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Discussions

Interactive comment on "Rapid Detection and Location of Debris Flow Initiation at Illgraben, Switzerland" *by* Fabian Walter et al.

Anonymous Referee #1

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This study uses relative time-averaged seismic amplitudes recorded on a 9-station seismic network around the Illgraben torrent to detect and track a debris flow event and estimate it's relative seismic strength over time. There is an existing warning system and scientific observation system in place that uses observations on different instruments at check dams along the channel. The authors frame this study as a new approach to debris flow warning systems that overcomes some of the challenges of the existing warning system, for example, by allowing stations to be installed away from the channel in less difficult terrain. This is an important topic, as existing methods have limitations and don't take advantage of recent advances in seismology.

However, I do not feel that the paper is ready to be published. It's an interesting demonstration of a technique for tracking moving flows that is moderately successful at tracking a flow, but for a single flow that was known about beforehand and well-characterized

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by other data sources. The technique applied is not new, yet the paper is framed as if it is, and they do not get into nearly enough detail regarding the limitations of the proposed method despite being unusually well suited to because of the trove of data from other existing instrumentation. In my opinion, in order to be publishable the paper needs to 1) be reframed in context of other studies that have used similar techniques and with a more modest/realistic approach to how these methods could be used in warning systems, 2) include more analysis to convince the reader of some of their claims and to illustrate limitations, which I detail below and in the specific comments, and 3) to include more specifics about the choices they made, why, and how those choices affect the results.

Regarding point one, the authors apply methods that have been used elsewhere for similar purposes (e.g. Kumagai et al. 2009, several references in Walsh et al. 2016 - refs at end), yet these studies are not even mentioned in the paper. The authors also do not seem to be aware that the volcano community has used acoustic flow monitors to detect lahars/debris flows relatively reliably for decades. Granted these systems have many of the same downsides as the system in place at Illgraben, but they still should be acknowledged. See links to some such studies listed at the end.

Regarding point 2, in other studies that have used similar methods, amplitude correction factors are derived for each seismic station to account for variation in site response. In the present study they skipped this step, stating that it was not needed because they were able to get a good solution without it. However, they did not convince me that it isn't necessary. Their method is not able to track the flow reliably after the initial few minutes (Fig 9), this is even more apparent in the supplemental movie. This could very well be because they do not correct for station response - some stations may have higher amplitudes than others in the frequency band used due to their specific site conditions, which would bias the location towards those particular stations. The authors should either add in the station corrections, or make a more convincing case demonstrating that it isn't necessary because it is hard to believe it isn't.

As for the decay fitting, the authors show that the variance reduction can be just as high when fitting noise as when fitting signal. This is not surprising because they are doing a grid search so they are basically rearranging the data points in many different distance configurations for each time step. The chances of having a decent fit somewhere are quite high. So they depend on the amplitude at the “source,” A0, to differentiate noise from signal. However, I have personally tried something similar and found that A0 was extremely variable and highly dependent on the particular data points being used, especially if there weren’t many data points close to the source. Excluding one or two outliers could drastically change the results. I would be much more convinced that their source strength estimations were providing a reliable way of discriminating signal from noise if there were some sensitivity analysis included. For example, the authors could use the jackknife technique to show how much A0 can vary by randomly excluding some of the data and redoing the fit many times. And/or they could run their algorithm for a long time period (they state they have 100 days of data on these stations...) and see how often they get false alarms based on the A0 thresholds they found for the known event.

Another thing that would be interesting to see and would be very relevant for the application of amplitude location methods to this type of seismic source (but may be beyond the scope of this small paper) is an analysis of what the solution looks like if there are two or more simultaneous sources, such as from multiple surges, or an elongated source. Can the technique differentiate between two sources? How far apart would they have to be? What does a pinpoint source look like using these methods (analogous to an array response function for array techniques)?

Regarding point 3, the authors leave out critical details (particularly on page 8) regarding why they made the choices they did in their implementation of these methods, what those choices were (actual values for things like seismic velocities, Q, window length etc.), and how varying those choices affected the solution. Without any information given, how do we know that they didn’t just turn knobs until the method located the event near where they knew it should be for part of the flow?

Specific comments:

P1-L18-19: It's not clear to me that the author's alternative solves this challenge. The seismometers still have to be installed in steep terrain and still have to be telemetered.

P1-L21-23: This implies that geophones aren't seismometers. What is different about the author's method is the algorithm, namely that it doesn't depend on the vibrations detected right next to the channel. This should be clarified.

P1-L24-27: Acoustic flow monitor-type detection systems also use time-averaged ground vibration amplitudes, just the ones right next to the channel, and do not rely on single station detections. The authors should clarify what is actually different about their method.

P1-L29: This implies that they applied this algorithm in real time, but from what I gather, they did this analysis long after the event occurred and was already characterized. For example, it is stated later on that the data was not even telemetered. Would the outcome have been so good in real time without prior knowledge of the existence of the event and with all the delays and complications of telemetry that the existing system already has to deal with?

P2-L22-23: The number quoted for maximum ground velocity surely isn't the highest of any debris flow ever, I would change this sentence to "ground motions of up to 2e-3 m/s have been observed. . ." Also, observable frequencies near the channel are often much higher than 100 Hz, acoustic flow monitoring systems often look at bands of several hundred Hz. For example, see Marcial et al. 1999.

P2-L29: I don't know if I agree that no reliable implementation for debris flows has been found. The authors describe one in the next few pages, and acoustic flow monitoring setups have been pretty reliable at volcanoes, though they certainly could be improved. The method proposed also requires site-specific parameter tuning (seismic velocity structure, station amplification factors etc.), so that's still a factor. Also, the use of the term "single station detections" is misleading. To my knowledge, none of the existing methods are single station systems. They depend on time-delayed detections on multiple stations along the channel, including the method currently in place at Illgraben.

