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Earth Surface Dynamics Discussions

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Interactive comment

Interactive comment on "Spatiotemporal patterns and triggers of seismically detected rockfalls" *by* Michael Dietze et al.

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This manuscript describes seismic signals of rockfalls and discuss possible triggering mechanisms. The part about the detection, location and interpretation of rockfalls signals presents more detailed analysis and additional data compared to their previous study (Dietze et al. ESDD 2017). The second part is very speculative. The limited duration of the catalog and the limited number of events does not always support their interpretation. In addition, no uncertainties are given and no statistical tests are performed, so that it is not clear wether the results are statistical significant or are just due to random coincidences.

The two parts are rather disconnected: we don't need to analyze in details the seismic signals to understand their triggering mechanisms. In my opinion, it would have been better to include some parts of the manuscript into the previous manuscript, and focus this second manuscript on the triggering mechanisms. As it is, the manuscript keeps moving from one topic (analysis of seismic signals) to the other one (triggering mechanisms). It makes the manuscript difficult to read and the message unclear.

1) Interpretation of seismic signals

Detection. p7 l5. I think the time criteria used to detect rockfalls (max duration and time delay between stations) are two severe and may reject true events (see review of previous paper)

p11 l27. Why not looking at the full catalog from the Swiss Seismological Service (not only d<20 km and m>1) in order to classify seismic events? I think that a significant fraction of your 359 events classified as earthquakes should be listed in this catalog, and the earthquake catalog could be useful for an automatic classification of events.

Rockfall volume. For events detected by TLS, could you add the estimated volume in the supplementary manuscript? For the other events, don't you have pictures of the cliff (before and after the monitoring period) that might allow you to detect the largest events and to estimate their volume?

Location. p13. Frequency range. How do you adjust the frequency range: manual or automatic? On which criteria? How do you know if a location is good or not (if you don't have TLS data)?

Figure 3. I do not understand the time interval you use to locate each pulse. Could you add this information in the table of Fig 3 (or in a separate table)? I am surprised that you can separate pulses so close in time looking at correlation between signal envelopes, and with only 4 stations. And that the results seem so accurate! It looks almost too nice to be true. Maybe add a plot showing the envelope of the filtered signal at each station (frequency range used for location), zoomed around time of pulses 0-3?

Organization of the manuscript. Why don't you present the interpretation of seismic signals in section 4, just after the description, rather than in the discussion section 5? When reading 4.2, I was curious about the interpretation of these signals. But by the time I reached the discussion section, I had already forgotten what I read in section 4. The discussion should be more general, and compare your results with previous studies.

Classification of rockfalls I don't find the classification of events in type A (free fall), B (multiple impacts and C (avalanche) meaningful. One event can apparently belong to several classes, the event shown in Figure 2 is classified in type A (p20, l6), but the last part of this signal is of class C (p20, l11). Event III is a combination of types B and C (p22).

The first pulse of the signal is interpreted as an impact (e.g., p20 l18, l21, l24, p21 l15). Don't you think that it could be rather a detachment phase (elastic rebound)? Can you show the cliff profile at the location of a few events, and does the cliff geometry supports the hypothesis of an impact just below the detachment area? p20, l22: Vilajosana (2008) is not a good reference to discuss the detachment process, because this study describes an artificially triggered rockfall.

Location There is not enough information on the location method. p20, l24 : How do you adjust the time window to locate the beginning of the signal? Does your method works with short time windows, so that you can distinguish successive impacts? Don't you need time windows much longer than the time delay between stations (about 1 s)? p20, l26. I don't understand where this correction comes from?

p21 l29. One rockfall is located about 125 m above a preceding rockfall, that initiated 26 s earlier, and is supposed to have been triggered by the impacts of the preceding event. I find unlikely that a rockfall can be triggered above a first event 26s later, especially since the rockfall was probably quite small and the recorded ground velocity not very large (2 microm/s). Could it be a location error, so that the second rockfall initiated next to the previous one or close to an impact zone of the previous event?

5.1.3. I don't agree with the fall heights computed in this section and with the interpretation of the seismic signal. Maybe I missed some explanation or misunderstood something? p22, l17. A free fall height of h= 122m should last t=sqrt (2*H/g) = 4.98 s , not 3.52 s. For h=112, t=4.78s (not 3.38). And for H=795 m, t =12.7 s, not 6.9 s. The total fall time of 6.9 s should correspond to a fall height of 235 m for free fall, but the intermediate impact should have decreased the bloc velocity, so that the fall height should be smaller. This implies that the two phases could correspond to the initiation of the rockfall (phase 1) and to the impact at the base of the cliff (phase 2), rather than 2 distinct events.

p22 l24. I am not convinced that "seismology can provide insights . . . that not other method could achieve". A video camera would provide a more accurate description of a rockfall event (if it occurs during a sunny day).

