

Interactive comment on “Single-block rockfall dynamics inferred from seismic signal analysis” by Clément Hibert et al.

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Dear authors,

We have now received two reviews, and, since the discussion closes in a few days, I give a short summary of what I find important. While both reviewers are generally supportive of the manuscript, they both raise some criticisms that need to be addressed before publication. I add some comments of my own reading to that.

Presentation and language Reviewer #2 criticizes presentation and language and asks for rewriting with a focus on clarity. I agree that there are fairly frequent odd formulations and unclear writing.

Scaling, linearity and the fits Reviewer #1 raises concerns about the terminology used

C1

in the paper. I do not agree with his definitions; in my understanding, two variables A and B scale with each other, if they have a positive monotonic relationship, without the need of specifying a function. That is, if A increases, B increases also. Two variables are proportional if their ratio is a constant. And they are linearly related if $A=mB+b$, where m and b are constants. However, I agree with reviewer #1 that in the manuscript, the terminology is not used in a common way. For example, I would not say that two variables scale linearly, but rather that they have a linear relationship of linear dependence. That said, there is something funny about the plots in Fig. 4 and the way the relationships are discussed. The plots in Fig. 4 are all log-log. In this representation, a proportionality would result in a straight line with a gradient of one. A linear relationship would result in a curved line. A closer look reveals that the depicted fit lines actually have gradients that deviate from one. They show power law relationships. The fit values given in Table 2 indicate exponents of up to 2. This may change the entire results, discussion and outcomes of the paper. Here is at least a major problem with the communication, if not with the use of the fits and the statistical relationships. These need to be carefully resolved and communicated.

Relation to theory Reviewer #1 comments that the relations to his theory are partially incorrect and not well described. In light of the issue raised in the preceding point, I ask the authors to clearly present the used theory in the paper, identify appropriate hypotheses that can be tested with the data (both a theory-derived hypothesis and a null hypothesis), and to discuss how the outcomes of their experiments relate to these.

Energy budget I also like reviewer #1's suggestion of the energy budget and ask the authors to provide appropriate calculations and a discussion on this.

Relationship between seismic energy and amplitude The authors should also investigate and discuss the relationship between seismic amplitude and energy and how this would impact their analysis.

Significance and fit values The authors argue the significance of their trends based on

C2

goodness of fit statistics such as R^2 . There are at least a few instance (especially when claiming no significant relation), where a non-parametric statistic such as Kendall's tau would be more appropriate.

Please note that all reviewers' comments that I have not mentioned should also be treated with due care.

2.30 These authors. . .

3.15 French Alps (French with capital letter)

5.3 . . .dependent on. . .

9.19 . . .for blocks for which. . .

9.25 . . .the uncertainties associated with determining the amplitude at the source are lower than those associated with seismic energy.

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