

RC1:

One of the main issues in the study is the lack of clear separation of seismic signal sources (river vs. wind vs. rainfall). The authors assume that the river dominates at low frequencies, while wind and rainfall dominate at high frequencies. However, the results show a high correlation of wind (0.7–0.8) across the entire analyzed frequency range. Despite this, low frequencies are primarily attributed to river processes, indicating interpretative inconsistency and a lack of effective source separation.

We agree with the reviewer that wind represents an important source of seismic noise over a broad frequency range, and therefore we cannot exclude that wind-related effects may also influence the seismic records at low frequencies in the analysed dataset. Indeed, the separation of meteorological and hydrological contributions to seismic signals remains a challenging issue in fluvial seismology, particularly for stations located at some distance from the riverbed.

However, the results presented in Fig. 7 indicate that, at low frequencies, the relationship between seismic data and water height is significantly stronger than the correlation observed with wind velocity. In particular, the correlation values between seismic and water height data reaches values higher than 0.9 in the analysed low-frequency range, whereas the correlation with wind remains lower (approximately 0.7). Moreover, the comparison between the temporal patterns of seismic power spectral density and water height variations shows a clear similarity (Fig. 8), both during the rising and descending phases of the flood event, supporting the interpretation of a dominant hydrological contribution.

We have therefore added a statement in the manuscript 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.

Another concern is the assumption that the vertical component of the signal (interpreted as Rayleigh waves) directly corresponds to river processes. At distances of 2–3 km, the signal may be significantly attenuated, scattered, or mixed with other wave types (e.g., body waves) and noise. There is no direct evidence supporting this assumption, such as phase analysis or more advanced wavefield analysis.

We thank the reviewer for the comment regarding the propagation of river-induced seismic signals over distances of 2–3 km. We agree that attenuation effects and wavefield mixing are important

aspects to consider in fluvial seismology, and that at these distances the seismic wavefield may not be exclusively composed of Rayleigh waves.

Our interpretation of the seismic signal as being mainly associated with Rayleigh waves was based on several observations and assumptions. First, following the model proposed by Tsai et al. (2012), bedload impacts on the riverbed can be approximated as normal forces, which preferentially generate Rayleigh-polarized waves with a dominant vertical component (Woods, 1968). This theoretical framework motivated our choice of analysing the vertical component of seismic recordings. Moreover, as discussed in the manuscript (Lott et al., 2017; Withers et al., 1996), the vertical component provides a more conservative approach regarding wind-induced noise, which generally has a stronger influence on horizontal components.

In addition, the observed spatial and temporal patterns of the seismic signal, together with its relationship with river dynamics, are consistent with the expected behaviour of surface waves generated by fluvial processes. The propagation characteristics of the signal and its persistence at the analysed distances also support the possible contribution of Rayleigh waves, as surface waves are less affected by geometrical spreading compared to body waves.

Eventually, the polarization analysis resulted in back-azimuth values that point towards the sections of the river closest to the seismic station, suggesting the seismic contribution given by the Tagliamento (and But torrent for wavefield received at FUSE station) during the flood event.

However, we acknowledge that, at distances of a few kilometres from the river source, the seismic wavefield is likely affected by scattering, attenuation, and the mixing of different wave types. Therefore, we have revised the manuscript (Sections 1 and 3.4) to avoid implying that the observed seismic noise is exclusively composed of Rayleigh waves, and we now refer to it as a signal with a predominant, but not exclusive, Rayleigh-wave contribution.

The interpretation of the characteristic “V-shape” in spectrograms is speculative. The authors suggest two possible explanations: migration of the source (flood wave front) or changes in the size of transported material. However, no analysis is provided to distinguish between these hypotheses.

We thank the reviewer for the comment regarding the interpretation of the V-shape observed in the spectrogram. We agree that this pattern may have multiple possible explanations and that its interpretation in terms of specific river processes remains speculative.

However, we would like to emphasize that the observed frequency modulation during the flood event is strongly related to variations in water height and, consequently, to changes in river conditions. The available observations indicate a clear temporal correspondence between seismic spectral variations

and the hydrological evolution of the event, supporting a relationship between the seismic signal and river-related processes.

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.

Accordingly, we have added a statement in the revised manuscript clarifying that the interpretation of the V-shape remains a hypothesis and that further investigations, supported by additional field measurements, are required to better constrain the underlying physical processes (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”.

