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Earth Surface Dynamics
Discussions

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

Anonymous Referee #2

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============ General Comments ============

In "Spatiotemporal patterns and triggers of seismically detected rockfalls", Dietze et al. exploit an array of broadband seismometers to locate individual rockfall events occurring across a cliff face in the Lauterbrunnen Valley, in the Swiss Alps during two periods – autumn 2014 and spring 2015. They subsequently assess the spatio-temporal evolution of rockfalls at this site and compare the located rockfall events against local meteorological data to make inferences regarding their trigger mechanisms. The first component of the study – the detection and location of rockfall events – is very nicely presented and provides convincing evidence regarding the exciting potential for seismic monitoring to help unravel the complex spatiotemporal evolution of geomorphic activity. I think that this on its own would be an interesting contribution that would fit

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very well with the broader remit of Earth Surface Dynamics. However, the subsequent analysis attempting to attribute the spatiotemporal patterns observed to different triggers and seasonality suffers from some serious flaws, leading to conclusions that are greatly overstated. I detail these issues below, in addition to a series of more minor revisions.

========== Major revisions ==========

1. I would argue that you detect two, not three types of rockfall based on the seismic signals

My interpretation of the seismic signals presented is that there are two types of rockfall signal: (i) abrupt impulsive collisions of falling rocks against another surface, and (ii) the gradual acceleration and deceleration of rock avalanches. The other type described by the authors – multiple impulsive collisions – is really just a case of several repeated instances of the former. Adding this as a separate third category is a bit misleading because it moves beyond the characteristics of the seismic signal and requires an additional layer interpretation based on the perceived likelihood that events are directly connected. In contrast, where you have Type A (or B) events triggering a Type C event, you do not make such a distinction although this would be equally valid as a classification as the suggested Type B event. I would suggest simplifying to two types of rockfall detected, and then make subsequent interpretations as required. This does nothing to diminish the significance of the paper, but separates out differences that are observed from interpretation of the temporal clustering of signals.

2. It is impossible to make robust inferences regarding seasonality based on the observations available

The reported observations span two monitoring periods that together capture less than 12 months. Given the stochastic nature of rockfall events, it is simply not possible to make inferences regarding the seasonal controls on rockfall hazards with any degree of confidence. One aspect that is picked out by the authors is the reduction in elevation of rockfall events between the spring and fall monitoring periods. However, this neglects the fact that the rockfall events are not only distributed vertically, but also laterally. The rockfall hotspots located within the two periods cluster in different parts of the cliff face. These also happen to be at slightly different elevations, but the most parsimonious explanation here is that the detected rockfalls are clustering around independent failure-prone areas and that the elevations are incidental. Multiple years of monitoring would be required before seasonality impacts can be reliably inferred. The fact that rockfall foci shift in space over time is interesting, and highlights the capacity for seismic arrays to monitor this process, but the authors should avoid over-reaching the limitations imposed by such a small monitoring period (it would, however, provide good motivation for a longer term study).

3. Ambiguity as to exactly how “trigger events” are defined

A major component of the study is to attribute detected rockfalls to specific “trigger events”, predominately relating to meteorology, and the calculation of lag time separating triggers from a given rockfall. However, there are a number of issues to be addressed here. Firstly it is not clear from the authors’ explanation exactly how trigger events are defined for all the meteorological variables assessed. This is particularly the case for wind, freeze-thaw and thermal gradients, for which an individual “trigger event” is conceptually more difficult to interpret as a single event - many of these processes would be likely to induce failure through repeated exposure and gradual weakening. The relevant methods section (Section 3.4) needs rewriting to make the rationale that determines the timing of each potential “trigger event” as clear as possible. Secondly, the authors acknowledge early on that triggers are not mutually exclusive, but are frequently additive. However they do not factor this into their analytical framework. This should be outlined towards the tail end of Section 3.4, prior to presenting and discussing the results. Without a more detailed assessment of how different processes interact, I am concerned that too much confidence is being given to the trigger attribution.

4. Is wind likely to be uniform in complex terrain?

It may still be that wind is not that important, but my expectation would be to see significant variations in wind speed across the site compared to the met station dependent on height within the valley, proximity to sheltering promontories in the cliff, and the wind direction. It is not clear whether the authors have attempted to account for this.

========== Minor Revisions ==========

1. How reliable is detection of detachment?

A number of references are made to locations of detachment and/or distances travelled between detachment and subsequent contacts. However, do all detachment events produce a signal that can be reliably detected?

2. Please avoid the “jet” colour scheme

The “jet“ colour scheme suffers from a number of issues, the most important of which is that it is not perceptually uniform. One effect of this is that the jet colour map produces perceived sharp transitions where there are none. It is also difficult to interpret for people who are colour blind, and if printed in black and white. There are plenty of other alternatives that are perceptually uniform. Please use one of these instead.

============== Line by line comments ==============

Abstract

Page 1 Line 2: “Rockfalls are an essential geomorphic process” Odd choice of word “essential” – consider revising

Page 1 Line 5: “independent information” – please be more specific so that it is clear exactly what you have done (i.e. compare against meteorological data)

Page 1 Line 6: I would suggest that “ii) identify seasonally changing activity hotspots” is actually a subcategory of the following point: “iii) explore temporal activity patterns at different scales…”. Please revise accordingly

– Introduction –

Page 1 Line 20: “…essential questions.” Essential for what? Phrasing is a little awkward

Page 2 Line 20: “environmental seismology” is a very vague term – can you refine to something more specific to the methods employed?

