullet points: 1) proposing a vegetation-adaptive WCTM method to address seasonal decorrelation challenges; 2) developing a multi-source data synergistic error correction scheme; and 3) validating the method's high-precision identification capability in extremely complex terrain. This better emphasizes the breakthrough nature and practical application value of our study compared to existing techniques. The modified content is highlighted in red in the introduction.

Comment 2: Issue regarding the basis for vegetation coverage classification thresholds: The paper directly presents three key vegetation coverage classification thresholds: 45%, 60%, and 75%, without detailing their scientific basis. The authors are requested to supplement the explanation: On what standards (e.g., industry standards, previous research) or data-driven analyses (e.g., natural breaks method, cluster analysis) are these thresholds based?

Response: We thank you for pointing out this important omission. You are correct that the basis for determining these thresholds needs to be clearly stated. We adopted an approach that combines data-driven analysis with references to existing literature. In the revised manuscript, we have added corresponding explanations in Section 4.1 "WCTM Optimization of Interferometric Baseline Results", with the modifications highlighted in red.

Comment 3: It is recommended to add a clear and comprehensive technical flowchart at the beginning of Chapter 3 to help readers quickly understand the process.

Response: We greatly appreciate this valuable suggestion. We have added a clear technical flowchart (which can be newly numbered as Figure 3) at the beginning of Chapter 3 "Research Methods and Data Processing", accompanied by a brief textual description. This visually illustrates the complete workflow from data preparation and core methods (WCTM optimization and atmospheric correction) to result output (deformation acquisition and landslide identification).

Comment 4: Some result description paragraphs are slightly verbose and repeat information clearly presented in the figures. It is recommended to summarize and generalize the accuracy of the results.

Response: We thank you for this correction. We have streamlined and optimized the relevant descriptions in Chapter 4 "Results and Analysis", particularly regarding the content of Figures 8 and 9. In the revisions, we reduced direct restatements of information readily available from the figures. Instead, we strengthened the summarization of deformation patterns, the analysis of landslide dynamic evolutionary characteristics, and emphasized the exploration of relationships between deformation results and driving factors such as reservoir water level fluctuations and precipitation. This shifts the focus of the discussion from describing observations to interpreting patterns, enhancing the depth and generalizability of the analysis. Specific modifications can be found in the highlighted Section 4.3 of the manuscript.

Response to Reviewer 3

Comment 1: Overall, the narrative of the manuscript is too verbose. For instance, in the methodology section, significant space is devoted to listing traditional techniques, while the method proposed in this study is only briefly introduced. This fails to highlight the advantages of the proposed method. The authors are advised to refine the manuscript to enhance its scientific rigor and readability.

Response: Thank you for this suggestion. We have restructured and streamlined the methodology section accordingly. In the revised manuscript, we have condensed the background introduction to traditional InSAR interferogram selection methods, retaining only parts directly relevant to our study. Simultaneously, we have provided a more detailed explanation of the technical workflow, parameter settings, and the rationale behind the proposed Vegetation-Adaptive Weighted Coherence Threshold method. This revision aims to clarify its applicability under conditions of seasonal vegetation decorrelation and complex terrain.

Comment 2: The authors use a threshold of 16 mm/year to identify potential landslide hazards. Please discuss the rationale for this choice.

Response: The selection of this threshold is primarily based on regional characteristics: (1) Referring to existing landslide investigation data in the area, deformation rates during the initial creep stage typically concentrate within the 10-20 mm/year range. (2) Sensitivity analysis, considering the Sentinel-1 data wavelength (C-band) and the region's annual average coherence level (approximately 0.3-0.4) in the deep valley terrain of southwest China, indicates that this threshold effectively distinguishes true deformation from atmospheric phase noise. Therefore, 16 mm/a was chosen as a relatively conservative criterion for preliminary screening of potential deformation zones.

Comment 3: There are many established atmospheric correction methods in InSAR data processing. Please compare these with the method used in this study, which combines ERA5 meteorological data and a tropospheric delay model for atmospheric correction.

Response: Thank you for this valuable comment. In this study, we indeed applied atmospheric correction to the InSAR data using ERA5 reanalysis data combined with a tropospheric delay model to improve the reliability of deformation results. However, the main focus of this work is on evaluating and optimizing interferometric baseline selection for early identification of reservoir-bank landslides, rather than systematically comparing atmospheric correction methods. Therefore, we did not perform comparative experiments among different atmospheric correction schemes. The employed atmospheric correction workflow has been clearly described in the Methods section, and we consider it sufficient to support the validity of our baseline optimization approach. In future work, we may investigate how different atmospheric correction methods could affect baseline optimization and deformation detection accuracy.

