Dear Editor, Dr. Christos Evangelidis

I would like to thank you and the Reviewers for their constructive comments and suggestions, which have improved the clarity and quality of the manuscript. I have addressed all the comments in detail, answering point by point below. The main changes made to the manuscript can be summarized as follows:

- **The manuscript title was revised:** following the reviewer B suggestion, I changed the title to "Multi-hazard monitoring of storm surges, rainfall, wind, river floods, and human activity using seismic noise: the case of the 17 January 2025 southern Italy storm".
- **Improved readability and structure**: I revised the paragraph structure throughout the manuscript. In particular, the Methods (Section 3.4) is now divided into five subsections similar to the Results structure, and the discussion has been shortened, based on Supplementary Material, to reduce the need for readers to rely on it in detail.
- **Clarified the scope of the evidence supporting key interpretations**: I made explicit that the quantitative validation of the river-flood interpretation (1–5 Hz band) relies on a single station–gauge pair (ECTS) and revised the Abstract, Section 4.2.2, and Conclusions accordingly to frame this as a well-supported, site-specific case study rather than a network-wide result.
- **Strengthened methodological transparency**: I clarified the definition of the SNR method used in this work and its aggregation across stations, made the terminology for river-related seismic signals more precise and consistent, and corrected several text and figure-caption errors, including errors in **Figure 1**.
- **Provided a more balanced discussion of uncertainty and limitations**: I added a discussion of potential spectral overlap between simultaneously active environmental processes, moderated causal wording regarding anthropogenic noise where direct evidence was limited, and expanded the Conclusions to explicitly state the limitations related to the single-event case study, local site effects, and network density.

The detailed one-by-one response to the comments is included below (the review itself is in black, and the responses are in blue). Please note that the line numbers refer to the manuscript with the tracked revisions.

Reviewer B:

I have completed my review of the manuscript titled “From bad-weather noise to road silence: analyzing meteorological phenomena and the human reaction using seismic noise” by Alfio Marco Borzì.

The manuscript addresses a timely and potentially valuable topic by exploring how seismic noise can be used to investigate multiple meteorological and anthropogenic processes during a severe weather event. The study is ambitious in scope and is based on a rich multi-sensor dataset, which gives it clear potential interest for readers on topic. At the same time, I think

the manuscript would benefit from revision in several areas in order to strengthen the clarity of the analytical workflow, better define the scope of some interpretations and improve the overall presentation of the results.

A broader point that may be worth considering is that, although the study is introduced as being based on a large multi-sensor dataset, some of the central interpretations appear to be supported by more limited subsets of the available data. Making this distinction clearer would help the reader better assess the strength and scope of the conclusions.

I hope that the comments and suggestions below will help the author further clarify and strengthen what I see as a promising contribution.

General Comments & Suggestions

1. The manuscript would benefit from a more careful paragraph structuring in several places. Some sentences or figure references appear as very short stand-alone paragraphs, whereas other parts are presented in overly dense paragraphs. I suggest revising the paragraphing throughout the manuscript to improve readability and the overall flow of the text.

1. I thank the reviewer for this suggestion that helped to improve the readability of the work. I have revised the paragraph structure throughout the manuscript, merging short sentences with adjacent paragraphs and splitting long ones. For example, in Section 4 a single-sentence reference to **Figure 1** was merged with the previous one in the paragraph, while in Section 4.2.2 the paragraph discussing the river-flood polarization results was split into two sentences, separating the two distinct azimuth/rectilinearity behaviours observed during the storm. Similar adjustments have been applied throughout the manuscript.

2. The manuscript appears to rely quite heavily, at several places, on detailed discussion and description of figures that are only available in the Supplementary Material (e.g., lines 322–325, 367–370 and 523–524, among others). In some cases, the way supplementary figures are cited in consecutive sentences is also somewhat confusing for the reader (e.g., lines 523–524 among others). I suggest either moving the most important supplementary figures into the main text, if the author considers them crucial for the interpretation, or shortening the corresponding discussion so that the reader is not required to consult the Supplementary Material in such detail in order to follow an argument.

2. I thank the reviewer for this observation. I have shortened the discussion of results relying on Supplementary Material throughout the manuscript, not only in Section 4.2 but also in Section 4.1 (e.g., in the description of Supplementary Figure 4 and of Supplementary Table 4/Figure 5), removing repeated citations of the same supplementary material within consecutive sentences (e.g., in the description of Supplementary Figures 7, 8, 10 and 11) and condensing the corresponding text, so

that the main argument can be followed without consulting the Supplementary Material in detail.