Similarly, the interpretation of time lags between seismic signals and peak water levels appears oversimplified. These delays may result not only from hydrological processes but also from the geometry of the measurement setup, spatial differences between stations, or data artifacts. The study does not include analysis that would allow these factors to be clearly distinguished.

We thank the reviewer for the good observation about the time lag between PSD and water height data. Indeed, the spatial offset between seismic station and level gauge could influence a part of the lag. So, to assess this aspect, we calculated the mean flow velocity from Venzone (C305) to Villuzza (C621) using the distance between them in the river path (25 km) and the time of propagation intended as lag between the two water height curves, calculated with the Spearman’s correlation, resulted to be 120 +/- 15 min (Fig. S6a-b, attached in the manuscript as supplementary material). It is worth to note that these stations are used for their consecutive position in the Tagliamento way path. From this calculated mean flow velocity, we estimated the time gap given by the theoretical spatial offset of Villuzza (C621) level gauge moved by 9 km upstream towards MPRI, as close as possible to the station in the river path to overcome gaps generated by the propagation of the water flow, resulted to be 43,2 min. Then, we provide here as Fig. S6c the difference between original hysteresis and the one with the shifted level gauge time lag. This demonstrates that, despite the offset of 9 km and the estimated time of 43,2 min, the lag between seismic and water height data is weakly affected by the position of the stations. In addition, if the position between station would be the only responsible of the lag, the BOO-Venzone hysteresis would be counterclockwise, as the level gauge is upstream

relative to the seismic station in this case. To conclude, lag values obtained with the Spearman's correlation analysis are not exclusively given by the source-station distance, but the seismic station likely receives seismic waves generated from a different part of the catchment, even hours before the flood wave reaches the level gauge near the seismic station and during the beginning of the flood, as stated in the manuscript.

The conclusions regarding flash floods are also questionable. The claim that the absence of a seismic signal implies the absence of sediment transport represents a logical fallacy (absence of evidence is not evidence of absence). Alternative explanations — such as insufficient signal strength, short event duration, or masking by noise — are not adequately considered.

We sincerely thank the reviewer for the comment based on the flash floods. We agree that the absence of a detectable seismic signal in this the case can't be supposed as a proof that no bedload transport occurred during the event. However, middle section of Tagliamento where MPRI is located (called Medio Tagliamento) has variable width between 400-1000 m, depending on the considered location. This could influence the conveyance and spread of rainwater from the peripheric sections of the catchment into the river stream. The flash floods seem to have different impacts on seismic contribution generated by the river from the big and long-lasting floods due to their diverse trigger characteristics. Hence, the duration here could play a leading role in the amplitude of the seismic waves from a riverine source during a flash flood in Tagliamento River, considering the difference in important peaks of rain concentrated in a short period of time. Also, we think that the drop location of the rain within the catchment is important to understand why the seismic station is failing to detect a rise of 1-1.2 m of water height, as the flood wave could be generated in a section unfavourable to evolve in a flood wave in the river with features able to be detected by the seismic stations at 0.4 - 3 km from the river. This could explain why MPRI at 3 km from the Tagliamento does not detect PSD variations during flash flood, even at lower frequencies.

Considering these aspects, the L600-614 were modified 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 significative variation 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 relatively large distance between seismic stations and the river (0.4–3 km), which may lead to significant signal attenuation and hinder clear interpretation of its sources.

We would like to thank the reviewer for this comment, as we totally agree with him. We recognize the limitation of the seismic station for the exploited network distant from the river (0.4-3 km) and this could lead to signal attenuation from the source to the seismometer. Indeed, as well known in literature, installing new seismic stations close to the river for this purpose could help in collecting high quality seismic waves less influenced by the attenuation and noise disturbances (meteorological or anthropological). However, despite the methodology applied in this work, significative correlations were made with the river component. This underlines the feasibility of using available stations in poor or totally absent gauged systems to monitor river conditions, as highlighted in the abstract and also cited in the conclusions of the manuscript L629-638 of the revised manuscript.

Additionally, the study lacks direct field data on sediment transport (e.g., in situ granulometric measurements), meaning that conclusions about bedload are indirect and not empirically validated.