Comment 4: In Figure 6, the authors delineated the landslide boundary with a red polygon, but no obvious InSAR deformation signal is observed in these areas. Please explain this discrepancy.

Response: Thank you for raising this important point. The discrepancy between the InSAR deformation signals and the geomorphologically defined landslide boundary in Figure 6 is indeed a key observation, and we have reflected on it deeply. We believe this phenomenon aptly illustrates the intrinsic difference between long-term geomorphological features and short-term surface deformation monitoring, contributing to a more comprehensive understanding of the landslide's activity state and evolutionary history.

Specifically, the red polygon in the figure represents the geological boundary of the landslide, delineated based on high-resolution imagery and field surveys, using clear geomorphological features such as the main scarp, lateral tension cracks, and toe bulge. It signifies the overall potential extent of the landslide formed through its long-term geological history. In contrast, the InSAR technique captures minute surface deformation occurring during the specific monitoring period (e.g., 2022-2023), reflecting current activity. The spatial mismatch between the two can be attributed to several factors: the landslide may be in a state of overall, uniform slow creep, where the deformation rate or its spatial gradient is below the detection sensitivity threshold of the InSAR analysis. We fully agree on the importance of explicitly explaining the geological and monitoring significance of this discrepancy. Rather than being contradictory, it suggests that the landslide did not exhibit significant localized acceleration at its boundaries during the monitoring period, indicating a state of relatively stable creep. In response to your suggestion, we will clearly explain the definitions and differences of these two boundaries in the caption of Figure 6 and consider adding a brief discussion in the "Discussion" section on the complementary roles of geomorphological features and InSAR monitoring in landslide analysis to aid reader comprehension.

Comment 5: The reliability of the InSAR results in Figures 8(b) and 8(d) is questionable. It is difficult to discern whether these scattered pixels represent deformation, noise, or error. Furthermore, please explain the rationale for selecting time-series feature points P1 and P2. Additionally, it is suggested that Figures 2 and 3 be moved to the appendix.

Response: Regarding the reliability of scatter points in Figures 8(b) and 8(d), the revised manuscript now includes explanations concerning coherence levels, temporal continuity of time series, and spatial consistency to help distinguish potential deformation signals from noise. The selection of time-series feature points P1 and P2 was based on their relatively high coherence, stable temporal behavior, and representative spatial locations within the potential landslide body. This rationale has been added to the main text. Furthermore, following your advice, the original Figures 2 and 3 have been moved to the appendix.

Comment 6: The analysis of the research results in the manuscript does not align well with the data, which may confuse readers. For example, the text states: "Furthermore, the deformation trends of points 1 and 2 were not synchronized with reservoir water level changes during the two water impoundment cycles, indicating a time-lag effect of landslide deformation in response to water level fluctuations." The authors are advised to use signal processing methods to quantify this time-lag effect.

Response: Thank you for highlighting the issue of potential misalignment between analysis and data. We have addressed this by implementing the suggested signal processing approach. Specifically, we have employed cross-correlation analysis to quantify the temporal relationship between landslide deformation and reservoir water level changes. The revisions include: (1) quantifying the lag time (e.g., -40 time steps for H28 P1) through cross-correlation analysis; (2) explicitly explaining in the text the physical meaning of a "negative lag" (indicating deformation leading water level change); and (3) incorporating discussion on statistical significance with reference to significance thresholds in the cross-correlation plots. These modifications provide clear, data-supported evidence for the conclusion that "landslide deformation exhibits a time-lag effect in response to water level changes," thereby eliminating potential reader confusion.

Response to Reviewer 4

Comment 1: In abstract. I suggested that don’t highlight your main conclusion via compare the variation of number. A valuable conclusion should be focus on what we know about the new thing or interesting thing. For example, (2) 140,146 additional valid phase-unwrapping points were obtained, indicating substantially improved interferometric processing quality. The reader may don’t care what exactly number of valid phase-unwrapping point have been found, but care about what exactly it means. Is that means that the new method can found more landslides in vegetation covered areas?

Response: Thank you for the reviewer’s valuable suggestion. We have revised the Results statements in the abstract to avoid emphasizing our main conclusions through numerical comparisons. Instead, we highlight the key new capability enabled by our method: by explicitly incorporating vegetation-cover variations to adaptively adjust the coherence threshold, the proposed approach improves phase-unwrapping reliability and the spatial continuity of the deformation field under low-coherence conditions during high-vegetation seasons, thereby enhancing landslide detectability in vegetated, deeply incised canyon environments. Quantitative metrics (e.g., the increase in quality-controlled unwrapped pixels and the reduction in deformation noise) are retained only as supporting evidence to objectively document the magnitude of improvement. The abstract has been rewritten accordingly, and the corresponding changes have been marked in the revised manuscript.

