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Interactive comment on "Validity, precision and limitations of seismic rockfall monitoring" *by* Michael Dietze et al.

C. Hibert (Referee)

hibertclement@gmail.com

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Dear Authors, dear Editor,

This study focuses on the seismic detection and location of small rockfalls that originated from an unstable cliff of the Lauterbrunnen Valley in the Swiss Alps. The Authors show that they are able to accurately locate some of the rockfalls they detected. They also show that some qualitative information on the dynamics of the events can be inferred from the recorded seismic signals.

Using seismology to detect and study rockfalls, and more generally gravitational instabilities, is challenging but constitutes a high research priority to improve the completeness of catalogs and the assessment of hazards associated with slope failures. In this context, I think this study can become an excellent contribution to promote the use of seismology for rockfalls studies. I found the manuscript clear and well-written but I identified several minor issues that have to be addressed. I also have few suggestions that might improve the overall impact of this study. Below are my comments and those suggestions.

Best regards,

Clément Hibert

Comments and suggestions:

Title: When first reading the title of the paper I was expecting an analysis of the feasibility to study rockfalls with seismic methods in different contexts, or an enriched review of past studies on this subject. ‘Validity’ and ‘limitations’ of seismic rockfalls monitoring in general are not discussed in this paper and I find that what the authors propose is essentially an interesting case study. This should be explicit and clarified in the title. I would suggest for example: “Validity, precision and limitations of the seismic detection and location of small rockfalls in the Swiss Alps”.

P2 L17: Please order the references chronologically. There is a wealth of studies on landslides seismic signals. If you decide to select some of them as examples, use “e.g.” before citing them.

P2 L18: While it would be an honor to share the name of David Hilbert, I am not. Here and everywhere else please correct the references to “Hibert et al.” (no “L”).

P2 L21: “cf” not necessary here or elsewhere.

P3 L14: Is this different from spectrograms?

P3 L19: Burtin et al. [2016] were not the first to show that seismic signals generated by rockfalls are dominated by surface waves. In the references here you can add Deparis et al., [2008], Dammeier et al., [2011], and Levy et al., [2015].

P3 L22: “Vital” seems a bit strong. Interesting? Significant? Crucial?

Section 3: Is this section necessary? Could you move this into the introduction?

P3 L28: What is the “limit of detection”? Is it the targeted (or the possible?) resolution of the point clouds?

P4 L3: Are the seismometers 3 components? Please add this information here.

Figure 2 – caption: Do you know the volume of this particular event? If so this information could be added in the caption. Are the signals filtered?

P6 L17: The STA-LTA ratio picker was first proposed by Allen [1982].

P6 L18: Envelopes of seismic signals are commonly computed from the Hilbert transform of the signal. I think using the absolute amplitude is not a problem for detection, but for a localisation method based on the cross-correlation of envelopes, the Hilbert transform might yield better results. While I certainly do not think it is necessary to redo the current analysis with Hilbert envelopes, I would suggest testing this in future studies.

P6 L24: Can you indicate here what are the threshold values chosen?

P7 L1 and 11: You choose here a velocity for S-waves but as stated before rockfalls seismic signals are dominated by surfaces waves (which are slower than body waves). How many events have you excluded based on this criterion?

P7 L10-11: This is a bit confusing. You first had an automated exclusion criterion based on the time delay between the onsets of the waves recorded at each station of the network and then you still check manually if this criterion is verified? What is the point of the first automated exclusion then? Maybe reorganize this paragraph and the one just before to improve clarity.

P7 L13-14: Criterion iv) : Does this imply that you know the location of the events before manually selecting the signals?

P7 L17: References : “e.g.” or add at least Surinach et al. [2005].

P7 L20: “Multiptaper” Typo?

P7 L21: References : “e.g.”. Hibert et al. [2011] and Dammeier et al. [2011] seem more appropriate references here.

P7 L29-34: The approach proposed by Hibert et al. [2014 and not 2011] is designed to overcome all the issues regarding the specificity of rockfall seismic signals you enumerate before this sentence (emergent onset, waveform discrepancies, absence of seismic phases, high-frequency). Moreover if the dominant issue is the “differences between waveform properties at different stations” the cross-correlation approach would not work. In your case you can use a method based on the cross-correlation of the signal waveforms because the signals recorded at different stations are not too dissimilar. I suspect this is the case because the aperture of your network is not large (inter-station distances of $\sim$1km). This is not the case at the Piton de la Fournaise volcano and is one of the difficulties that forced us to develop a new kurtosis-based first-arrival picker that is accurate enough to pick emergent signals. Please rewrite this paragraph by taking this remark into account.

P8 L19-21: Topography correction is necessary because rockfalls generate surface waves that propagate following the topography. Also the correct reference is Hibert et al. [2014] and not [2011] here.

P8 L8-18: If you change the frequency range used to find the time lag that yields the best cross-correlations this should have an impact on the optimal velocity, and vice-versa. Can you elaborate on the interdependence of the optimal frequency bands and the optimal velocities found? It can be interesting to add in Table 2 the velocity that gives the best location for these 10 rockfalls.

P8 L21-22: Not all Earth surface processes generate seismic signal dominated by surface waves. I suggest to change “other Earth surface processes” into “other mass-movement processes” or “gravitational processes”. Also see comment on P3 L19 regarding the reference to Burtin et al., [2016].