We totally agree with the reviewer’s comment about the lack of direct granulometric measurements. To be more conservative about the sentence concerning the bedload in the conclusions, we opted for modifying the L621-624 as follows: “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 sentence leaves all the references to transported sediments, making it more generic with “information about river conditions”.

Another issue is the reliance primarily on correlation analysis. Correlation does not imply causation and may result from the influence of a common variable, such as rainfall affecting both river discharge and seismic noise simultaneously. The study does not attempt to disentangle these effects.

We thank the reviewer for raising this important point. We agree that correlation analysis alone does not demonstrate causality and that common forcing factors, such as rainfall, may simultaneously affect river conditions and seismic noise generation. This represents a well-known challenge in the interpretation of environmental seismic signals, where different sources can contribute simultaneously to the recorded wavefield.

In this study, correlation analysis was used as a first-order approach to identify the frequency ranges in which seismic variations are most closely related to the flood evolution. Given the available seismic network and the distance of the stations from the riverbed, this approach represented the most suitable method to investigate frequency-dependent relationships between seismic and hydrological parameters.

To address the possible influence of common meteorological forcing, we expanded the analysis by comparing seismic data not only with water height variations but also with rainfall rate and wind velocity (Fig. 7). This comparison was performed for the MPRI station, located less than 400 m from the meteorological station equipped with a pluviometer and an anemometer. The results show that, although meteorological parameters may contribute to the recorded seismic noise, the correlation between seismic power and water height is stronger in the low-frequency range, supporting the interpretation of a predominant river-related contribution.

We acknowledge that this is only an empirical evidence which does not allow a complete separation of the different contributions or a definitive demonstration of causality. Therefore, we have clarified this limitation in the revised manuscript (e.g., Section 3.2) and have moderated our interpretation, presenting the observed relationships as evidence of a strong association between seismic variations and river dynamics rather than as a direct proof of causation.

Finally, the selected frequency range (1–40 Hz) may be limiting. Rainfall often generates signals at higher frequencies (>50 Hz), and excluding frequencies below 1 Hz may result in the loss of important information about longer-period processes.

We would like to thank the reviewer for the comment. Indeed, the frequency range here is limited for this kind of survey, as a wider range, especially for the higher frequencies, would help in analysing the noise generated by the meteorological sources affecting the seismic data. In fact, many studies in literature (e.g., Roth et al., 2014) exploit ranges overcoming 50 Hz to mainly evaluate the rain impact. However, in our case the seismometers are set with 100 Hz sample frequency, which allow us to fully investigate frequencies up to 40 Hz due to the aliasing effect. Moreover, as explained in lines 199-200, frequencies below 1 Hz are mostly affected by microseism (Hasselmann, 1963; Longuet-Higgins, 1950) coming from Adriatic Sea in the South, which could interfere with our analysis during strong flood events like Ciaràn Storm.

References:

Woods, R.D. (1968) Screening of Surface Waves in Soils. *Journal of the Soil Mechanics and Foundations Division, American Society of Civil Engineers*, 94, 951-979.

RC2:

We thank the reviewer for the valuable work made that helped in improving the quality of the work. With this section, we want to answer the general comments made from the reviewer, whereas the specific points are discussed in detail in the corresponding responses provided below. First, the reviewer made us note that the BOO station results for the Spearman's correlation were performed with a different frequency window, so we had to repeat the analysis and, now, the results are further discussed, considering only the high range of frequency with the highest correlation value found between water height and seismic data. In addition, the statement related to the big lags found in this work is now extensively discussed in the paper, and we are reporting here the explanation as a comment to clarify this aspect. Also, the part regarding the hysteretic behaviour is now discussed in a more detailed way and the figures were modified following the suggestion made by the reviewer (e.g., Fig. 3). Eventually, we specified in the conclusions section the importance of the use of seismic stations to study flood events, even installed at distance from the river, underlining the difficulty we had with the same analysis performed for the flash flood in L646-651: "It is worth to note that, during the analysed flash flood occurred on 19 September 2023, the methodology used for this work was not able to clearly identify the same seismic features generated by the river during the Ciaràn Storm, probably due to different characteristics and behaviours between the two events (e.g., different duration of the flood, maximum water height registered).".

