

Response to referee comments

[Seismic monitoring of small alpine rockfalls validity, precision and limitations]

July 13, 2017

We would like to thank the referee for the encouraging and helpful comments, all of them obviously devoted to improve the quality and impact of the manuscript.

Referee 2.1: *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”.*

Reply: We understand the arguments and changed the title (almost) as suggested.

Referee 2.2: *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.*

Reply: All reference lists were checked for chronological order and corrected where necessary. The term “e.g.” was inserted as suggested.

Referee 2.3: *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”).*

Reply: Indeed, this was an unnecessary bug that sneaked into the tex file. It has been corrected throughout.

Referee 2.4: *P2 L21: “cf” not necessary here or elsewhere.*

Reply: Terms have been removed where necessary/appropriate.

Referee 2.5: *P3 L14: Is this different from spectrograms?*

Reply: The term has been replaced by “spectrograms”.

Referee 2.6: *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].*

Reply: References included as suggested.

Referee 2.7: *P3 L22: “Vital” seems a bit strong. Interesting? Significant? Crucial?*

Reply: Changed to “unique, important”. We believe that this is the best phrase to describe the value of seismic data with respect to the level of detail they can provide in some cases, e.g., as shown in the example figure 2.

Referee 2.8: *Section 3: Is this section necessary? Could you move this into the introduction?*

Reply: We had thought about adding this chapter to the introduction during the writing process but then decided to keep it separate, mainly to adequately set the scope for the entire manuscript: i) “We know that seismic monitoring works for characterising rockfall, but there is a set of unknowns at the moment” and ii) “For those who are unfamiliar with the seismic approach, this is what the data looks like one can record and has to interpret”. Furthermore, this section already presents results of this study, which makes it difficult to include it to the introduction. Thus, we prefer to keep the section in its current form, also backed up by no such impression by any of the three other referees.

Referee 2.9: *P3 L28: What is the “limit of detection”? Is it the targeted (or the possible?) resolution of the point clouds?*

Reply: This term is common jargon among the TLS community. We added a short definition in brackets for comprehension by a wider readership.

Referee 2.10: *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?

Reply: Component information was added as suggested. Released rock volume, event ID and link to table 1 are provided in the caption, now. Filter window (1–90 Hz) is given in the caption, as well.

Referee 2.11: *P6 L17: The STA-LTA ratio picker was first proposed by Allen [1982].*

Reply: Reference was changed as suggested.

Referee 2.12: *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.*

Reply: Absolutely correct. The utilised algorithm used the Hilbert (with 1 this time) transform to calculate the envelope. The main idea in the original manuscript was to provide the unfamiliar reader with a short explanation of the term “envelope”. However, obviously this plan was misleading. We removed the short and wrong definition, now.

Referee 2.13: *P6 L24: Can you indicate here what are the threshold values chosen?*

Reply: Since these values are part of the results, we provide here now the link to the adequate chapter (5.2), where these values are presented and justified.

Referee 2.14: *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?*

Reply: We have added further credit to earlier studies that point at the value of 2000 m/s for land slides and rock falls. Actually, after a test re-run of the approach on a short section of the data base with lower velocities,

all additionally included events were rain drop impacts (based on the short duration of the picks and relation to the meteorological data).

Referee 2.15: *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.*

Reply: Indeed, point i) and ii) are redundant. The initial idea was to have all decision/rejection criteria at one place as a summary. However, this was confusing. We removed these two points.

Referee 2.16: *P7 L13-14: Criterion iv): Does this imply that you know the location of the events before manually selecting the signals?*

Reply: The section was rephrased to be more general, it now simply expresses that the signals are expected to show a significant difference in their amplitudes due to the different source–receiver distances causing attenuation. If the source is inside the network, the differences between source and receiver for all possible station pairs is expected to be much higher than for a source location outside the network, especially if the source is away several times the network aperture, when only site amplification effects may modify the picture.

Referee 2.17: *P7 L17: References : “e.g.” or add at least Surinach et al. [2005].*

Reply: Both suggestions were implemented.