3. The title of the manuscript is engaging, but I believe that it could be made more precise and scientifically neutral. In particular, phrases such as “bad-weather noise”, “road silence” and “human reaction” read as somewhat metaphorical relative to what is directly demonstrated in the manuscript. I would encourage the author to consider an alternative title.

3. Following your suggestion, I modified the title to “**Multi-hazard monitoring of storm surges, rainfall, wind, river floods, and human activity using seismic noise: the case of the 17 January 2025 southern Italy storm**”.

Main Comments & Suggestions

1. Section 4.2.2 (Relationship between seismic signals and river floods) is interesting and potentially valuable, but the manuscript should be more explicit about the scope of the supporting evidence. The manuscript states that 14 river-proximal stations were selected, yet the detailed quantitative validation of the river-flood interpretation is clearly presented only for ECTS. It is not sufficiently clear what analysis, if any, was carried out systematically across the full 14-station subset. Related to this, the role of the supplementary figures in this section would benefit from clearer explanation, including which station(s) are represented in Supplementary Figures 7–10.
   More specifically, my reading of the Methods section (lines 244–252) is that, although 14 seismic stations were selected near rivers, only one station (ECTS) is located near both a river and a river gauge. The subsequent detailed analyses, including the polarization analysis (lines 257–265), also appear to be based on ECTS. If this interpretation is correct, then the strongest validation of the 1–5 Hz river-flood interpretation relies on a single station–gauge pair. In that case, I would suggest framing this result more clearly as a well-supported site-specific case study. My concern is not that the interpretation is necessarily incorrect, but that the current wording may over-generalize from limited validation. For example, the statement in the Abstract that “River-flood signals showed strong correlations with water-level variations in the 1–5 Hz band” and the corresponding wording in the Conclusions should be checked carefully to ensure that they do not imply broader validation than is directly demonstrated.

1. I thank the reviewer for this observation. I agree that the quantitative validation of the 1-5 Hz river-generated noise relies on a single station-gauge pair (ECTS), the only station on the considered network co-located with both a river and a river gauge. For this reason, I revised some parts of the manuscript to frame this result more clearly as a well-supported, site-specific case study.

In particular, in the **Methods** section, we now clarify that of the 14 river-proximal stations, only ECTS is paired with a river gauge, while the remaining stations were examined through a qualitative comparison of the RMS amplitude time series in the 1–5 Hz and 25–35 Hz bands, without direct water-level validation (Lines: 298-302). The same clarification has been added at the opening of **Section 4.2.2**, stating explicitly that the detailed analyses are based only on ECTS (Lines: 603-611). The **Abstract** (Lines 24-29) and **Conclusion** (Lines 859-873) have been revised accordingly to these sections. Now I have specified that the strong correlation with water level in the 1-5 Hz band is attributed only to the ECTS station. The frequency band separation examined at the other stations is described only as "similarly examined", without implying broader quantitative validation.
I have also clarified in the figure captions that Supplementary Figures 7, 8, and 9 refer to seismic station ECTS, while Supplementary Figure 10 refers to EPOZ, ESAL and OPPI.

2. The anthropogenic-noise analysis is potentially interesting and the comparison between 17 January and the median behaviour of other Fridays provides a reasonable basis for interpretation. However, the manuscript goes beyond describing a reduction in 5–25 Hz amplitudes and interprets this decrease as reflecting civil-protection measures and human response to the alert conditions. In my view, this causal wording is somewhat stronger than the evidence currently demonstrated. I therefore suggest either moderating the wording so that the result is presented as being consistent with reduced anthropogenic activity during alert conditions or providing additional support for the stronger causal interpretation (e.g., mobility data if they are available).

   A related point that would benefit from clarification is the interpretation of the 5–25 Hz band itself. In the Methods (lines 266–268), this frequency range is selected in order to minimize contamination from other sources, including river noise, rain and wind. However, the manuscript later appears to invoke meteorological forcing, previously associated mainly with the 25–35 Hz range, to explain amplitude changes during part of the same time series. This does not necessarily invalidate the interpretation, but it means that the anthropogenic interpretation of the morning decrease requires clearer justification. In particular, the manuscript should clarify why the lower amplitudes observed during the morning hours are best interpreted as reduced anthropogenic activity, rather than reflecting the spatiotemporal evolution of the storm and the later arrival of meteorological forcing at the station site.