Comment 2: Introduction. Comment on the first paragraph: short the first paragraph, please just focus on the big research background introduce in the first paragraph.

Response: Thank you for the reviewer's valuable suggestions. We understand your recommendation for a more focused opening in the Introduction section. Accordingly, in the revised manuscript, we have adjusted the structure: the first paragraph now only briefly outlines the research background of reservoir landslides, the representativeness of the study area, and the necessity of early identification. The technical details originally included in the first paragraph—such as interferometric pair selection and baseline network construction—have been moved to a separate later paragraph, allowing for a clearer and more organized review of existing methods and their limitations. These adjustments have improved the overall logic and flow of the Introduction. We appreciate your thoughtful comments.

Comment 3: Figure 1. It should be Legend but not Legebd. By the way, the map of China in the top-right should be (a). In the bottom-left, Why only Yunnan has been highlight using pink color?

Response: Thank you for pointing out the cartographic issues in Figure 1. We have revised the figure accordingly to improve its consistency and readability: (1) the misspelling "Legebd" in the legend has been corrected to "Legend"; (2) following common cartographic conventions, the China locator map in the upper-right corner has been designated as panel (a), and the remaining study-area and thematic maps have been labeled sequentially as (b), (c), etc.; and (3) regarding the previous use of pink shading to highlight only Yunnan Province in the lower-left map, this was originally intended to provide a quick administrative reference for the study area, but we acknowledge that it could be misleading by overemphasizing Yunnan and obscuring the broader context of the Sichuan–Yunnan border reservoir region. To avoid ambiguity, we have removed the single-province highlighting and replaced it with a neutral basemap, while delineating the Baihetan Reservoir study area using consistent symbols/boundaries (with provincial boundaries shown in a uniform style where necessary) and clarifying these elements in the legend. These revisions have been incorporated into the revised Figure 1 and its caption.

Comment 4: Figure 7. I keep my query about the polygons in Figure 7. I think some of the polygons are not the boundaries of landslides.

Response: Thank you very much for your careful observation. In Figure 7, the red polygons denote inventory-based landslide boundaries, which were manually delineated by interpreting geomorphological evidence from high-resolution optical imagery and the DEM (e.g., head scarps/tension cracks, lateral margins, and depositional features). These polygons therefore represent the geomorphic extent of mapped landslides rather than boundaries derived directly from the InSAR LOS velocity field using a deformation threshold. Because InSAR measures only the line-of-sight (LOS) component and is constrained by coherence, landslides may show spatially heterogeneous deformation, local decorrelation, layover/shadow effects, or motion directions partly insensitive to the LOS. Consequently, the observable deforming pixels may not fully cover the entire landslide body and may not coincide exactly with the mapped geomorphic boundary, which can create the impression that some polygons do not match the deformation pattern. To address this potential ambiguity, we have carefully re-checked the polygons against the optical imagery and DEM and revised the caption to explicitly state that the polygons are inventory-based landslide boundaries, thereby making the relationship between landslide extent and InSAR deformation information clearer.

Comment 5: Again, Figure 8. Especially, the polygon has showing in the Figure 8a. I believe that it's just a deep valley but not a landslide. Because I did not see the typical landslide scarp in this figure.

Response: Thank you for your valuable feedback. We understand your concern that the "typical arcuate rear scarp" may appear less distinct in the single nadir optical image of Figure 8a, given the target's location on a steep, deeply incised valley slope affected by shadows, illumination, and surface erosion. To address this, we have re-evaluated the polygon by integrating multi-temporal high-resolution optical imagery and DEM-derived data (e.g., slope, hillshade). Although the rear scarp is subtle in the original context, the area still exhibits a consistent set of landslide geomorphic evidence—including an upper slope break, relatively clear lateral boundaries, slope texture disturbance, and lower accumulation/frontal features—supporting its identification as a landslide. To improve clarity, we have optimized Figure 8a by using a clearer base map (with optional supplemental hillshade/slope maps), enhancing annotations of key features, and slightly adjusting the polygon boundary, ensuring the landslide characteristics are more intuitively presented in the revised figure.

We once again sincerely thank the Editor and Reviewers for their valuable time and insightful comments, which have greatly strengthened the scientific quality, clarity, and overall presentation of this work. All comments have been carefully considered and addressed through substantial revisions to the manuscript. We believe that the revised version has comprehensively responded to the concerns raised and has been significantly improved in terms of rigor, coherence, and methodological transparency. We respectfully submit the revised manuscript for your further consideration and look forward to your evaluation.

Sincerely,

Wenfei Xi (on behalf of all authors)
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