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Comment on nhess-2022-31

Anonymous Referee #2

Referee comment on "Characterizing the evolution of mass flow properties and dynamics through analysis of seismic signals: insights from the 18 March 2007 Mt. Ruapehu lake-breakout lahar" by Braden Walsh et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-31-RC2, 2022

This observational study investigates along-channel seismic signals of a lahar at Ruapehu caused by an outbreak event in 2007. The study focuses on using all three components of the data to investigate changes in peak frequency and directional energy in the high frequency energy and makes some interpretations about what the observed patterns mean about flow style and how it evolves as the flow progresses down the channel. The main takeaway the authors want to get across seems to be encouraging the use of all three components of seismic data instead of just the vertical component. The authors also present a conceptual model for what changes over time in flow style they think are responsible for their observations at each of the three stations.

My overall assessment is that the study is definitely an interesting additional datapoint to add to the lahar seismology literature and the directionality patterns and frequency changes over time are compelling. However, I found the discussion and interpretation to be quite speculative and many of the observations have been made in other papers at other sites already so this paper is not necessarily groundbreaking but does add more information to the literature. I don't think the study makes good use of the other non-seismic data that was collected and their study could be strengthened and made less speculative by doing so (e.g. they don't link their ideas about changes in flow behavior to the time-lapse photos, and on line 138 they mention many data types that aren't used in the study but seem like they would be extremely useful for understanding changes in flow style). The part of the paper that does use some other non-seismic data, Figure 7b, is barely discussed in the paper despite being one of the most interesting observations. That alone could certainly be emphasized more. The conceptual model is definitely credible and reasonable, though not especially strongly or uniquely supported by the observations so it's more of a hypothesis/discussion point.

I do have one major concern with the basic analysis though that should be addressed before the paper goes further, and that is the lack of any investigation into the site amplification due to the subsurface structure below each monitoring site that may be

influencing the peak frequencies of the seismic data. I suspect the stations likely do all have strong site amplification at specific frequencies because the sites are probably installed on past lahar or alluvial deposits that overlie some older higher velocity materials which would give a good impedence contrast. Since the time window prior to the arrival of the lahar has the same peak frequency as the flow itself for many of the cases, I suspect that it is very possible that the peak frequency might be controlled by the site effects at each station more than the source. This may also explain why we see some jumping between two discrete frequencies, I suspect those sites where that happens may have two peaks in the spectral response due to multiple layers of contrasting sediment under the site. Differences in spectral peaks between the two horizontal components can indicate 2D/3D subsurface structure so it's also possible any observed differences between the components are not due to the flow itself. Site effects and their influence on the observed peak frequencies needs to be investigated and accounted for before any detailed interpretations about frequency content can be made. For example, the authors could use H/V analysis (see Molnar et al 2022 https://doi.org/10.1007/s10950-021-10062-9 for a recent review), spectral ratio analysis using a nearby reference site on bedrock if there is one, and/or an analysis of the noise patterns at the sites over longer periods of time.

Along the same line, I also have concerns about the robustness of the frequency peaks. No information is given on how those are computed. Were they picked off a raw FFT (which is often quite noisy and unstable in exact peak locations)? Or was smoothing done prior to picking the peaks? Was any analysis of the robustness of the peaks and their stability done? Some of the temporal patterns, especially at COLL, are very subtle and I am not sure they are statistically significant, at least as presented. More detail needs to be added. Some more robust ways of estimating the frequency content to consider would be perhaps using the multitaper method to compute the spectra on which the peaks are picked, or using the dominant frequency (e.g., see Douma and Sneider 2006, https://doi.org/10.1111/j.1365-246X.2005.02807.x), or another weighted or smoothed method of estimating a more robust spectral peak. Another option to look at would be instantaneous frequency, which can be obtained by the Hilbert transform.

