Response to Reviewers

We would like to express our sincere gratitude to the Editors and the anonymous reviewers for their valuable time, constructive feedback, and thoughtful guidance on our manuscript. Below, we provide a point-by-point response. Each reviewer's comment is reproduced in full, followed by our detailed response.

RC1:

Comment 1: C-values. Although the calculation of the C-values is well known by the community and it is referenced in the manuscript, from my point of view, a real example of the calculation for a case study could clarify many aspects of the scientific discussion. For instance, the authors gave a good example of STL calculation in Figure 1.

Reply: We sincerely thank the reviewer for this constructive suggestion. As the reviewer pointed out, while the C-value calculation method is well known and referenced in our manuscript, providing a concrete example can indeed enhance clarity for the reader. To address this, we have now added Figure 7, which presents representative linear fitting results between the logarithm of the electric field spectrum and the logarithm of the frequency at four randomly selected observation points within one orbital cycle. The regression equations and determination coefficients (R^2) are displayed in each panel. This example illustrates the actual fitting procedure from which the parameters a, b, and R are derived, followed by the calculation of the C-values according to Eqs. (5) and (6).

Comment 2: How can we be sure whether the high C-value is associated with the earthquake? It is important to observe that the C-value peaks are almost coincident with magnetic disturbances (Figure 6). Another question, As shown by the authors, the STL method is very efficient to identify lightning activity. Thus, how to separate those influences when we are looking for possible earthquake precursors?

Reply: We sincerely thank the reviewer for this valuable comment. In our analysis, the C-value peaks were observed on April 21 and April 29, whereas the major geomagnetic disturbance shown in Figure 6 occurred around April 24, with a time gap of three to five days. According to our previous studies, the impact of geomagnetic disturbances on the C-value is relatively minor and generally limited to short-term effects on the day of the storm itself. Therefore, we consider it unlikely that the C-value peaks identified in this study were driven by geomagnetic activity. Instead, they are more plausibly related to ionospheric disturbances associated with the earthquake preparation process.

Indeed, as shown in our results, the STL method is also effective in capturing variations related to lightning activity. In our analysis, these effects mainly manifest in the trend

component, where they exhibit relatively strong regularity and spatial recurrence, making them more easily identified and separated by STL decomposition. Therefore, when investigating possible earthquake precursors, we focus on the residual component after removing the dominant seasonal and trend signals. The residuals are less affected by such recurrent lightning-related disturbances, and thus provide a more suitable basis for detecting unconventional anomalous variations potentially associated with seismic activity.

Minor Comment 1: How many days did the author use in Figure 4 to compare to the lightning activity in May 2023 (Figure 5)? Was there more available data?

Reply: We appreciate the reviewer's question. In Figure 4, we used the full dataset from May 2023 to make a direct comparison with the lightning activity dataset shown in Figure 5, which also corresponds to May 2023. Thus, both figures are based on the same one-month period, ensuring consistency between the satellite observations and the lightning climatology. While additional data outside May were available, our aim was to provide a one-to-one comparison within the same time window.

Minor Comment 2: Line 130: => a0=log a

Reply: Revised according to the reviewer's comments.

Minor Comment 3: Line 220; Why 7 days before and one day after, please, explain the reason, maybe saying that the average C-value is going to be showing for a long period ahead?

Reply: We thank the reviewer for this comment. To provide a more comprehensive view of the anomalous disturbances associated with the earthquake, we selected a period of 7 days before and 1 day after the event to construct a nine-panel figure as an example, illustrating the spatial evolution of anomalies before and after the earthquake, while maintaining clarity and readability of the figures. The longer-term evolution of the C-value anomalies is already presented in Figure 8, where the time series over a broader period is shown.

Minor Comment 4: Figure 8 and others: Show a vertical line indicating the exact time of the earthquake as shown in Figure 6. It can help the reader to localize the event easily.

Reply: A vertical line indicating the exact time of the earthquake has been added in the corresponding figures.

RC2:

Comment 1: “in the Earth’s atmospheric structure” – the ionosphere is part of the upper atmosphere, not “in”.

Reply: We thank the reviewer for the correction. We have revised “in the Earth’s atmospheric structure” to “as part of the upper atmosphere.”

Comment 2: “storms, are substantial”, remove the comma

Reply: We thank the reviewer for the suggestion. We have removed the unnecessary comma at this point.

Comment 3: “…making them highly sensitive precursor indicators for earthquake prediction.” - overstatement, predictive skill is demonstrated? - making them potential precursors indicators

Reply: We accept the reviewer’s suggestion and have revised “highly sensitive precursor indicators” to “potential precursor indicators.”

Comment 4: “Since the 1960s, the phenomenon of pre-seismic ionospheric disturbances has garnered extensive attention…” – Authors should add reference to key reviews to strengthen context

Reply: We have added relevant review references to strengthen the background.