2. I thank the reviewer for this constructive comment. Unfortunately, independent mobility or traffic datasets were not available for the study area during the analyzed period. To fully address the reviewer's concern without overinterpreting our data, I revised the manuscript as follows:
   - I revised the manuscript text, abstract (Lines: 30-32), and conclusions (Lines: 875-881) to soften the findings. Rather than asserting a direct cause-and-effect relationship, I now discuss the morning reduction in 5–25 Hz seismic noise as

being *interpretable* as a probable decrease in anthropogenic activity and traffic following civil protection alerts and municipal restrictions (e.g., school and office closures).

- I have expanded Section 4.2 (Lines: 468-491) and Section 4.2.3 (Lines: 803-819) to clarify the spatiotemporal evolution of both anthropogenic and meteorological signatures in the 5-25 Hz band: during the morning hours (until ~14:00 UTC), the main storm front and severe downburst winds had not yet reached the area (meteorological forcing was minimal). Therefore, the lower amplitudes in this band compared to the multi-Friday median can be explained as a probable reduction in human activity rather than early storm forcing. During the climax of the storm in the afternoon, strong winds and heavy rainfall significantly affect the entire high-frequency spectrum (partially overlapping into the 5–25 Hz band).

3. I think the manuscript would benefit from a clearer presentation of the seismic data analysis workflow in Section 3.4 (lines 184–281). Several distinct analyses are grouped together within a single methodological section, including PSD/spectrogram analysis, RMS-based analysis, SNR-based analysis, river-stage correlation, polarization analysis, anthropogenic noise comparison and the corresponding frequency band selections. While all of these steps of the analysis are relevant, their presentation makes it difficult for the reader to follow, step by step, how the different analyses relate to one another. I therefore suggest restructuring this part of the Methods, possibly into clearer subsections that would improve readability.

3. I thank the reviewer for this suggestion. I have modified Section 3.4 by adding five subsections (3.4.1 Spectral analysis and data quality control, lines: 221-249; 3.4.2 RMS amplitude analysis and frequency-band framework lines: 250-263; 3.4.3 Frequency-band identification for rain- and wind-induced noise, lines: 264-292; 3.4.4 River-flood analysis: correlation and polarization, lines: 293-321; 3.4.5 Anthropogenic noise analysis, lines 322-338), similarly to the structure of the Results section. In this way, each analysis and its corresponding results can be more easily followed throughout the manuscript.

4. The author states that the SNR analysis was performed across all stations used in the study and clearly explains which parts of the time series were treated as “signal” and “noise,” which is helpful. However, the exact definition of the SNR metric at the station level and the way the station-wise results were combined into the final network-wide result, are not sufficiently transparent. I therefore encourage the author to clarify this more explicitly.

4. I thank the reviewer for this suggestion that helped to improve the clarity of the SNR method used in this study. In particular, I first normalized the RMS amplitude values

to allow a comparison across the stations. Station-SNR was then computed as the ratio between the normalized RMS amplitude during the signal window and during the noise window. The network-SNR reported in the text is the median of the station-SNR across all the stations. This clarification has been added to **Section 3.4.3** (Lines: 277-280).

Other Comments & Suggestions

1. I find Figure 1, presenting the main map of the manuscript, somewhat difficult to interpret in its current form. In particular, it is not immediately clear whether the labels shown on the legend refer to station codes or network identifiers, which makes the figure harder to read at first glance. In addition, there seems to be some inconsistency between the legend, the caption and the features displayed on the map. I suggest revising the figure so that the main map is visually cleaner and of higher quality maybe.

1. I thank the reviewer for this observation. However, **Figure 1** is intended to provide an overview of the study area and of the extensive dataset used, showing the general spatial distribution of seismic, meteorological, and river-gauge stations. The position of specific stations discussed in detail in the manuscript, such as those near the Alcantara River, is addressed individually in the relevant paragraphs and supplementary figures (e.g., Supplementary Figure 7 for the ECTS station and river gauge).
   As noted by the reviewer, the **Figure 1** caption contains some errors regarding the colour scheme since it refers to an earlier version of the figure and does not match the correct colours used in this new figure. I have corrected it according to this new scheme and I added a sentence clarifying that the coloured triangles indicate the specific seismic stations discussed individually in the text.