L 104. The use of quotation marks seems strange here

The quotation marks were added to the sentence because it was copied from Monegato and Stefani (2010), but now are removed from the revised version of the manuscript

L 105: “The area is tectonically active”. If the sentence is retained, provide some indication to prove that (seismic activity, evidences for active faults, etc..)

We thank the reviewer for the suggestion. An example of an important recent earthquake in L111-113 could clarify the recent fault dynamics in the catchment. Thus, the sentence has been modified as follows: “The area is tectonically active, characterised by thrust and fault structures that influence the tributary stream orientations, especially in the north-eastern part of the catchment (Ward et al., 1999). The last strong earthquake in the area occurred in recent years 1976, reporting 6.4 as magnitude Mw (Lyon-Caen, 1980; Galadini et al., 2022)”

Section 1: I think that a. comment about the steepness of the river is needed: presence of falls, steady gradient, changes along in steepness along the investigated area, riverbed width?

As kindly suggested by the reviewer, a section describing the morphology of the analysed section of the river would improve the clarity of the idro-geo morphological settings in Section 1.1. For this reason, we opted to add more information about the gradient and the width of the river in L108-113: “More specifically, the investigated reach of the upper Tagliamento River extends for approximately 40 km from Tolmezzo (~325 m a.s.l.) to Villuzza (~125 m a.s.l.). The river exhibits a broad gravel-bed morphology, with the channel width increasing downstream from 300–600 m near Tolmezzo to as much as 800–1000 m at Villuzza. Despite an average longitudinal gradient of 0.8–1.0%, the profile is not uniform, and local bedrock-controlled sections, particularly near Venzone, attain slopes of up to 6.5–7% over distances of about 1 km.”.

Figures 1 and 2: Using a topographic map instead of a satellite image as a background will make the figure more readable. In their present form, the background is very dark and features as the Fella-Sava fault are hardly visible.

We would like to thank the reviewer for this comment. We agree with the new background in topographic map, as could help visualize the stations, rivers and geologic structures (e.g., Fella-Sava fault). Figures 1 and 2 are now shown in topographic map.

The Introduction section could be rearranged: meteorological data is included in sections 1.2 and 1.3, a section discussing the geometry of the river bed (steepness, presence of falls, width of the channel...) should be added.

The reviewer here kindly suggested to rearrange the sections 1.2 and 1.3 in a single section, as meteorological events and methodologies for hydrological and meteorological data are described in both sections. As a result, 1.2 and 1.3 are now merged into a single section titled “1.2. Hydro-meteorological data and analysed floods”. Also, the reviewer cites about a new section comprehensive of morphology of the river in terms of steepness, presence of falls and width of the channel. We opted to add the considered information in previous section in L108-113 (previous comment) with a new title that better reflects the content “1.1. Geological and morphological settings”.

I think the structure of the paper will be clearer if the information relative to seismic data and processing are presented in a different section “2. Seismic data and processing”, including the sections 1.4, 1.5, 1.6 of the submitted manuscript.

As reported by the reviewer, sections 1.4, 1.5 and 1.6 could be included in one single section with title “Seismic data and processing”. As a result, a new section with this name is added, including all the cited sections.

L 190: A reference for the different packages is probably needed.

Matplotlib, SciPy and TwistPy citations are now added in the sentence indicated by the reviewer (now in L197-199): “and other related libraries with useful scientific tools (e.g., Matplotlib from Hunter, 2007; SciPy from Virtanen et al., 2020; TwistPy from Sollberger, 2024).”

L 199: “mainly” can be suppressed (microseismic peak extends from 0.05 to 1 Hz)

The word “mainly” in L199 has been deleted, as indicated by the reviewer

L 201: Obspy can be referred by a usual citation (Krischer et al 2015)

The citation suggested by the reviewer has been added as “Krischer et al. (2015)”

L 210: Precise the kind of meteorological time series (wind, rainfall)

In L210 (now in L219), “rain rate”, “wind velocity” and “water height” have been specified as meteorological and hydrological time series, as suggested by the reviewer

L225: You could add a sentence stating that the numbers in brackets following each stations are those appearing in Fig. 2

In L225 (now L237-238), the sentence “with codes in brackets relating to Fig. 2” has been added to clarify the further list of stations, referring to Fig. 2

L235: I think it can be useful to comment the distance between the seismic, meteo, and hydro stations for each set.