Comment 5: “Using data from the Sugadaira Space Radio Observatory…”- This long paragraph lists many examples without synthesis; Authors should summarize and highlight what knowledge gap remains.

Reply: We thank the reviewer for the reminder. We have added a summary of the limitations of existing studies and highlighted the knowledge gap addressed in this work.

Comment 6: “Beijing Standard Time ()” – Authors should indicate UT time as well; satellite data is global and time-zone-dependent.

Reply: We have provided both Beijing Standard Time (BST) and Universal Time (UT) at this point, and revised the times in the subsequent figures to Universal Time.

Comment 7: “In this study, electric field data in the ELF band observed by the CSES-01 satellite were utilized.” - Authors should add more method detail, for example, exact frequency range considered for ELF in the analysis.

Reply: We have added a description of the frequency range considered for the ELF data analysis.

Comment 8: “The onboard Electric Field Detector (EFD) can observe ULF, ELF, VLF,…”-

sampling rate, resolution, how the PSD was computed? FFT parameters, window size? Authors should add more method details.

Reply: We have added details on the sampling rate, frequency bands, and related parameters of the EFD observations in the ELF range.

Comment 9: "STL (Seasonal and Trend Decomposition using Loess) is a versatile and robust method for time .. " – Authors should add a more recent application (reference) in geophysical data satellite.

Reply: We have added recent references on the application of STL in satellite-based geophysical data analysis.

Comment 10: "The design of the STL decomposition is straightforward…"- straightforward? Authors should use objective description, allows control of…

Reply: We have revised the wording by replacing "straightforward" with a more objective description: "The design of the STL decomposition allows flexible control of the seasonal and trend components."

Comment 11: Authors should explain the physical meaning of the Fs (seasonal intensity) dataset, and improve the interpretation.

Reply: We have added an explanation of the physical meaning of Fs and improved its interpretation in the manuscript.

Comment 12: Authors should add details about what the C-value threshold means (significance relative to quiet-time baseline).

Reply: We have added the relevant explanation as suggested.

Comment 13: "…defining an exponential relationship between the spectral data S_e and the frequency f" - Authors should explain the frequency range over which this fit was applied

Reply: We have explicitly specified the frequency range over which the exponential fitting was applied.

Comment 14: "This approach eliminates conventional perturbations," – eliminates – reduces

Reply: We have replaced the term "eliminates" with "reduces" as suggested.

Comment 15: "…allows the study of conventional.." - allows – enables

Reply: We have replaced the term "allows" with "enables" as suggested.

Comment 16: “the seasonal and trend components both exhibit distinct periodicity.” - Authors should discuss the origin of this periodicity (orbital cycle, geographic variation, etc).

Reply: We have added a discussion clarifying that the observed periodicity is related to the satellite’s orbital cycle and geographic variations.

Comment 17: Why 0.67 seasonal intensity indicate latitude effects specifically? (physical link)

Reply: We have explained at the beginning of Section 3.2 that the periodicity of the seasonal component originates from the latitudinal motion of the satellite’s orbit, and we have added related explanations when calculating the seasonal intensity to help readers better understand.

Comment 18: “are likely due to the ionospheric electric field being significantly influenced” – add references to previous studies supporting these mechanisms

Reply: We have added two references to support the conclusion that auroral currents in the polar region and equatorial electrojets can influence the ionospheric electric field.

Comment 19: Authors should interpret what a 0.20 trend strength means in terms of magnitude of variation.

Reply: We have added an explanation of the trend strength and have restructured the corresponding paragraph to improve clarity.

Comment 20: How high is the degree of overlap? Authors should consider statistical tests of overlap

Reply: We sincerely thank the reviewer for this insightful suggestion regarding statistical tests of overlap. We fully agree that introducing quantitative statistical analyses would strengthen the robustness of the conclusions. However, in this study, the trend component peak distribution map (Fig. 4) is based on the entire satellite orbit (from −70° to 70°) containing the peak, without further subdivision. Because each orbit covers a wide range, this construction is not fully suitable for precise spatial quantification of overlap. Therefore, our current analysis mainly relies on qualitative comparison to demonstrate the feasibility of the STL decomposition method. Nevertheless, we fully recognize the importance of statistical tests and plan to incorporate quantitative overlap metrics in future work, especially when extending to multiple earthquake cases with larger samples.

Comment 21: “…could be attributed to high-density lightning.” – “may be influenced

by", instead of "could"

Reply: We thank the reviewer for this careful wording suggestion. We have revised the phrase to "may be influenced by high-density lightning" to more appropriately reflect the uncertainty of the statement.

Comment 22: "return to a stable electric field state" – "return to baseline levels"

Reply: We appreciate the reviewer's valuable comment. We have revised the expression to "return to baseline levels," which we agree is a clearer and more precise wording.