2. The terminology used for river-related seismic signals could be made more precise and consistent. Expressions such as “river noise” or “river noise frequency band” sound somewhat informal in places. Consider using more explicit wording, such as “river-generated seismic noise,” “river-induced seismic noise,” or “frequency band associated with river-generated seismic signals,” and applying the chosen terminology consistently throughout the manuscript.

2. Fixed. I replaced “river noise” with “river-induced seismic noise” and “river noise frequency band” with “frequency band associated with river-generated seismic signals”.

3. Line 104: There appears to be a duplicated numbering item (“ii) ii)”).

3. Fixed.

4. Line 170: The dataset MEDSEA_HINDCAST_WAV_006_012 is introduced using a raw product-code name, which reads somewhat awkwardly in the main text. Consider using a more descriptive wording and keeping the exact product identifier in parentheses or adding it in the Data and Code Availability section.

4. Fixed. I use the word “hindcast” in the main text (To obtain information about the sea state, the **hindcast** dataset provided by the Copernicus Marine Environment Monitoring Service (CMEMS) was used) and move the product name in the “Data and Code Availability” section.

5. Lines 185–192: The choice of 81.92 s windows is relevant in terms of reproducibility, but the explanation is more detailed than necessary. Consider shortening this and keeping only a concise justification. For example: Welch’s method was applied using non-overlapping 81.92 s windows (8192 samples at 100 Hz), chosen to provide a power-of-two sample length for the FFT.

5. Fixed. I removed the separate explanatory sentence and integrated a concise justification into the preceding sentence.

6. Line 193: “trough” should be corrected to “through.”

6. Fixed.

7. Lines 219–220: This sentence appears somewhat repetitive.

7. I deleted this sentence, it appeared to be a refusal.

8. Lines 234–238: This part of the text appears to describe a figure, but no explicit figure reference is provided. Please clarify this.

8. Fixed, I refer to **Figure 6**, now cited in the text.

9. Line 252: There appears to be a sentence fragment (“band.”).

9. Deleted.

10. Lines 293–298: Please define the abbreviation SM, SPSM, and PM when they first appear.

10. I chose to define only the acronym for the Secondary Microseism (SM), as it appears several times throughout the text. Short Period Secondary Microseism (~~SPSM~~) and Primary Microseism (~~PM~~) were instead cited without acronyms, since they are mentioned only once.

11. Lines 299–302 and 514–515: The text here is more descriptive than necessary, mainly repeating basic information already visible in the spectrograms. A shorter reference to the figures would be sufficient. Please also consider whether similar cases elsewhere in the manuscript, where detailed technical figure description appears in the main text rather than in captions.

11. I thank the reviewer for this comment. I shortened the two sentences mentioned in this point and modified four similar cases (description of Figures 2, 3, 7, 8 and Supplementary Figure 9). In all cases, I removed the redundant axis/color-scale description from the main text and moved it to the corresponding figure caption.

12. Lines 560–562: Please check the date. In the context of the event discussed in the manuscript, “17/02/2025” may need to be “17/01/2025.”

12. I have standardized this date to match the format used for all other dates in the text and changed “01” (“02” in the text, for error) to “January”.

13. Lines 634–635: The manuscript refers to Supplementary Figure 7 for the hysteresis result, whereas the relevant cross-plot appears in Supplementary Figure 9.

13. Fixed in "**Supplementary Figure 9**”.

14. Lines 742–743: Please remove the extra punctuation (“1–5 Hz.,”).

14. Fixed.

15. Lines 776–777: Please revise “river-generates seismic signals” into “river-generated seismic signals.”

15. Fixed

Reviewer D:

This is an interesting and well-written manuscript that explores the use of seismic noise for monitoring multiple environmental processes during an extreme weather event. The study is comprehensive and has the potential to make a valuable contribution. However, I have several concerns regarding the interpretation of the results, the robustness of the proposed framework, and the generalizability of the conclusions. I encourage the authors to address the following comments.

1. The manuscript attributes specific frequency bands to different environmental processes (e.g., 0.1–0.2 Hz for sea state, 1–5 Hz for river floods, 25–35 Hz for rainfall and wind, and 5–25 Hz for anthropogenic activity). However, these assignments appear to rely primarily on temporal correlations and SNR analyses. Since multiple environmental processes evolved simultaneously during the storm, it remains unclear how confidently the observed seismic signals can be attributed to individual sources. In particular, potential spectral overlap among different environmental processes has not been sufficiently addressed. The authors should provide stronger physical evidence supporting these frequency assignments or explicitly discuss the uncertainty and potential source interference associated with the proposed frequency-band classification.

