

Dear Dr. Tran,

We would like to sincerely thank you for your time and consideration throughout the review process.

We would also like to express our gratitude to Reviewer 3 for carefully reviewing the revisions made to the manuscript following the first round of review and for recommending the manuscript for publication.

We would also like to thank Reviewer 1, Bernard Twaróg, for his contribution to the review process.

Regarding the comments raised during this second round of review, it appears that some of them may be based on the original version of the manuscript rather than on our responses to the previous comments and the corresponding revisions. Also, the comments seem to be very similar of the ones the reviewer made during the precedent phase of review. In addition, comments do not focus on changes and further analyses performed after the first round of revision.

Therefore, for the sake of clarity, we will report our responses to the minor comments below, together with the corresponding changes made in the revised version of the manuscript, which are highlighted in italics.

To clearly distinguish reviewer's comments, we are underlining them in italics.

The main weakness of the study remains the lack of clear separation between the different sources of the seismic signal. The authors assume that low frequencies reflect fluvial processes, whereas high frequencies are mainly associated with wind. However, the results shown in Figure 7 indicate a high correlation between wind speed and the seismic signal ($\rho \approx 0.7\text{--}0.8$) across nearly the entire analyzed frequency range of 1–40 Hz. This undermines the unambiguous attribution of low-frequency signals exclusively to fluvial processes. It is also questionable to treat Spearman's rank correlation as evidence of a physical mechanism. Correlation indicates only covariation, not a causal relationship; therefore, some of the interpretations extend beyond what can be directly supported by the results.

We would like to thank the reviewer for the comment. As previously answered, we acknowledge that wind can influence seismic records, including at low frequencies. However, our results show a significantly stronger correlation between seismic data and water height than with wind velocity (Fig. 7), while the temporal patterns of seismic PSD and water height are also highly consistent (Fig. 8). We therefore clarified in the manuscript that the observed low-frequency variations are primarily consistent with river-related processes, although wind-related effects cannot be completely excluded. Eventually, we added a statement in the revised version of the paper highlighting that, although wind is a well-known broadband source of seismic noise and remains one of the main challenges for the interpretation of environmental seismic signals, the observed low-frequency variations are primarily consistent with river-related processes. These processes may include changes in water flow, bedload transport, and water–air interactions, which are known to contribute to seismic signals generated by rivers, L477-482 of the revised manuscript: “*Although it is well known that wind and rain generates seismic noise over a broad frequency band (Rindraharisaona et al., 2022), our analysis makes it possible to exclude wind and rain as the primary factors controlling both the shape of the PSD curve and its time lag relative to water height (Fig. 6). The obtained values of Spearman's correlation are indeed higher than 0.9, demonstrating that the shape of PSD curve filtered in specific frequency range reproduce quite well the shape of the water height. This indicates that the seismic noise generated by the river emerges from other storm-related noise sources, such as wind and rainfall*”.

Further concerns relate to the interpretation of the physical processes. The explanation of the characteristic V-shaped signature remains speculative, because the authors propose

two possible hypotheses—source migration or changes in the grain size of the transported material—without performing analyses that would allow these hypotheses to be distinguished. Similarly, the interpretation in terms of Rayleigh waves is not supported by phase analysis, FK analysis, beamforming, wavefield analysis, or dispersion analysis. The polarization analysis also appears insufficient, as it is limited to the estimation of back azimuth and degree of polarization (DOP), without considering ellipticity, rectilinearity, planarity, or wave-type identification.

We thank the reviewer for pointing out the possible alternative interpretations of the V-shaped pattern observed in the spectrogram. We agree that the available data do not allow us to identify its specific physical origin with certainty.

Nevertheless, the observed spectral variations show a clear temporal correspondence with changes in water height during the flood event, suggesting that the V-shape is related to the evolution of river conditions. We have therefore clarified in the revised manuscript that its interpretation in terms of specific river processes should be considered as a hypothesis rather than a definitive explanation.

A more detailed investigation of the mechanisms responsible for the V-shape would require additional observations that are not available for the studied catchment, such as seismic stations installed closer to the river channel and measurements capable of directly monitoring bedload transport or sediment dynamics (e.g., sediment traps). Therefore, the aim of this study is not to provide a definitive interpretation of the V-shape mechanism, but rather to describe the observed relationship between seismic signals and flood evolution using the available seismic and hydrological datasets.

Also, the FK analysis here suggested by the reviewer would be out of context of the paper, as would require a seismic array properly set for field investigations, while our intent is to “use an existing seismic network originally intended for earthquake monitoring, demonstrating the potential of low-cost environmental monitoring”, as the reviewer correctly reported in the introduction of the posted minor comments.