Also, especially in the discussion section but also earlier, I feel like the paper is lacking an explanation for why, physically, the different flow styles would be reflected differently in the directionality and frequency peaks. There are many speculations, like, to give one example "The reason DR decreases…could be due to the parallel component being more sensitive to flow processes than bedload forces" but I want to know why, physically, would the parallel component be more sensitive to flow processes than bedload forces? Is it a difference in the wave types they generate? The forcing directions? There are many places where I want to know the physical reasons behind why a given pattern is being invoked as an explanation for flow characteristics. Why would "underflow" cause lower frequencies? Why would a low frequency occur when a bow wave is passing? Why would the angle of the slope cause higher frequencies? Why would the vertical component be higher when bedload dominates? And so on…

Other comments:

I don't see any information on how the data used in the study can be accessed, this is necessary for a reproducible study.

L61-63, a lot of these things are true of seismic instruments as well, and there is more ambiguity in interpretation for quantitative values. I also disagree that they can be used for "accurate" (L66) estimates of flow properties. Only in very limited situations is that true.

L69-70, using seismometers for flow monitoring is not young, it's been used operationally since the 1990's and there are even earlier papers that use seismometers for mass movements.

L77 previously not recorded by who? People have used three component sensors for recording lahars and other mass movements many times in the past. Maybe rephrase as "information about the flow that is not recorded if only vertical-channel sensors are used"

L88-89, perhaps it would be useful to explain what you mean by terms like plug-like and laminar here and elsewhere in the paper, many debris flows are still very agitated if not turbulent, is that still called plug-like by your definition?

L133 missing "the" and missing comma

L133-134 Please explain how velocities stated here and elsewhere in the paper were measured

L157 Same as above, and also please explain what the averaging represents, is it time averaging over the entire flow? Spatial averaging?

L166-167 Please give detail as to how the peak spectral frequency was estimated (and I assume the authors didn't mean peak spectral frequency amplitude but just peak spectral frequency since they don't present spectral amplitudes anywhere, so remove the word amplitude if that is true).

L169 Please explain how the arrival time is known, is this from the cameras? It doesn't seem to be from the seismic records based on later figures and discussion.

L180-181 How do you know this is the arrival of the head for the lahar? Is the definition of the "head" that time when the peak amplitude occurs or is it determined independently from the cameras or flow height sensors? I also don't know why the word streamflow is in

parentheses here or what that is meant to convey.

Figure 4: it's interesting that there is an upward sweep of frequency on the vertical component, but it jumps discretely on the horizontals. Any idea why?

L232: rephrase this sentence, such as "installed so that the North component of the sensor is instead aligned to be parallel to the flow"

L234: but site effects cannot be ignored, see my main comment

L236: give details on how the energy was computed, over what frequency etc. and how directionality is computed for reproducibility

L237-241: Add some information here about physical reasons why the directionality would contain information about rheology changes and other flow parameters.

L265-266: I don't understand what multiple pulses has to do with bulking material that is different from collecting material from erosion, do the authors mean more pulses of material bring more material down and the flow fronts coalesce into one bigger one? Perhaps this sentence could be rephrased?

L284: "The ~10 Hz PSF may be explained by flow processes" is a very vague statement, can you be more specific?

L305: Here or wherever it was first introduced, can you explain what is meant by a 4-phase lahar? I don't think that is common enough to not define.

L364-365: here and elsewhere, are the speculations about flow style corroborated by the camera images and/or other data types that was collected?

L405: Unclear what is meant by "at different distances away from source" – this study did not investigate changes with distance from the source so how does that come into play? Maybe this statement needs some clarification?

L435: This statement and the supporting evidence is one worth emphasizing more in the paper.

L444-446 and Figure 9, this discussion makes me wonder what the directionality looks like for long time periods of noise and earthquakes and whatever other signals the station records when a lahar is not passing by. Is the change in directionality unique to when a lahar is passing by? Could it be differentiated from other seismic sources?

L464-466 tilt is usually at much lower frequencies than was the focus of the investigation here. Since no details were given on how the energy was computed for each component (on raw data? Filtered? Etc.) it's hard to assess whether this would have a substantial influence or not without more information.