1. I thank the reviewer for this important point. I agree that during a multi-hazard event, concurrently active processes and consequently spectral overlap are expected. I added a paragraph in Section 4.2 (Lines: 468-491) in which I discussed this aspect. In particular, I clarify that the identified bands represent a range of maximum sensitivity for each process and summarize, band by band, the evidence available beyond the temporal correlation, such as:
    - the well-established microseism theory and spectral separation of the sea-state band;
    - the multiple independent lines of evidence (spectral shape, hysteresis, and polarization) supporting the river-flood attribution;
    - the non-separability of wind and rain within the 25-35 Hz band;
    - the partial overlap between the anthropogenic-noise band and meteorological forcing, for which the most robust evidence instead comes from the pre-rainfall period and the independent multi-Friday, multi-alert-level comparison.

2. The proposed framework is evaluated using a single meteorological event in southern Italy. Although this case study is comprehensive, it remains unclear whether the identified frequency bands and observed relationships are transferable to other storm events, climatic regions, geological settings, or seismic networks. The Discussion would benefit from a more balanced assessment of the generalizability of the proposed framework and a clearer acknowledgement of its current limitations.

2. I thank the reviewer for this observation. I agree that this framework, based on a single case study, needed a more extensive discussion of its limitations. For this reason, I expanded the limitations paragraph in the Conclusion section (Lines: 899-917) to explicitly state that these findings are based on a single storm event and that application in other regions, with other networks and during other events, is an important direction for future work.

3. Many of the manuscript's interpretations are inferred from temporal agreement between seismic amplitudes and environmental observations. While these correlations are encouraging, they do not necessarily demonstrate the underlying physical mechanisms. Alternative explanations, including overlapping environmental sources, local site effects, or other unaccounted-for processes, may also contribute to the observed seismic variations. A more balanced discussion of these alternative interpretations, together with the associated uncertainties, would improve the robustness of the conclusions.
3. I thank the reviewer for this observation. As discussed above (Point 1), I summarized, band by band, the evidence supporting the band attributions beyond the temporal correlation in the paragraph added to Section 4.2 (Lines: 468-491). I now added a sentence to the same paragraph to explicitly state that temporal correlation alone cannot fully exclude the contribution of local site effects or other unaccounted noise sources coinciding with the storm.

4. Throughout the manuscript, rainfall and wind are treated as a combined source of seismic noise within the 25–35 Hz frequency band. However, these two environmental processes occurred almost simultaneously during the storm, making it difficult to distinguish their respective contributions to the observed seismic energy. It remains unclear whether the reported signals are primarily associated with rainfall impacts, wind-induced ground vibrations, or a combination of both. The authors should clarify this issue through additional analyses or explicitly acknowledge this limitation in the Discussion.
4. I thank the reviewer for this point. As already mentioned in the manuscript, the contribution of rain and wind cannot be distinguished as individual noise sources in the 25-35 Hz band. I revised section 4.2.1 (Lines: 500-503) to state this more explicitly: the RMS amplitude in this band reflects the combined effect of rainfall and wind, and cannot be quantitatively decomposed into their respective contributions. The observed temporal pattern allows only a qualitative distinction (an initial wind-dominated phase preceding rainfall onset, followed by a phase where both sources are active), and this is now clarified in the text.

5. Because the seismic stations are installed under diverse geological and environmental conditions, local site effects may substantially influence both the amplitude and spectral characteristics of the recorded seismic signals. Variations in soil properties, bedrock conditions, station installation, vegetation cover, topography, and proximity to environmental noise sources may all affect the observed frequency responses. Consequently, it remains unclear to what extent the identified frequency bands are

transferable across stations with different site characteristics. A more thorough discussion of local site effects and their implications for the robustness and transferability of the proposed framework would strengthen the manuscript.

5. I thank the reviewer for this important observation regarding the seismic network. I agree that local site effects may influence the spectral features of the recorded signal. The seismic network used in this study spans several site conditions, but the analyses performed consistently identified the same frequency bands as most sensitive to each phenomenon across this heterogeneous network, providing empirical evidence of robustness at the network scale. However, a systematic characterization of individual site conditions was not performed in the present study. I expanded the limitations paragraph to note that the network's site diversity and the observed robustness of the results support the frequency-band identification. However, a site-specific geological characterization of each station is a valuable direction for future work to further constrain the transferability of the proposed framework (Lines: 909-917).

