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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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The paper analyses a very useful rockfall inventory in order to identify triggers and corresponding time lags. But the analysis is not very convincing and should be more developed. Moreover, it's important to know the context in which the conclusions apply, notably the order of magnitude of the rockfall volumes and the geological structure of the cliff (thickness, dip, dip direction of the beds).

Definition of rockfall Page 2, lines 27-33. This paragraph is not consistent for the following reasons: A falling rock volume is rarely fully isolated before its detachment; The relative separation of an "isolated" volume is the result of a geological process which acts over several million years; There is a contradiction between the sentences "its sub-



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sequent detachment by a release mechanism activated by a driving force" (line 28) and "The release mechanism is essentially a decrease in the stabilising forces" (line 30); Actually, the detachment can result from "decreasing material strength OR increasing stress" (line 33). So I suggest this section to be rewritten.

Section 2 (Anticipation of rockfall triggers) General comment: The different processes described in the section can be triggers (= near immediate response) but also have a delayed action (for example, an earthquake can induce new cracks whose propagation until failure can take a long time). So it would be proper to replace "The reaction of a rock mass to excitation by an earthquake is almost immediate" (page 3, line 15) by "The reaction of a rock mass to excitation by an earthquake can be immediate". In the same way, I suggest to replace "is supposed to be immediate" (line 22) by "can be immediate". Page 3, line 25. How precipitation can reduce pore pressure? It is usually assumed that precipitation increases pore pressure. Page 3, line 27: "Increasing the load requires time for rain water infiltration, percolation and retention inside the rockmass": Water doesn't need to infiltrate inside the rockmass for its weight to overhelm the superficial rock volumes which are prone to fall. Section 2.4 (page 4). Heat related triggers. It would be more proper to describe the freezing process before the thawing one. I don(t understand the meaning of "transgressive" (line 16).

Section 3 (Materials and methods) 3.1 (Study area) As the influence of meteorological factors will be studied in the paper, the climatic context of the area should be presented: Minimal and maximal temperatures, annual precipitation at the weather station (elevation of the station); Minimal and maximal elevations of the cliff; Temperature and precipitation gradient between the station and the base of the cliff. Remark: from the 1/50000 topographic map, the maximal height of the cliff would be 600-700 m and not 1000 m (between 1000 and 1600 m). Page 8, line 32: "Snow melt-generated water input is regarded as irrelevant because the cliff face is snow free in winter due to the steep gradient." This sentence is contradictory with the following one (page 28, line 11): "Water storage is also refreshed by snow melting higher up in the catchment,





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which provides a more or less continuous supply of water that can seep into the karstic limestone plateau on top of the cliff during the melt season." This process has not been analyzed quantitatively, but its influence seems to be not negligible. I suggest to reformulate this section. Page 9, line 15. I suppose that lag times for precipitation were defined as the time span between THE BEGINNING of a precipitation event with > 0.1 mm/h and the next rockfall, but it's better to precise.