Indeed, the station-river and station-seismometer distances in L235 (now L247-249) could help understand the disposition of the used network, as suggested by the reviewer. Thus, we opted to add this information as follows: “Also, for the Venzone section we used 2 meteorological stations, as the instrument 118 is a pluviometer (missing anemometer) and the instrument 58 is an anemometer (distant 2 and 3.8 km respectively from the seismic station BOO, 2 and 1 km from the river, respectively).”

L 279: Figure 3 shows the spectrograms, but not the PSD curves

We would like to thank the reviewer for the comment. In L292 we refer to the PSD values in spectrograms (Fig. 3c-d), which gets higher during the rising phase of the water level curve in Fig. 3a-b. To make the sentence clearer, we added “The PSD in the spectrogram calculated on the selected seismic stations”, now in L 293-294.

L 287: Based on the spectrograms, stating that FUSE noise is linked to wind and rain above 30 Hz and flood at lower freq. seems not justified. I propose to suppress this comment here and retake the discussion when commenting Fig. 7.

We would like to thank the reviewer for the comment regarding FUSE station spectrogram in Fig. 3. In L287 we want to underline just the pattern, but we are not linking the frequency contributions to the sources yet. Also, the Fig. 7 is related only to the MPRI seismic station, which allows to perform the correlation with meteorological parameters for the vicinity of the anemometer and rain gauge to the seismometer. As a result, investigating the same parameters with FUSE station would be misleading, as the rain gauge and anemometer are 1.5-2 km distant from the seismic station. To follow the reviewer suggestion, we added new information in L287 (now L301-304) to clarify this aspect: “The spectral analysis of the FUSE station recordings shows that the PSD in the first part of the flood tends to follow meteorological parameters over time, such as wind velocity and rain rate. Also, the frequencies between 1 and 30 Hz align more closely with the water height curve, displaying a decrease in PSD values during the descending phase after the peak in water height (Figs. 3c–d). This aspect is further explained in Section 3.2 and Fig. 6”

Note that the spectrograms in the figure 3, altogether with the wind speed values and the late increase of water height suggest that most of the noise is due to wind. I propose to state in this section that spectrograms suggest a strong contribution to wind, but that the detailed analysis presented in the next section prove that flood has a clear role in explaining the noise at low frequencies.

We want to thank the reviewer for the note made about FUSE spectrogram and seismic feature described in Fig. 4a. As suggested, we added a new sentence in L312-314: “Even if high seismic PSD here seems to show strong contribution to rain-wind data (Fig. 4a), the data presented in the next sections prove that river component has a clear role in explaining noise during the flood, especially at low frequencies”.

In the previous section it has been stated that wind bursts arrived to 100 km/h, while the graphics here only show values around 15km/h. I understand that these are mean values over a time period, but still the difference is quite sticking and should be commented.

As the reviewer suggests, 100 km/h is a high value relating to our analysis, reporting only 15-20 km/h as mean maximum. This led to a late noted mistake caused by Figure 3 and Figure 10 wind time series and their interpretation. The actual value is related to m/s, which would be 54 and 72 km/h,

respectively, which is more closely related to reported 100 km/h in the introduction section. Moreover, in the Section 2 “Results” the wind velocity values are related to m/s for the Beaufort scale classification. We want to apologize for this unknotted mistake. Now, the Figures 3 and 10 are corrected with m/s wind velocity labels, as well as the description of their results.

Fig. 3: As commented above, I would appreciate to include here panels showing the seismic wiggles. We thank the reviewer for the suggestion to add the waveform in Fig. 3, as this information could enrich and improve the quality of the paper. We appended the FUSE and MPRI waveforms as Fig. 3e and Fig. 3f, respectively.

Fig. 4: Although PSD and water height are compared later, you may consider to add here the water height curve

Considering the suggestion of the reviewer, we added the water height curve in Fig. 4b, as it could emphasise the major seismic contribution during the first part of the curve, which is recognised in literature having the stronger seismic energy than the descending part of the water height curve, leading to the recognised hysteretic behaviour of the river floods.

Fig. 5: As the correlations are calculated in 5 Hz intervals overlapping intervals in steps of 1 Hz, I would suggest to label the frequency panels using simply 1,5,10,15...Hz ticks, instead of using “1-5”, “7-11” etc

In Fig. 5, the ticks have been modified in single numbers rather than ranges of frequency, as suggested by the reviewer.

