Revision to “Real-time Monitoring and Analysis of Debris Flow Events: Insight from seismic signal characteristics”

Author(s): Yan Yan et al.

General comment:

The authors present the analysis of rainfall and seismic data and infrared images recorded during 3 debris-flow events in two different catchments in China, each recorded with 2 different seismic station and infrared camera. In particular, they use spectral analysis of seismic data to extrapolate information of the events and also apply cross-correlation analysis to estimate flow velocity.

Despite the work offering good quality seismic data of debris flow activity, most of the analysis and conclusions do not seem convincing and the work lacks novelty compared to previous contributions. More specifically, even if and not an expert in modelling of seismic propagation, I am not convinced at all about the reconstruction of the original seismic signal removing the propagation effects; it is not clear if authors considered geometrical spreading, inelastic absorption and or site effects and their approach, limited to considering only a short channel section does not appear to be robust or acceptable. In addition, many conclusions derived from the spectral features of the recorded seismic signals sound highly speculative and poorly supported by experimental or theoretical evidence to me.

I am also not convinced about the stress given on the use of the proposed system for monitoring and early warning purposes. If on one hand the use of seismic signals already proved to be a promising and effective tool for monitoring and early warning of debris flows, on the other hand, the system here presented, also lacking real time transmission of recorded data does not demonstrate any use for real time monitoring, despite authors recall several time to the demonstrated use of their system for the real time monitoring. No real time detection system is presented. I would suggest therefore toning this down and simply say that the paper suggests once more that seismic sensors could be used for debris flow warning, in agreement with previous events.

Furthermore, the text is too long and difficult to follow, also presenting long trivial and repetitive sections, and despite it being long, lacks important information on instrumental set up, seismic source process, and data analysis. No information is for example given on sensor type (1d or 3d seismometers or geophones, brands, response ecc). Similarly, the method section results confused, offering superfluous mathematical details while lacking explanation of the analysis actually conducted in the framework of the paper (e.g. window of analysis in spectral and cross correlation analysis). I also found difficult to understand why (to get which information or to investigate what) are same analysis conducted in the study.

In addition, the introduction focuses on debris flow monitoring and goes on the difficulties of deployments in poorly accessible sites. I think that a section on seismic source processes, presenting the accepted models on debris flow seismicity, is missing in the introduction: authors use seismic signal to invert for debris flow dynamics and features, so this section should be present.

Finally, many sentences need to be linguistically revised and reworded. All text also requires to be shortened.

I therefore suggest paper rejection and resubmission after additional work is performed on data analysis, results interpretation and text revision.

Specific comments:

Title: Real-time monitoring? You don't perform any real time monitoring, please remove this from the title.

Introduction:

A section on the seismic source process in debris flow is missing (view Burtin 2009, 2014 Lai 2018, Kean 2015, Zhang 2021, Belli 2025…). Shorten the parts referring to hard access to DF sites.

Line 37-38: I don't understand this sentence "a strong correlation between… spectrum"

Highlights: the 3 highlights are not completed sentences; rephrase them all

Line 90-93: remove all lines 90, 91 and 92 and 93, leaving just "to monitor landslides (Li 207; Fuchs 2018), rockfalls…"

Line 104-105: also Belli et al 2022 found a linear relation between seismic signals and flow depth/ discharge. Also change "rate" with "magnitude"

Line 105-108: add "investigation of the source process" in the list and refer to Zhang et al., 2021 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021JB022755) and Belli et al. 2025 (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2025GL116107?af=R).

Line 111-113: remove this period

Line 113-117: I suggest to remove also this period

122-129: shorten this section

132: put a "." after "camera" and start a new period with "The system recorded 3 debris flows…"

137: semi-quantitative analysis of what?

Section 2:

153-155: not clear if these events are included in the 17 cited before

Figure 1: put the stations in the map; sea boundaries of China in the panel a are not evident

164-167: Slope is before 12° and then 15°.

164-165: "a debris flow transportation area of between 5 and 12°" ??

167-170: remove, already written at 150-152 and not relevant

175-176: remove, already written at 150-152 and not relevant

Section 2.2: I suggest renaming this section "Instrumental set-up" or "observation system" or similar instead of "monitoring system": a monitoring system requires real time alerting or warning and refers to surveillance purposes, and this is not the case. Change all the "monitoring system" referring to your instrumental set up in the text (e.g. line 207, 218, 220 …).

Section 2.2: revise the all section: please provide sensors specifications (seismometer or geophone model, digitizer, camera … Some of them are in the table which could be omitted then) and avoid trivial details on sensor deployment

183-206: all this section is too long and results trivial and not so meaningful: I suggest reducing it in a very few lines stating how you faced the low insolation of the area with no further details (single costs ecc)

210-211: and station 2?

Figure 2: seismometer instead seismograph. Is it possible to unify Fig 1 and 2 in a single Figure?