Dear Editor Dr. Christos Evangelidis,

Thank you very much for your positive feedback and for the time you and the reviewers have dedicated to mine manuscript "**Multi-hazard monitoring of storm surges, rainfall, wind, river floods, and human activity using seismic noise: the case of the 17 January 2025 southern Italy storm**" (~~**From bad-weather noise to road silence: analyzing meteorological phenomena and the human reaction using seismic noise**~~).

I have now carefully addressed the remaining minor points raised in your message. The point-by-point responses are provided below.

Section 3.1 and/or Data and Code Availability

Please explicitly acknowledge and cite the seismic networks IV, MN, OT, and ML used in this study by including their corresponding network DOIs:

IV: Istituto Nazionale di Geofisica e Vulcanologia (INGV). (2005). *Rete Sismica Nazionale (RSN)* [Data set]. Istituto Nazionale di Geofisica e Vulcanologia (INGV). https://doi.org/10.13127/sd/x0fxnh7qfy

MN: MedNet Project Partner Institutions. (1990). *Mediterranean Very Broadband Seismographic Network (MedNet)* [Data set]. Istituto Nazionale di Geofisica e Vulcanologia (INGV). https://doi.org/10.13127/sd/fbbbtdtd6q

OT: University of Bari 'Aldo Moro'. (2013). *OTRIONS* [Data set]. International Federation of Digital Seismograph Networks. https://doi.org/10.7914/SN/OT

ML: University of Malta. (2014). *Malta Seismic Network* [Data set]. International Federation of Digital Seismograph Networks. https://doi.org/10.7914/SN/ML

Since data from these networks are distributed through the Italian EIDA node hosted at INGV, please also acknowledge the Italian EIDA node and add the following reference:

Danecek, P., Pintore, S., Mazza, S., Mandiello, A., Fares, M., Carluccio, I., Della Bina, E., Franceschi, D., Moretti, M., Lauciani, V., Quintiliani, M., & Michelini, A. (2021). The Italian Node of the European Integrated Data Archive. *Seismological Research Letters, 92*(3), 1726–1737. https://doi.org/10.1785/0220200409

These acknowledgements/citations should be included in Section 3.1 and/or the Data and Code Availability section, as appropriate.

In addition, network 1Y does not appear to be registered with the FDSN as a temporary network code (another one is registered concerning a temporary deployment in Greece and North Macedonia). Please clarify the origin and status of this network code and provide the appropriate network citation/DOI or other reference, if available.

I added the networks and EIDA citations both in Section 3.1 (Seismic Data) and in the Data and Code Availability statement.

Regarding the network code mentioned in the comments, I would like to clarify that the network used in this study is **IY**, operated by the **University of Calabria**, rather than **1Y**. The corresponding reference is now included as **Università della Calabria (1981)**.

IY: Università Della Calabria, Italy. (1981). *Rete Sismica Unical* [Dataset]. International Federation of Digital Seismograph Networks. https://doi.org/10.7914/SN/IY

Additional minor corrections:

L184: Do you mean 99%? Please check and correct if necessary.

Line 188: I confirm that the 99.9th percentile is the correct threshold used for filtering outliers. Therefore, no change was made.

L278–279 and L284–286: There appears to be unnecessary repetition between these passages. Please revise to avoid repeating the same information.

I deleted the sentence in Lines 278–279 and revised the sentence in Lines 284–286 (Lines: 288-289 in the tracked change file) to avoid repetition.

L347: Please briefly mention again here the hypotheses proposed earlier in the manuscript, so that the discussion/conclusion can be followed without requiring the reader to refer back to the previous section.

Lines 350-353: Thank you for this suggestion. I have briefly reposted the two hypotheses in this section before identifying hypothesis (i) as the most realistic explanation, so that the discussion can be followed without referring back to the previous section.

L349–351: The English is unclear/awkward. Please revise these sentences for clarity and readability.

Lines: 357-358: I have revised the sentence to improve its clarity and readability

L390: The paragraph/sentence starts with “However,” but there does not appear to be a preceding contrast that justifies its use. Please revise the transition or remove “However.”

Line 398: Fixex, I removed “However”

L488: Please add the missing full stop (“.”).

Line 496: Fixed.

L497: “Co-located with the river” appears to be incorrect. Do you mean co-located with the meteorological station? Please check and correct.

Lines 505-506: I rewrote the sentences to clarify this aspect.