As commented above, I see a difference between the low frequency correlation at BOO and MPRI. While in the later the correlation peaks at 2-6 Hz, the coefficient remains high till at least 12 Hz and then diminishes smoothly. On contrary, for BOO there is only a peak in the 1-5 Hz, that I suspect that may be due to some kind of spurious feature. I would appreciate a discussion on the real significance of the first peak at BOO

We would like to thank the reviewer for the good comment. This made us note that the correlation for the BOO station and its respective lag in Fig. 5c-d were calculated with 3 Hz window length by

mistake. We apologize for the confusion. As a result, we modified the figure with the new plots showing a single maximum correlation value at 17-21 Hz frequency range and a relative time lag of 270 minutes, which seems more in line with our analysis (increasing lag of 255-270-360 min for FUSE-BOO-MPRI, respectively). Also, all references to the old 1-5 Hz frequency range for BOO station are deleted from the revised version of the manuscript. The increase of the Spearman coefficient for very low frequency ranges (1-5 Hz) is now less peaked but still an increase could be seen. This has been interpreted as “probably derived from the effects of the wind perturbation acting on the near trees or antenna, which generates low frequency waves from the wiggle of the near objects and maybe beyond our lower investigated threshold of 1 Hz. Also, we don’t exclude the contribution of high frequency microseism (near 1 Hz) generated by the strong meteorological perturbation influencing the offshore sections with lowest range of frequency, considered in this work” in L 468-471.

The large lag times observed, between 3 and 6 hours are quite surprising and should be highlighted in the text, indicating that their origin will be discussed later.

Following the suggestion of the reviewer, the large lag times are now highlighted in the text with the sentence in L347-348: “The reported lags range from 255 to 360 min for the three used stations. The interpretation will be discussed later in section 3.2.”. As a result, in section 3.2 is reported the interpretation of the time lags we have found in our results, discussed in detail in further answer.

Fig. 8: Explicit that the observed hysteresis curves are counterclockwise and comment that the hysteresis is very clear at MPRI, less consistent FUSE and BOO (17-21) and much less evident for BO= (1-5Hz)

We would like to thank the reviewer for the comment. Underlining the verse of the hysteresis (which we think the reviewer is referring to “clockwise” and not “counterclockwise”, as reported in the comment) could be a good information to improve the presentation of the results and their discussion further in the paper. We modified the L395-397 as follows: “More specifically, FUSE and BOO reported a more scattered hysteresis than MPRI, with a higher difference between minimum and maximum PSD value reported in BOO rather than in FUSE”.

L 384: Several paragraphs start with “Moreover”, “furthermore” that could be suppressed
Following the reviewer’s comment, most of the paragraphs starting with “Moreover” or “Furthermore” are now suppressed in the revised version of the manuscript (e.g., L405).

Figure 9: While two of the panels show the full backazimuth range (0-180), the two others focus on a very limited range (160-185°, 148-178°). This makes their visual comparison difficult. Use the full range for all the cases and comment that in some cases, the difference in polarization during flooding is smaller than in others

Regarding the Fig. 9, the reviewer made a good suggestion to compare the polarization outputs with the same oriented scale. Now, Fig. 9 polarization plots are shown in the 0-180° range of back-azimuth, and the relative discussion in L555-558 (revised version) are modified as follows: “The presented results in Fig. 9 show a major difference in back-azimuth for FUSE station, while BOO and MPRI display a less evident transition between background and seismic waves orientation during the flood. This suggests a different impact due to the station-source distance or a similar orientation between background noise (generally more scattered) and flood noise (clearer oriented azimuth)”.

L 405: The attenuation with distance of high frequency signals can be documented citing any general seismology book, no need to refer to contributions focused on fluvial seismology.

In L405 (L431 in the revised version) the citation was modified with a general seismology book by Shearer (2009), as suggested by the reviewer.

L409: Stating that the PSD reaches its maximum “just before peak water height” is confusing, taking into account that the observed time lags are of several hours !!

As suggested by the reviewer, the L409 (now L435) has been modified as: “during the increasing part of the water height curve” to overcome misleading referred to the found time-lags.