To conclude, we report here the reference made in the previous review phase as answer to the reviewer’s question as the modified part of the paper in L457–459: “*Though our findings*

are in line with the aforementioned works, attributing the V-shape observed in Fig. 4a exclusively to variations of bedload size, water depth, or distance to the source(s) remains speculative and needs further investigations, supported by field measurements”.

The interpretation of hysteresis and time lags also remains ambiguous. The authors associate hysteresis primarily with bedload transport, whereas it could equally result from flood-wave propagation, catchment storage, nonlinear hydraulic response, or other processes. Likewise, the time lags between the seismic signal and water level may be related to the geometry of the monitoring network, the location of the gauging stations, flood-wave propagation, or the method used to calculate PSD, rather than solely to the earlier detection of fluvial processes.

We thank the reviewer for highlighting the possible influence of the spatial offset between the seismic stations and the water-level gauges on the observed time lag between seismic PSD and water height.

As stated in the authors' answers, to investigate the geometrical effect suggested by the reviewer we estimated the mean propagation velocity of the flood wave between the Venzone (C305) and Villuzza (C621) gauges, which are approximately 25 km apart along the river path. Based on the time lag between the two water-level records obtained through Spearman's correlation, we estimated a mean flow velocity corresponding to a propagation time of 120 ± 15 min (Fig. S6a–b). Using this value, we calculated the expected time shift associated with moving the Villuzza gauge 9 km upstream, toward the MPRI station. The resulting time correction was approximately 43.2 min.

We then compared the original hysteresis with the hysteresis obtained after applying this correction (Fig. S6c). The comparison shows that the observed lag between seismic and water-level data is only weakly affected by the spatial offset between the stations. Furthermore, if the station position were the only factor controlling the observed lag, the BOO–Venzone hysteresis should exhibit a counterclockwise behaviour, since the water-level gauge is located upstream of the seismic station.

These results suggest that the time lags obtained from the Spearman's correlation cannot be attributed solely to the spatial distance between the stations. Rather, the seismic signal recorded at the stations may integrate contributions from different portions of the catchment, potentially detecting river-related seismic sources several hours before the flood wave reaches the nearby water-level gauge, particularly during the initial stages of the event, as discussed in the manuscript.

The interpretation of the flash-flood results also raises concerns. The authors suggest that the absence of a detectable seismic signal indicates the absence of bedload transport, which constitutes an example of the logical fallacy that absence of evidence is not evidence of absence. Equally plausible explanations include a signal amplitude that was too low, masking by other noise sources, the presence of the signal outside the analyzed frequency band, or the event being too short to allow unambiguous detection.

As already pointed out by the reviewer, the flash flood event interpretation as not detectable event for only its bedload seismic contribution could be misleading. We agree that the absence of a detectable seismic response cannot be considered evidence that bedload transport did not occur during the event.

We believe that the different response observed during the flash flood may be related to its short duration, the spatial distribution of rainfall within the catchment, and the lower water levels reached compared with the larger and longer-lasting flood events. In particular, the location of rainfall within the catchment may influence how runoff is conveyed toward the river and whether sufficiently strong hydrodynamic conditions develop in the river sections contributing to the seismic signal recorded at MPRI and BOO. The relatively large and variable width of the Medio Tagliamento may further affect the propagation and distribution of the flood wave.

We have therefore clarified this aspect in the revised manuscript (L600–614) as follows:

“The analysed flash flood did not generate detectable river-induced seismic signals. The duration of the rain peaks and, consequently, the range of time concerning significant var-

iation of the water height in the river, could play a leading role in the quantity of water conveyed into the river in Tagliamento River. Also, intense but localised rainfall (18–21 mm/15 min) may have generated the flood wave in catchment sections hydrologically disconnected from the most water turbulence reaches (e.g., narrow section of Venzone or Fella River confluence), reducing the seismic amplitude generated by the river within the sensor's field of view. According to the model proposed by Gimbert et al. (2014), the seismic PSD is dependent on water flow height. This is consistent with our findings, as the flash flood that occurred on 19 September 2023 did not reach the water levels observed during Storm Ciaràn, with maximum stages remaining below 1.5 m compared to approximately 3.5 m at MPRI and 4.5 m at BOO during Storm Ciaràn (Fig. 6). Consequently, the seismic signal generated by the river was likely too weak to be clearly detected at the seismic station. This effect, combined with the localized and short-lived nature of the rainfall event, likely prevented the river-induced seismic contribution from emerging above the background seismic noise. Under these conditions, the recorded seismic noise at BOO was dominated by daily anthropogenic sources (1–15 Hz), while at MPRI it is related primarily to the meteorological forcing (wind and rainfall), overlapping and covering the seismic river source.”