P3-L4-5: The phrasing of this sentence implies that moving the instruments away from the torrent decreases the influence of site effects on ground motion, I don't see why this would necessarily change anything regarding site effects, the new site will also have site effects. Increasing the distance also adds another challenge, path effects through an unknown subsurface structure.

P5-L29: If the stations had sample rates of 125 to 200 Hz, as stated above, you would only be able to see up to the nyquist frequencies of 63 or 100 Hz, respectively. . .the spectrogram shown in Fig 4 is from a station that Table 1 says was sampled only at 125 Hz, so we should only be able to see up to 63 Hz yet the spectrogram goes up to 100 Hz. This is not informative about the upper limit of the frequencies observed; there could be and probably are higher frequencies present.

P6-L8: The two papers referenced are about bedload in river flow, NOT debris flows, though the processes described may be similar between them seismically. Regardless, this should be made clear in the text to justify.

P6-L20-21: I'm not sure panel a is actually showing relative amplitudes as the text implies. I think it may be scaled to the maxes. The relative amplitudes don't look the same in panel b as where the red line is in panel a. For example, IGB09 looks lower in amplitude at the red line than IGB07 in panel a, but the opposite in panel b. Also, the Rhone Valley stations mentioned in the text don't have the highest amplitudes in panel a, other stations look just as high.

P7-L12: Signal coherence depends on how close the stations are to each other and the frequencies of interest, if the stations were actually in an array configuration designed for the frequencies of interest here, the signals very well could have been coherent.

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The statements here imply that no debris flow signals are coherent ever.

P7-L18: Kumagai et al. 2009 should be referenced here. They used similar methods for essentially the exact same purpose you did, just on a much larger scale. Others have used these methods for debris flows and other surface flows, see Walsh et al. 2016 and references within.

P8-L10-11: Station corrections should not be neglected in my opinion, see main comments.

P8-L13-L25: Many critical details are missing here regarding why the authors made the choices they did, what those choices were (actual values), and how varying those choices affects the solution (see main comment above). More specifically, why did they decide to assume body waves? Body waves are not going to follow straight line paths, surface waves would (approximately). Some people argue that the strongest waves from surface flows are surface waves. Do the authors have evidence one way or the other here? Could using surface waves produce a similar result? Third, are they assuming P or S waves? Why? What velocity are they using? Why? Why did they choose 100 sec windows for amplitude average? What are the values of alpha and Q that they use that are "within the range expected for body waves near the surface of the earth"?

P9-L14-15: Since source strength is their best way to distinguish between noise and debris flow signals, a time series of source strength solutions should be shown on the movie and fig 6 and 7 in a similar way to how the seismic data is shown so we know how to interpret the solution at each point in time.

P9-L24-26: This statement lacks evidence. It could just as easily be because they aren't correcting your station amplitudes and that may inflict more of a bias on locations further downstream than upstream. Either show evidence for the statement here or show that the amplitude corrections really don't make a difference.

P10-L16: How do the authors exclude these stations to avoid affecting the decay fit?

P11-L16: It would be more convincing if the authors tried adding white noise comparable to what is sometimes seen, for example, during a storm, to the signals during the debris flow to see how it actually affects the decay fit scheme.

Figure 3: The geophone data looks processed in some way, are these envelopes of the amplitude data? Time averaged absolute values of amplitude? Why is the geophone signal flat before the arrival of the debris flow, is the instrument turned on by some sort of trigger? The label "geophone impulses" is vague in meaning. Please clarify these things either in the text (page 5) or in the caption, or both.

Fig 6 and 7: A0 for the fit at the initiation is lower than A0 for the noise window – this is confusing because the text implies that the A0 value was the main way they were able to differentiate between signal and noise (e.g., Fig 8)

Minor/editorial comments:

P2-L18: Debris flows do not always move so slowly, I would add a qualifier like "debris flows typically move at. . ."

P3-L2: The Arattano 1999 reference is missing from the reference list

P4-L5: What type of material are the slopes made of?

P4-L19: What is an "instrumented wall"?

P4-L20-24: It would be nice to have a map of this layout or photos to help visualize the setup.

P5-L12: Is this time local or UTC?

P9-L20: Are the authors projecting to the channel? Or is this the rate the best fitting location moves regardless of channel location?

P9-L24: lacks -> lags

Figure 1: Label the check dams on the map so we know which is which. Also, an inset map showing where Illgraben is in Switzerland would be helpful.

Figure 2 is not mentioned anywhere in the text.

Figure 9: Is distance referring to distance along the channel? Starting from where?

References:

Kumagai, H., Palacios, P., Maeda, T., Castillo, D., Nakano, M., 2009. Seismic tracking of lahars using tremor signals. J. Volcanol. Geotherm. Res. 183, 112–121.

Walsh, B., A.D. Jolly and J. N. Proctera, 2016, Seismic analysis of the 13 October 2012 Te Maari, New Zealand, lake breakout lahar: Insights into flow dynamics and the implications on mass flow monitoring, JVGR, vol 324, pg 144-155.

Links to some acoustic flow monitor papers:

Marcial et al. 1996, Instrumental Lahar Monitoring at Mount Pinatubo, from Fire and Mud, available at: https://pubs.usgs.gov/pinatubo/marcial/

https://pubs.usgs.gov/pinatubo/tungol/

https://volcanoes.usgs.gov/volcanoes/mount_rainier/mount_rainier_monitoring_99.html

http://www.sciencedirect.com/science/article/pii/S0377027300001517

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