Comment 23: "normal levels" - baseline levels

Reply: We thank the reviewer for pointing this out. We have revised the expression to "baseline levels" at the relevant occurrence for improved accuracy.

Comment 24: "renders" – "can make.. more apparent" , unless demonstrated across multiple events.

Reply: We sincerely thank the reviewer for this suggestion. We have revised the sentence to "can make··· more apparent," which better reflects the conditional nature of the statement, particularly in the absence of validation across multiple events.

Comment 25: "This exclusion also enables unconventional anomalous disturbances to emerge more prominently over a longer time scale." – Authors should specify the time scale, and whether this is supported by quantitative information in anomaly detection rates.

Reply: We thank the reviewer for this constructive comment. In the revised manuscript, we have specified the time scale as several weeks and provided supporting evidence by noting that multiple anomalies were detected within the 20 days preceding the Mw 7.6 Tonga Islands earthquake. At the same time, we fully acknowledge the reviewer's valuable suggestion that more rigorous quantitative evaluations, such as statistical assessments of anomaly detection rates, would further strengthen the methodology. As the present study mainly focuses on methodological exploration through a case demonstration, such analyses have not yet been incorporated. In our future work, when extending the analysis to multiple earthquake cases, we plan to implement systematic statistical evaluations as recommended.

Comment 26: Authors should consider add some metrics to describe the V-shaped and bimodal, for example, slope, peak-to-trough difference

Reply: We appreciate the reviewer's valuable comment on incorporating quantitative metrics such as slope and peak-to-trough difference to better characterize the V-

shaped and bimodal features. This is indeed an excellent suggestion that would make the description more comprehensive and rigorous. In the current study we have focused on illustrating the overall feasibility of the method, but in our future work, we plan to introduce such metrics in order to provide a clearer and more systematic evaluation of these features.

Comment 27: "auroral currents and the magnetic field substantially influence the ionospheric electric field." – key reference(s)

Reply: We thank the reviewer for the suggestion. We have added the relevant references to support this statement in the revised manuscript.

Comment 28: "Profoundly influences the ionospheric F-layer" – "can influence"

Reply: We have made the corresponding revisions according to your valuable suggestion.

Comment 29: Authors should consider the number of anomalies detected vs false alarms in a control period without earthquakes

Reply: We are grateful to the reviewer for highlighting the importance of evaluating the number of detected anomalies versus false alarms in control periods without earthquakes. We agree that this is a critical aspect for assessing the practical reliability of the method. Although our present study emphasizes a single case to demonstrate feasibility, we plan to include such comparative evaluations in our future work, particularly when extending the analysis to multiple events and longer control periods, to better understand the trade-off between sensitivity and specificity.

Comment 30: Authors should discuss the capability of STL decomposition algorithm against other decomposition methods or over multiple events (perhaps based on literature).

Reply: In the revised manuscript, we have added a discussion comparing the STL decomposition method with other commonly used decomposition approaches, in order to highlight the advantages of STL in this study.

Comment 31: Authors should add some limitations, perhaps some non-seismic disturbances remain in residuals

Reply: We have added an explanation in the Discussion section noting that some non-seismic disturbances may still remain in the residual component, which represents an important direction for further research.

Comment 32: Authors should detail about the STL parameter settings and on the

spatial interpolation/resolution used for mapping the anomalies

Reply: We appreciate the reviewer's suggestion. The STL parameter settings (seasonal = 7, period = 5200) have been explicitly included in the revised manuscript (Section 2.2.1). Regarding the spatial interpolation, no additional interpolation was applied in this study; the anomalies were mapped directly at the geographic coordinates of the satellite measurements.

Comment 33: Some redundant sentences and occasional grammatical error, authors should revise carefully the manuscript to correct this

Reply: Thank you for the reminder. We will carefully revise the language throughout the manuscript.

Comment 34: Detail in the figures the variables, color scales, units.

Reply: We thank the reviewer for this helpful comment. We will revise the figures to include detailed information on the variables, color scales, and units to improve clarity and readability.

Comment 35: Conclusions are based on one event, no clear control period analysis, authors should discuss this.

Reply: We appreciate the reviewer's observation. We acknowledge that the present study is based on a single major earthquake event (Mw 7.6, Tonga, May 2023) and that a systematic control-period analysis was not performed. Our intention here was to illustrate the feasibility of the STL-based approach and the C-value method for detecting ionospheric perturbations possibly associated with earthquakes. While the absence of an explicit control period is a limitation, we attempted to mitigate this by (i) removing known conventional periodic disturbances (trend and seasonal components), and (ii) presenting residual anomalies over an extended time window to reduce the likelihood that they represent random variations.

In future work, we plan to apply this method to multiple seismic cases and include quiet-time control periods in order to statistically assess the robustness of the observed anomalies.