Section 3: Metodology: presenting the equations for FFT, PSD and cross-correlation functions appears superfluous to me, as these are well known techniques of analysis in geophysics. In addition the text is rather confused and difficult to follow. I would suggest removing the equations, rewriting the section in a simplified version more focused on your signal analysis and your scopes of investigations, rather than of mathematics and technical details. A structure like the following: "With the aim to investigate… we perform the XXXX analysis on seismic data… "

233-244: revise and rephrase all this section: the subject of the sentences is always missing

240: what are "on site investigations?" specify

242: keyframes?

242: what is the amplitude method?

Figure 3: is this necessary?

Section 3.1: this section is unclear to me; I suggest to put 3.1 and 3.3 together in a single section and revise it just stating that seismic signals are analyzed in the frequency domain and spectra are computed in the form of PSD and spectrogram

253-258: remove the equation

260-261: specify that you perform the x-corr analysis between the seismic signals recorded at the stations in each site. Do you perform it on raw data or on the amplitude envelope? As I understood it is the second one but make it clearer.

262-273: rewrite this removing the equation and explaining how and why you used the cross-correlation analysis

278-281: I suggest removing the equation for PSD: it is well known spectral analysis

282-285: this is true but rephrase the sentence

Eq. 6: where does this eq come from? Please introduce it and explain why you present it in the text. The purpose is to compute some debris flow features from PSD? If so, write this. Anyway, previous theoretical and experimental studies indicate that the recorded seismic frequency depends on the source-to-receiver distance, which controls the recorded peak frequency (Tsai 2012, Kena 2015, Lai 2018, Belli 2022). The debris flow is a white source emitting almost all frequencies in the range of 1-100 Hz. What you record depends on the propagation effects. Therefore it seems unrealistic to me using PSD to get info on flow parameters as the recorded frequency depends only on signal attenuation and results the same for different DFs recorded at the same site (Belli et al. 2022).

Section 3.4:

292: seismic energy and velocity?

295: energy loss is h? write this

300: what is Γ? Please introduce the equation.

303-304: I don't understand this sentence

Section 4:

312: processing raw data? How?

End of 314-315: move this line at 312

317: figure 5?

321: instrument layout?

322: how do you calculate SNR?

330: specify that you limit the analysis at the second event just for the Fotangba Gully; you also analyze the Er Gully event, right?

340-341: rainfall is a common trigger for debris flow. Your conclusion is reasonable but discuss it in the framework of the state of the art: our findings agrees with …

Figure 4: put circles with numeric labels on the 2 events in Fot. Gully toin (a) and (c) make easier to see the two events: it is not clear that a and c shows 2 events. You can also state this in the caption

4.1.2: I am not an expert, but this procedure to recover the original signal does not appear reliable to me and seems too simplistic approach. In addition, it is not clear to me how the propagation effects are cancelled. Did you also account for geometrical spreading? Also, you use σ^2 computed at 1 Hz but the seismic signal are mostly above 10 Hz (Figure 5). Here equations should help: Signal restored=signal recorded * h?

354: reference to Marchetti et al. 2019 and Belli et al., 2025 for the extended source

358-359: as I understand you use a total of 50 m, so it should be 25 m upstream and 25 m downstream. Anyway, limiting your procedure only to this short channel section seems incorrect to me. As you too wrote, a debris flow is an extended moving source and what you recorded is the result of the contribution of the signal components produced in different sections of the channel and in different times. The signal produced in the short section in front of the seismometer almost only corresponds to a short duration window of the all signal. You use the parameters of this section to reconstruct the all signal which is mostly produced in several other channel portions.

380-386: I don't understand this paragraph

389: why are attenuation at station 1 and 2 so different? Why is it larger at 2?

Figure 5: adding labels with "Fotangba Gully" and "station 1" and "station 2" in the corresponding plots should help a fast understanding of the figure

410-411: trivial

433: rephrase as: "to investigate the seismic manifestation of the evolution of the second debris flow…"

441: not clear if the frequency passed from 8 to 43 Hz or if it is between 8 and 43 Hz. Clarify this.

447: the seismic peak amplitude phase reflect the passage of the debris flow front in the closest point to the sensor (Marchetti 2019, Walter 2017, Belli 2025, Coviello 2019), and not an increased

magnitude of the event. The source is moving along the channel. The boulder rich front dominates the seismic signal.

450: 21.6 and 28.6 Hz. Show the PSD to see these values.

449-451: “potentially due to varying particle impacts and scale”: more likely the varying peak frequencies reflects variations in the source-to-receiver distance: the closest station should show the higher peak amplitude (Tsia 2012, Kena 2015, Belli 2022). Belli 2022 clearly showed that the seismic peak frequency of several events is the same regardless of flow parameters.

467-468: but you should have removed the propagation effects, no?

Figure 7: if I am correct, these plots are already shown in Figure 5, why do you repeat them?