Section 4 (Results) Figure 5-a and b. The legend indicates a cumulative probability, but it seems to be rather a cumulative number (a probability should be lower than 1) Figure 6-a and b. The title of the vertical axis is incomplete: It should be precised "Number of events per week or per n days"; Moreover, the time interval is different between a and b. In March 2015 and in July and August 2014, the circles are not aligned with the rugs. The text (page 18, lines 1-2) is not in agreement with Figure 6-a. From the figure (and contrary to the texte), the three periods of enhanced activity occurred in the beginning of March (7 events), end of March (6 events), and beginning of April (7 events), with few activity after April 17. Figure 6a should be dilated horizontally for a better readability. Page 18, lines 11-12 and Figure 7a. The density estimate of earthquake lag times (between an earthquake and the next rockfall) is compared to the lag time between earthquakes. Uncertainty for the first one is visualized on Figure 7a, but what about the uncertainty for the second one? Could you give the mean time between earthquakes in the text (line 12)? Page 18, lines 19-20 and Figure 7b. As for earthquakes, the lag time density estimates for precipitation events should be compared to the lag time between precipitations. Given that there are 108 rainfall events (page 24, line 10) for a monitoring period of 190 days, the mean lag time between precipitations is 42 hours. Assuming the precipitations and the rockfalls are regularly distributed in time, the mean lag time between precipitations and rockfalls should be 21 hours, which is dramatically longer than the observed peak of 1 hour. Of course, this oversimplified approach is not sufficient and the density function of the lag time between precipitations should be estimated as for earthquakes. Remark: The peak of the lag time density function is not the best parameter to characterize this function because it is not very marked. For a Poisson occurrence law (which could be a good model), the density function of the time between the events follows an exponential law, and the mode equals zero! So I suggest to indicate the mean or the median value. Assuming the rainfall events occur according to a Poisson law, the mean lag time would be 42 hours and the median lag time (mean lag time * ln2) 29 hours. Assuming the rockfalls are uniformly distributed in time, the lag time between precipitations and rockfalls should be on average half of the lag time between precipitations. As the median lag time between precipitations and rockfalls is longer than 24 hours (Figure 7b), it seems that the hypothesis of a lag time of only several hours (anticipated in section 3, page 3, line 30) can be rejected and that the rockfalls doesn't occur preferentially after a rainfall. Figure 7e. Please explain what does mean "normalised" regression slope. Please give the percentiles which correspond to the different values given in the figure. Figures 7a-7e should be dilated horizontally for a better readability.

Section 5 (Discussion) Page 22, line 32. "Within each of the activity periods the impacts are predominantly laterally spread by more than a kilometer". It's better to be coherent with page 18, line 4: "events close in time were separated by several hundred metres". Page 24, line 6. My former comments on page 18, lines 11-12 and Figure 7a, applies also to this section. The analysis should be more advanced. Page 24, line 13. "the two prominent rockfall episodes in late May and late April were not associated with any rainfall". On Figure 6, there is no prominent rockfall episode in late May. On Figure 6a, there is a rainfall episode in late April! The section 5.3.2 should be more advanced. Page 24, line 16. It seems on Figure 8 that the variability of weather-sensitive events is not very different from the one of the weather-insensitive events. So I would say that the peaks at 8 am and 8 pm are not significant (unless a statistical test prove the opposite). Page 24, line 18. The definition of precipitation-related rockfalls appears to be subjective. Comparing the time series of weather insensitive and precipitation-related rockfalls in Figure 8d, it appears there are not so different (clusters exist in both series). I suggest to test this temporal distributions against a Poisson law. If the first series is compatible with a Poisson process and not the second one, it would prove that the





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occurrence of these rockfalls is not aleatory and may be correlated with rainfall. Page 24, line 23-29. In my opinion, the proposed interpretation is not convincing. Again, this interpretation needs to prove that the temporal distribution of the weather-insensitive rockfalls is not aleatory. A 2-3 h heat diffusion time lag is invoked to explain the delay for the group 8-11 am, but the group 0-6 am occurs before the minimal temperature. Why a heat diffusion time lag is not needed for this group? Why don't the warmest hours of the day cause stresses? Page 26, line 1-4 and Figure 9. The percentages given are not objective because: a) They depend on the choice of the maximal time lag (here 385 h); b) Only the freeze-thaw season must be considered for freeze-thaw-related rockfalls (it would increase the percentage). From a risk point of view, it would be more interesting to determine the rockfall frequency within one day after a precipitation or freeze-thaw event, and to compare it with the mean rockfall frequency which is 49/190 = 0.26 rockfall/day. The number of rockfalls occurred within one day after a rainfall is about 25 (Figure 9) and the number of rainfall events is 108, which gives a frequency of 25/108 = 0.23. So the risk is not increased by rainfall. This frequency seems to be higher for freeze-thaw events: 5/10 = 0.5. Page 26, line 12. This value of ground acceleration is to compare with the accelerations of the earthquakes observed.

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