P8 L22-25: It is not clear how you performed this correction. What is: “that part where direct distance is above the actual surface elevation”? Does this mean that you corrected the direct straight line distance from pixel to pixel by the slope angle? Did you compute profiles for each pixel-station pair from the intersection of the straight line between those two points with the grid points of your DEM? Integrating the topography in propagation maps is not a trivial task but as you mentioned is critical to have accurate locations of rockfalls. This should be a bit more detailed, especially if the main focus of this paper is the capability to locate rockfalls from the seismic signal they generate.

P8 L30: Please define what is the “likelihood quantile” before.

P9 L26: What caused this tilting? Do you know when it started? What is the influence of this tilting on the seismic signals recorded before dismantling those stations?

P11 L16-18: I am a bit sceptical regarding the “rain drop sources” because you have buried your stations at 30-40 cm depth. This should prevent any direct contact between the seismometers and rain drops and I think rain drops are too weak seismic source to generate signals that would not be attenuated in the first few centimeters of propagation. Other common sources that can generate impulsive signals with energy in high-frequency bands are thunder, numerical glitches or close footsteps (animal or human). You based your attribution of those signal to rain drops from the observation that “it only occurs in the records when it was raining in the Lauterbrunnen Valley during deployment and maintenance of stations”. So you observe these noise signals on the days you were on the sites. There is a possibility that these signals are your footsteps, but without clear evidences we do not know. So did you observed those signals on days where you were not on site? Can you provide other arguments to attribute those signals to rain drops? For example, did you observe that those signals appeared and disappeared gradually over a period of times of several tens of minutes (or few hours), mimicking the passing of scattered showers? If so could you show this on a figure to definitely convince your readers that those signals are indeed generated by rain drops? If not I would suggest to rename this class of source to “impulsive noise”.

P14 L3-4: What is “n”? What is “r”?

P14 L4: If you want to provide to the readers an analysis based on the SNR you need to indicate how you have computed this quantity before.

P17 L13-14: What are those relationships? Do you refer to the studies of Hibert et al. [2014], Manconi et al. [2016]? Dammeier et al. [2016]?

P17 L20-21: Levy et al. [2015] used a different approach (first-arrival picking and not cross-correlation back propagation as in the present study) and had a network with a much larger aperture (inter-stations distances of few kilometers).

P18 L7-9: I agree with the assumption that a rockfall with a higher volume should generate a higher-amplitude seismic signal if the travelled path and the fall height are the same. However you say latter that in your case there are no correlation between seismic energy/amplitude and the volumes of the events.

P18 L12-13: The volume of the rockfalls in the study by Hibert et al. [2011] had volumes as low as few cubic meters.

P18 L18-19: Indeed, you are working with complicated events and I acknowledge that extracting quantitative laws might be difficult in this case. However, as shown by the example discussed in section 6.3, you are able to identify the different stages of the rockfall propagation. Is this true for the 10 rockfalls in your database? If so, you have every information you need (location of the events, volumes/masses, average velocity of the medium) to go further in your analysis. For example, what are the relationships between the first impulsive arrival amplitude (corrected from propagation effect) (phase 1) and the volume? The relationships between the seismic energy and the potential energy lost at the first impact with the topography (phase 2) ? The same relationships during the propagation phase on the talus (phase 3)?

Those are fundamental issues that you might be able to contribute to answer with your dataset. Even if no relationships are found, this would still be very interesting as it will nourish discussion on the validity for small rockfalls of the relationships found by others [e.g. Deparis et al., 2008; Vilajosana et al., 2008; Hibert et al., 2011; Dammeier et al., 2011; Yamada et al., 2012; Ekström and Stark, 2013; Farin et al., 2015; Levy et al., 2015; Hibert et al., E-Surf- in press]. I understand that this might be out of the scope of this study, but I think that adding this deeper analysis will significantly improve the impact and the reach of your paper.

P18 L31: references : add “e.g.” and/or other references, for example : Helmstetter and Garambois [2010], Yamada et al., [2012], Zimmer & Sitar., [2015], Hibert et al. [2017].

P19 L3-5: While it seems reasonable to think that large mass detachments are preceded by cracking and fracture opening that generates an increasing rate of micro-earthquakes, this is more debatable for very small rockfalls such as the ones in this study. Another assumption to explain this first impulsive signal is that it is generated by the rebound of the Earth in the departure zone due to the detachment of the mass. This was observed at Piton de la Fournaise volcano [Hibert et al., 2011]. I think both assumptions should be mentioned here.

P19 16-17: If larger particles have higher momentum they will reach the bottom of the slope more rapidly than small particles. In fact this is what is observed in many cases on video recordings of events: large blocks preceding the flow of small granular materials. The loss of high-frequency at the end of seismic signals generated by gravitational instabilities is complex and still not yet fully understood. Analytical models [e.g. Okal, 1990; Farin et al., 2015] suggest that events with larger volume will indeed generate signal with a lower corner frequency, but the overall amplitude of the signal across the whole frequency range will be higher. To this adds the fact that high-frequencies generated by small particles are more attenuated. The combination of those two processes suggests that the loss of high-frequency at the end of those seismic signals is due to the early immobilization of the largest particles, not the smallest. This is highly speculative, and any interpretation of this frequency shift has to be done carefully and supported by data. If you want to comment on this, please add references.

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Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/esurf-2017-12/esurf-2017-12-RC2-supplement.pdf

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