Section 4.2: More details are needed: do you compute the x-corr on the entire signal duration or on subsequent signal windows (specify this in the Method, see comments above)? It would be nice to see a plot of the x-corr on a time-lag (XY) diagram (like those in Ichihara 2012 or Belli 2025): this could enable to see variation in the flow velocity if a varying lag is observed through time.

477: suggestion: Debris flow velocity “estimation” instead of “analysis”

480-483: remove this, repetition (478-480)

484: Comiti et al., 2014 perform cross-correlation on flow depth measurements

500-502: rephrase this

500: what is this? RMSA (root mean square amplitude)? Amplitude envelope? Explain this and add it to the method section if necessary.

508: what values of tau and r you used to get 38.3 km/s?

Figure 8: show a plot of cross-correlation though time (like those in Ichihara 2012 or Belli 2025): this could enable to see variation in the flow velocity if a varying lag is observed through time.

519-520: show the Manning formula. I don’t know if this is applicable for debris flows, where the extreme particle transport affects flow dynamics, or just to water flows.

Table 3: why are the manning formula values missing for event 1 and 3

Section 4.3: rephrase the title

537-540: rephrase the period

Section 4.3.1: this section is too long and results boring and difficult to follow. Shorten it keeping only the key aspects.

573-578: you state that images match seismic data: but with seismic data you just reconstruct several surge and gave a velocity estimate so far. No other info was retrieved.

599: bottom? Isn’t it apex?

603: how can a 4.3 kg sample be representative of a debris flow deposit where several boulders up to a few meters in diameter are present? What about bigger rocks, which also dominate the seismic signal? I think this granulometric analysis is biased by the sampling method. You sampled the matrix not the deposit. What is important for the seismic signal are mostly the larger boulders (Kean 2015, Walter 2017, Coviello 2019, Marchetti 2019 ecc).

Section 4.3.3: PSD in Figure 12 a are really smoothed for being computed on raw seismic data. How do you computed them? Signal window of analysis? Smoothing applied? Specifiy this in the methods. The D values are too low for a debris flow a not representative of the seismic source. It is not corrected using particles with a maximum of 2.5 cm diameter for computing the PSD of the seismic signal produced by a debris flow. I suggest repeating the analysis with more senseful D values.
Also the recorded seismic frequency only depends on propagation effects: using it to get information on the particle size or velocity is highly speculative and incorrect. As I said before, I don't think that the signal correction you performed eliminated the propagation effects from the signals.

The results are highly speculative and not supported by independent evidences.

627: What are time points?

628: whose infrared images?

633-636: it seems to me that the PSDs show almost the same trend at the different times: only the amplitude changes. High and low frequencies show the same trend (the one you described for the low frequencies).

642: these grain sizes are unrealistic for a debris flow and result from a wrong sampling method (see above). In addition, the seismic signal is dominated by the larger boulders. Computing the PSD using diameters up to 2.5 cm is not representative of the most energetic seismic source in DFs. Indeed you get PSD at -110 dB compared to the recorded one which is at -70 dB.

654: which one? The real of the synthetic one? Specify

654-655: highly speculative

659: the same is observed for high frequencies!

662: the recorded peak frequency results from the source-to-receiver distance

669: the variation is very small (2-3 Hz) and I don't know if it is representative of the source or depends on data analysis.

670: this is not true: generally, as velocity increases, it increases the diameter of the transport particles, if still available, because the flow has more transport capability.

Figure 12: (a): not clear how these curves are computed (signal window, smoothing…). (b) not clearly readble: I suggest to make 2 different plots: one for the different D, the other for different velocities.

682-684: you can't say this, you have no evidence.

683-684: contradiction: flow velocity decreases and later first increases and the decreases.

721-724: this is true, however you use preliminary results to extrapolate detailed information on the debris flow, with no strong independent evidences supporting your conclusions.

748-750: I don't understand this sentence

763-765: numerical stability: I think a better explanation should be given.

768: your work does not demonstrate anything for the real time monitoring.

<u>Some technical corrections (all text requires language revision):</u>

Line 26: loss TO life…

Line 29: remove "to electricity and batteries"

Line 31-32: repetition: monitoring … monitoring

Line 39: change “reconstruct” with “characterize”

Line 60-61: remove “early warining”, “and evolve”, “based”

Line 70-71: rephrase

Line 79-80: rephrase

Line 97-98: rephrase

Line 99: change “movement” with “debris-flow”

Line 104: “while” instead of “and”

Line 105: “localization” instead of “location”

110: rephrase as “…propagation distances, and therefore are often recorded only by close-range instruments (Zhang 2021).” Specify how close.

117-118: rephrase as: “Unlike landslides, debris-flow seismic signals lack significant low-freq. features, making remote monitoring impractical”

119: rephrase as “and their source processes is still limited”

130: rephrase

131: “consists” instead of “is compised”

133-135 rephrase

639: “We comptuted theoretical seismic PSD…” something like this

717-719: rephrase