Another important limitation is the lack of independent field data on bedload transport. The authors do not have granulometric measurements, direct sediment-transport measurements such as Helley–Smith sampling, camera observations, ADCP measurements, LiDAR data, or analyses of channel-bed morphological changes. As a result, all conclusions concerning bedload transport are indirect. In addition, although the authors refer to the models of Tsai and Gimbert, they do not use them for a quantitative interpretation of their own results, nor do they attempt to model flow, process energy, or expected PSD values. The analysis therefore remains primarily empirical.

We would like to thank the reviewer for the suggestion of the analysis that could be done to improve the quality of the work as rich investigation of the flood event and multidisciplinary approach.

More specifically, we fully agree with the reviewer regarding the lack of direct granulometric measurements. To make the statement in the Conclusions more conservative and avoid drawing conclusions that cannot be directly supported by the available data, we have revised

L621–624 as: *“From the comparison between PSD and water height it is possible to reconstruct the river flow behaviour (and thus have information about river conditions) drawing a hysteresis trend, especially focusing on specific bands of frequencies, that are most correlated with river water height time series (16–20 Hz, 17–21 Hz and 2–6 Hz for FUSE, BOO and MPRI stations, respectively).”*

This modification removes the specific reference to transported sediments and provides a more general interpretation in terms of river conditions, which is more consistent with the available observations and the scope of the study.

The choice of the analyzed frequency range, 1–40 Hz, also raises some concerns. The authors themselves note that rainfall-generated signals often occur above 50 Hz, yet they restrict the analysis to 40 Hz, which may hinder effective source separation. Conversely, excluding frequencies below 1 Hz may result in the loss of information related to processes operating over longer timescales.

We thank the reviewer for this observation. As previously posted, we agree that the frequency range considered in the analysis is relatively limited and that extending it, particularly towards higher frequencies, could provide additional information on meteorological sources affecting the seismic recordings, helping discriminate the environmental noise from river generated noise during the flood. For example, previous studies such as Roth et al. (2014) have investigated frequency ranges above 50 Hz to better characterize the seismic response to rainfall.

However, the available seismometers used in this study were configured with a sampling frequency of 100 Hz, properly set for seismic monitoring, which limits the reliable frequency range to below the Nyquist frequency. We therefore restricted our analysis to frequencies up to 40 Hz to avoid potential aliasing effects. Furthermore, as already discussed in L205–209, frequencies below 1 Hz are strongly influenced by microseismic noise (Hasselmann, 1963; Longuet-Higgins, 1950), mainly associated with wave activity in the Adriatic Sea to the south. This contribution could interfere with the identification of river-related seismic signals, particularly during intense flood events such as Storm Ciaràn.

For the sake of clarity, we report here referred section L205-209: “*We analysed filtered records between 1 and 40 Hz. The high cut off was set below the Nyquist frequency to overcome aliasing and we discarded all frequencies below 1 Hz because it has been consistently shown that river-related signals have their strongest influence above 1 Hz (Abi Nader et al., 2023; Burtin et al., 2008; Gimbert et al., 2014; Barrière et al., 2015a). Also, since microseismic noise mainly contaminates the low-frequency range (<1 Hz) (Longuet-Higgins, 1950; Hasselmann, 1963), our analysis was restricted to frequencies higher than 1 Hz to reduce its impact.*”

Finally, the statistical analysis appears insufficient. Apart from Spearman’s rank correlation coefficient, the study does not present uncertainty analysis, confidence intervals, bootstrap procedures, sensitivity analysis, or tests of result stability.

We thank the reviewer for this valuable comment. We agree that uncertainty estimates and additional robustness analyses can provide further information on the stability of statistical relationships.

However, we would like to clarify that the relationship between river level and seismic noise was not assessed on the basis of a single correlation coefficient. The cross-correlation analysis was performed by systematically testing different frequency bands and a range of time lags. This allowed us to evaluate the sensitivity of the observed relationship to both the frequency content of the seismic signal and the temporal offset between the river-level and seismic observations. The reported correlation and corresponding time lag were therefore identified within this broader parameter space rather than selected a priori. As stated by the reviewer in another comment, the analysis “remains primarily empirical”. The methodology is mainly based on the calculation of PSDs for one major event, further compared with hydro-meteorological time series. The main goal of this work is to demonstrate the efficacy of pre-installed seismic network not properly set for the river monitoring and placed at distance from the source, as described on the abstract.

We acknowledge that bootstrap resampling and confidence intervals could provide an additional quantitative assessment of the uncertainty associated with the correlation coefficient.

Nevertheless, implementing a comprehensive uncertainty framework of this type would require a substantial revision of the statistical methodology and additional reprocessing and validation of the dataset. We therefore consider that such an extension would go beyond the scope of the present study.

We have clarified the cross-correlation procedure and the sensitivity of the results to frequency band and time lag in the revised manuscript. We believe that these analyses provide an important assessment of the robustness of the observed relationship, while we acknowledge that a more comprehensive uncertainty analysis could be addressed in future work.