Referee 2.18: *P7 L20: “Multiptaper” Typo?*

Reply: Yes, the typo has been corrected.

Referee 2.19: *P7 L21: References : “e.g.”. Hibert et al. [2011] and Dammeier et al. [2011] seem more appropriate references here.*

Reply: Included/corrected as suggested.

Referee 2.20: *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 1 km). 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.*

Reply: As suggested, the paragraph has been rewritten.

Referee 2.21: *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.*

Reply: The text was changed as suggested and the reference was corrected.

Referee 2.22: *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.*

Reply: As suggested, this section does now discuss the interconnectivity of wave velocity and frequency range used in the location routine. Since we kept the wave velocity constant for the different frequency bands there is limited value in adding it to the data table.

Referee 2.23: *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].*

Reply: Changed as suggested.

Referee 2.24: *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.*

Reply: We added further explaining sentences. The approach is a direct translation of the Matlab based technique discussed by Burtin et al. (2014, ESurf) to the language R and is part of the freely available package eseis.

Referee 2.25: *P8 L30: Please define what is the "likelihood quantile" before.*

Reply: This value is now defined at this position and used throughout the text. P is the location cross-correlation value of a given pixel and the 0.95 quantile is the threshold value arising from all P values of the location grid used to, e.g., clip the location polygons.

Referee 2.26: *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?*

Reply: The (most likely) cause of the tilting is now mentioned in the text. It is hard to say when this started because the TC120s sensors can compensate tilting up to about 10 degrees by using additional battery power but suddenly fail to record further data once beyond this tilting angle. Anyhow, we think this technical detail about the utilised sensor is of limited use for the reader and prefer not to infuse it into the text.

Referee 2.27: *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”.

Reply: We added a further figure showing the co-occurrence of the seismic signal pulses and a rain data record. The data is also interpreted in the text and we argue for the rain cause with respect to passing animals or humans on the base of the irregularity of the signals.

Referee 2.28: *P14 L3-4: What is "n"? What is "r"?*

Reply: "n = 8" has been replaced by "eight cases" and "r" has been removed completely, see comments of referee three.

Referee 2.29: *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.*

Reply: The term SNR is now defined where it is used for the first time (chapter 4.3).

Referee 2.30: *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]?*

Reply: The statement is now supported by some of the suggested references.

Referee 2.31: *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).*

Reply: Corrected as suggested.

Referee 2.32: *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.*

Reply: We rewrote the statement further down to say explicitly that it refers to relationships based on volume, only. In the Lauterbrunnen Valley case we would have to include many more parameters than just rock volume, as explained in the rest of this paragraph. So in summary, both parts are true: in the case where only the rock volume is different while all other parameters (e.g., height, fragmentation, debris entrainment and impact location) are identical, the seismic signal of a larger rock mass will undoubtedly be larger. But when all the other parameters can vary, as well, this energy-volume relationship will fade.

Referee 2.33: *P18 L12-13: The volume of the rockfalls in the study by Hibert et al. [2011] had volumes as low as few cubic meters.*

Reply: The sentence was rephrased and more appropriate references were used, now.

Referee 2.34: *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.*

Reply: Indeed, for some of the events there is a comparably favourable situation as for the event shown in figure 2, but this would reduce the number of suitable cases to about four. We believe this is not a sufficient amount of data to hypothesise about quantitative laws. As suggested by the referee, the topic is out of the manuscript scope and extending the discussion to this theme would inevitably require a significant redesigning process of the entire manuscript, a point we consider not balanced by the number of suitable events that can be used to support claims in the light of this goal. We have however opened the door for the reader to think about this possibility at the end of the paragraph (last two sentences).

Referee 2.35: *P18 L31: references : add "e.g." and/or other references, for example : Helmstetter and Garambois [2010], Yamada et al., [2012], Zimmer and Sitar., [2015], Hibert et al. [2017].*

Reply: Both suggestions were included.

Referee 2.36: *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.*

Reply: Implemented as suggested.

Referee 2.37: *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.

Reply: As this part of the anatomy section is not a central part of the scope of the manuscript we followed the referee suggestion. We removed the speculative part and provide a reference to support the first part.