– Anticipation of rockfall triggers –

This section can be amalgamated into the introduction following the suggested changes:

Page 3 Lines 5-8. Remove all text from “In the following paragraphs…” onwards

Sections 2.1-2.5 are long and repetitive. This can be readily and succinctly summarised in a table (e.g. trigger, description, predicted lag time, references), which would be much more useful for reference. This table could be subdivided into the sections identified by the authors.

– Materials & Methods –

Page 5 Line 26: “resampled to 10 m grid size”. Please provide a reason to justify choosing to resample to a coarser grid

General comment: It would be good to have more details regarding the LiDAR scan (e.g. spatial area covered, fraction of area sampled at 1m grid resolution, point density) – it’s a bit sparse at the moment.

Page 6 Figure 1: Note that the map does not print very well in grayscale. Even in colour, the yellow text is difficult to read

Section 3.3: Consider breaking down into sub-sections i.e. “detection”, “classification”, “source location” to help the reader navigate

Page 6 Line 6: "For this, the hourly raw signal files from both monitoring campaigns were appended to 25 hour long traces, overlapping by one hour". Could be clearer – presumably you mean that the data was collated into daily traces with a one hour overlap?

Page 7 Line 5: Provide justification for choice of STA/LTA parameters.

Page 7 Line 7: Again need justification for threshold signal:noise ratio

Page 7 Lines 21-22: "as for example compiled by Burtin et al. (2016)" -> shorten to: (Burtin et al., 2016)

Page 7 Line 22: "All remaining potential rockfall events were manually checked for agreement with these patterns" Are you excluding signals that don't look like the 10 previously recorded Lauterbrunnen rockfalls? What about signals that are similar to other previously published rockfalls and avalanches? Needs clarifying

General comment: throughout this section, reliant on citation "(cf. Dietze et al., 2017)" on several occasions. Would be good to diversify the references to acknowledge earlier work, especially given that this is still under peer review.

Page 8 Line 4: no need to italicise "m / s"

Section 3.4: Before talking about lag times, need to specify how the timing of each "trigger event" is determined. This section needs reworking into a more logical order, noting suggested major revisions.

Page 9 Line 9: Again, justify use of STA/LTA parameters (this could simply be that this satisfactorily isolated these signals based on a visual inspection, but should specify this is the case)

Page 9 Line 22: Did you consider the wind direction? Would expect it to be pretty important!

Page 10 Lines 15-16: "again, first order proxies for the susceptibility of a rockmass to

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thermal stress can be provided by the ambient air temperature time series and its first derivative… and spatially resolved sun exposure models" Citation(s) required

Page 10 Line 25: Why just March?

– Results –

Page 11 Line 25: "…adjusted to 6 and 4 respectively." …because… (presumably there was a reason)

Page 12 Line 12: Can you add this info to the figure caption too – would be useful to a reader just skimming the paper

Page 13 Line 2: what do you mean by "location approach frequency window"?

Page 14 Figure 3: Too many sub panels, so that (c) and (d) are too small to see properly. Split into multiple figures that are larger

Page 16 Figure 5: Label (a) and (b) with the monitoring period

Page 16 Line 5: "In contrast, most of the 32 rockfalls detected in 2015 detached and impacted in the upper and central parts…" How confident are you in "detachment" locations? Does detachment reliably induce detectable signals?

Page 17 Figure 6: On panels (a) and (b) there is no scale for the cumulative number of events. The plotting of the time series is quite counter-intuitive. Why not plot in temporal order?

Page 18 Line 6 (and again in Line 7): Should be rockfalls per month per km2

Page 19 Figure 7: The panels are too small

– Discussion –

General comment: Do not develop a detailed discussion around lag times, although this is given a dedicated section in the results, so presumably the authors thought it to be important.

Page 21 Line 30: “presumably this second rockfall was triggered by the impacts of a preceeding one” note that this is an assumption, not an observation. This is important to remember a page later (Page 25 Line 25) which states that the events would be mapped as “two discrete rockfalls” using a posterior mapping approach, suggesting that this would be incorrect. It would be inconsistent with your assumption, which may be reasonable, but you have not proven that the second rockfall was triggered by the first.

Page 22 Line 24: “no other method” this is overstating things a bit – I can immediately think of some (admittedly labour intensive and dangerous) manual methods that could collect the same information. Just need to revise the wording a little

Page 23 Lines 10:16: If you use all the spatial information, it looks like there are distinct patches of the cliff face active at different times of the year. These are not vertically connected so there doesn’t appear to be much evidence to support your assertion that rockfall activity is actively migrating down the cliff face. Note that there is activity throughout the vertical extent of the cliff face in the 2015 data.

Page 23 Line 20: “barely resolved” – it isn’t fully resolved. Additionally for stochastic processes need several years of observations to make robust interpretations regarding seasonality

Page 23 Line 23: “. . .there is a diurnal scale that modulates the effect of the prior one” Awkward phrasing - revise

Page 23 Line 25: “. . .conditions.” Citation needed

Section 5.3.1: Generalisation of seasonality cannot be made based on <1 year of data. Should be removed

Section 5.3.2: “The weather-relevant scale” This seems a somewhat ambiguous term, and I’m not sure why this is separated out from the diurnal scale?

Page 25 Line 2: slabs not “slaps”

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Page 25 Figure 8: Lighter colours not easily visible (especially for freeze-thaw in panel (d)). Panels generally too small.

Page 26 Figure 9: Rockfall activity drop is not the most intuitive of ways to express this. Much simpler just to plot the cumulative number of events. Also, use a legend and only one vertical axis – this would be much clearer.

Section 5.3.4. For reference, it would be good to know approximately what gravitational acceleration would be required before there is likely to be significant risk of moving rocks/crack propagation.

Section 5.4 I'm not sure that this section can be justified based on comment in major revisions

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