L442: The reference should be to Fig 5 and L448: The reference should be to Fig.7 We would like to thank the reviewer for the comment. We agree with the suggestion as the citations to Fig. 6 and 8 were incorrect. Now, we cited the corrected figures in the revised version of the paper (Fig. 5 and 7)

L449-453: AS stated before, this time lags are surprisingly large and should be discussed in further detail

As suggested by the reviewer, a further detailed discussion with new analyses is added in section 3.2-3.3) as follows: “Time-lag differences among sections (255 for FUSE, 270 for BOO, and 360 minutes for MPRI) likely represent the lag between the seismic detection of the flood perturbation and the subsequent water-height peak at the corresponding gauge, without considering gauge stations misalignment. Although it is well known that wind and rain generate 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.

Furthermore, to rule out the possibility that the large time lags obtained from the correlation analysis are simply due to the relative position of the seismic station with respect to the level gauge, we estimated the flood-wave propagation velocity for this event using the two level gauges at Venzone (C503) and Villuzza (C621). The time lag between the two water-level records was obtained through Spearman's correlation analysis, yielding a delay of 120 minutes with a Spearman's correlation coefficient of 0.96 (Fig. S6a–b). Given the 25 km distance between the two gauges (Fig. 1), this corresponds to an average flood-wave velocity of approximately 3.3 m/s.

Using this velocity, we estimated the travel time required for the flood wave to propagate from a point located 9 km upstream of the Villuzza gauge (C621)—approximately the closest point along the river to station MPRI—to the actual gauge location. The resulting delay is about 45 minutes. This value is much smaller than the 4–6 hours time lags obtained from the seismic–hydrological correlation analysis, indicating that the relative position of the seismic station and the level gauge has only a minor effect on the measured delays. These results therefore support the interpretation that the seismic stations detect flood-generated seismic waves originating upstream during the early stage of the storm, several hours before the water-level increase reaches the downstream level gauge.”.

Finally, in the section 3.2 we reported the interpretation of the lag concerning the hysteretic behaviour of the seismic data with respect to the water height: “the hysteresis analysis further confirms the delayed behaviour between the seismic (PSD) and hydrological (water height) signals. To evaluate the effect of the relative position between the seismic station and the level gauge, we compared the hysteresis obtained using the original Villuzza (C621) water-level record with that obtained after applying the theoretical upstream spatial offset described in Section 3.2 (corresponding to a 45-minute time shift for station MPRI; Fig. S6c). The two hysteresis loops are very similar over the same

frequency range, indicating that accounting for the station–gauge offset produces only minor changes in the seismic–hydrological relationship. This result confirms that the spatial misalignment between seismic stations and level gauges has a negligible influence on the overall time lags calculated using the actual station locations and can therefore be reasonably neglected in the analyses presented in this work.”

Comment on the significance of the counterclockwise hysteresis direction observed. Several authors have commented the presence of clockwise and counterclockwise hysteresis in fluvial seismology and discussed its possible origin. The different degree of hysteresis observed at the 3 stations can also be discussed

We would like to thank the reviewer for the suggestion related to the section 3.3. We agree with the comment, as a deeper insight on the type of hysteresis obtained in our work could help improving the quality of the paper. As a result, we added a brief discussion in section 3.3 L515-525 in the revised version of the paper: “The clockwise hysteresis behaviours, with a rapid increase of the PSD relative to the water height and successive slow decay to normal conditions, is widely documented in the literature (e.g., Burtin et al., 2008 on the Trisuli River; Hsu et al., 2011 on the Cho-Shui River; Shamndt et al., 2013 on the Colorado River). In our case, the hysteresis is mainly driven by the seismic signals generated by the flood anticipating the increase in the water height gauge in the river, near the station. Also, according to Borzì et al. (2025), the clockwise narrow hysteresis of the Cimia River indicates a main seismic contribution given by the turbulent river flow, as the transported sediments in their case is reported to be fine (e.g., sand and sandstones). While, in our case, the Tagliamento carries coarse bedload (Monegato and Stefani., 2010) and the morphology of the river itself is completely different, with very large riverbed (up to 1 km wide) and big quantity of water transported with turbulence from up to downstream. This leads to increases in seismic PSD amplitudes and, as a consequence, to a wider hysteresis, emphasizing the difference between the PSD values during the rising of the water height and its descending phase.”.