2) Triggering mechanisms

- Temporal variation of rockfall elevation (Figure 6 and section 5.4) I am not convinced by the results shown in Figure 6 and by their interpretation.

First the figure 6 is hard to understand because the x axis is not continuous (events of 2014 are shown after events in 2015). Replacing the x axis by "day of year" would make this figure easier to understand. But if you believe those variations are due to seasonal effects, why don't you fit your data by a sinus function rather than by a straight line? A straight line would imply a jump at the end of each year ... this does not seem very physical.

Looking at figure 6, and based on other studies of the same cliff (Strunden et al 2014), I can believe that events in winter have a higher elevation than in summer. The proposed interpretation seems reasonable (the upper part of the cliff is more exposed to freeze-thaw cycles in winter than the lower part of the cliff which is the shade all day in winter).

But I remain doubtful about the proposed linear trend and its interpretation (p28 l10, continuously lowering drying front along the cliff face). Precipitation should induced important and fast changes in the water table, and overwhelm the seasonal fluctuations. Since the number of events is quite small and the correlation rather weak, It is important to test statistically if this correlation is statistically significant and to provide uncertainties on the slope parameter.

Correlation with precipition and temperature Because of the limited time range of your dataset and the limited number of events, it is crucial to test wether your results are statistically significant or could be due to random coincidence. One method would be to apply the same methods as in Figures 7 and 9 to a random catalog, obtained by assuming a uniform distribution of rockfalls in time during the monitoring period.

Influence of precipitations p24, l6. Why don't you cite here Helmstetter and Garambois (2010), who analyzed the influence of precipitation on rockfall activity, - several years before the two other cited references, - with a longer datased (3 yrs) - with more events (several thousands), - and a better temporal resolution (5 mn)? I usually avoid citing my own papers in a review, but here I could not resist.

Figure 8 I am not convinced by the diurnal variations of rockfall activity shown in Fig 8a. Such variations are similar to statistical fluctuations expected for a poisson process (uniform distribution in time). And the fact that there are slightly less events during the day may be explained by the increase in the seismic noise during the day, so that the smallest events are missed.

Conclusion. Why don't you decrease the sampling rate of your meteorological data to a few minutes? It seems much easier to me than trying to estimate precipitation from seismic data ...

Details

p2 l4 : "10 minutes" rather than "less than 20 h"

p2 l9. You should add a reference of Lacroix and Helmstetter (2011) as it was the first study to locate rockfall seismic signals.

p2 l15. You should cite here Deparis et al (BSSA 2008) and Dammeier et al. (2011) instead of Helmstetter and Garambois 2010. Deparis et al (2008) and Dammeier et al. (2011) both used a national seismic network designed to detect earthquakes, while Helmstetter and Garambois (2010) used a seismic network devoted to the monitoring of the rockslide.

p2 l19. You should cite here D'amato et al (2016)

p2 l29. I don't understand "solution of solids". Do you mean "dissolution of solids"?

p2 l26. I don't understand the word "Anticipation" here. Maybe replace by "identification"?

p3 l14. I don't agree with the word "overwhelm" : Rockfalls can be triggered by earthquakes for shaking much smaller than gravitational acceleration (e.g., Meunier et al GRL 2007).

p3 l15. I am not sure that triggering by earthquakes is immediate, but there are few papers about time delay between earthquakes and triggered events. Lacroix et al (2015, doi:10.1016/j.rse.2015.05.010) suggest that there can be a delayed response of a landslide to a shaking.

p3 l23. Section 2.3 deals with precipitation rather than meteorological triggers, the effect of temperature is described in another section.

p9 l13. Why these thresholds of m=1 and d=20 km? Are there no larger and more distant earthquake that could have produced a stronger shaking?

p11 l7. Why not using UTC times? It would make the analysis much easier to have a continuous time.

p11 l15. You should explain why you detected 17 events in this manuscript for 2014 but only 10 in your previous manuscript.

p13 Figure caption. replace 2014 by 2015.

p28. l2: Do you mean "upper" instead of "lower"?

p28 l17. Rocks are not "mobile" before falling from the cliff. Rather replace by "loose"?

Suppl. Material : Can you add rockfall volume for events detected by TLS?

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