



Interactive
Comment

Interactive comment on “Assessment of rockfall hazards using databases and considering triggering meteorological events” by A. Delonca et al.

Anonymous Referee #3

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GENERAL COMMENTS

The paper presents interesting results concerning (a) the methodology for analyzing the correlation between rockfalls and meteorological factors (b) the influence of these meteorological factors. From the methodological point of view, a "classical" analysis is first presented and then compared to the method proposed in the paper. In my opinion, this classical analysis should be enhanced (see comment #7). Concerning the influence of meteorological factors, the results are not highlighted either in the conclusion or in the abstract (what factors do really influence the rockfalls?). So I think a moderate revision is needed for the manuscript to be acceptable.

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SPECIFIC COMMENTS

1) Page 1335, lines 1-3: The cited references concern the rockfall frequency (which depends on the intensity) and then the temporal dimension of rockfalls as the present paper do. So I suggest replacing "rockfall intensity" by "rockfall frequency". The specificity of the present paper is not to deal with the temporal aspect, but with the influence of meteorological factors on the rockfall frequency (rather than probability). So I suggest replacing the sentence "Temporal probability is recognized to be more difficult to assess (Hantz, 2007)" by: "But little work has been carried out to study the influence of meteorological factors on the rockfall frequency."

2) p.1335, l.9: I suggest to add the paper by Frayssines & Hantz (2006), which concerns the statistical correlation between rockfalls and meteorological factors (mainly temperature) and presents a similar (but more classical) approach. Reference: Frayssines, M. and Hantz, D.: Failure mechanisms and triggering factors in calcareous cliffs of the Subalpine Ranges (French Alps), Eng. Geol., 86, 256–270, 2006.

3) p.1335, l.27: Although the intensity is not considered in the paper, it should be useful to give at least on order of magnitude of the volume of the rockfalls considered. Indeed, the triggering factors could be dependant of the volume. This information will allow a comparison with other studies. Moreover, for the results of the work to be useful in terms of rockfall frequency, it is necessary to give the volume range of the rockfalls considered in this study (as well as the mean height of the rock walls).

4) p.1336, l.16-19: The proportions given are rather frequencies than true probabilities ("The daily rockfall hazard, which is the probability of a fall on each day, . . . , is close to these proportions"). So I suggest replacing the terms "probability" (line 16) and "proportions" (line 19) by the term "frequency".

5) p.1337, sections 2.1, 2.2 and 2.3: The information about temperature is not homogeneous. Please give for each site the extreme temperatures and the daily temperature range (it is missing in section 2.2).

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6) p. 1338, l.20: Please give also the difference in elevation between the weather stations and the studied areas (or the elevation of the stations).

7) p.1339, l.13 and Table 2: a) The total number of rockfalls in Table 2 doesn't correspond to the number of events in Table 1, for the Bourgogne and Auvergne regions. b) Concerning the sentence "Surprisingly, the maximum average number of rockfalls per day occurs for the lowest amounts of daily rainfall": It is not the "number of rockfalls per day" which is given in Table 2, but the number of rockfalls per interval. Moreover, it is not surprising because the lowest rainfall interval (0-20) has the highest frequency (given in the second column in Table 2). The right comparison should be between the frequency of the rainfall intervals and the corresponding frequency of the rockfalls. I suggest modifying Table 2 for a better classical analysis.

8) p.1340, l.7-8: Please precise what meteorological parameters have been used.

9) p.1342, l.11: After reading of the whole section 4.2.1, I suppose that the proportionality coefficient gives the number of rockfalls knowing that there is at least one rockfall in the day considered, and not the mean number of rockfalls (including the days without rockfalls). Otherwise the proportion x should be 100%. This point should be explained.

10) p.1343, l.23: It is not visible in Table 5 that the correlation can be identified "even when a rockfall occurs a few days later". This point should be explained.

11) p.1344, l.21: Please explain what is this marker of the freeze–thaw activity.

12) p.1345, l.3: I suggest to say "The new approach also allows estimating the conditional probability of rockfall", because it is rather a frequency.

13) p.1345, l.23: The sentence "The probabilities are considerably lower for databases with few rockfalls" is an evidence. I suggest removing the lines 23 to 25.

14) p.1346, l.4: The paper which concerns the correlation between rockfalls and meteorological factors is the one mentioned in comment 2 (Frayssines & Hantz, 2006) and not the one cited here (Hantz & Frayssines, 2006).

15) p.1346, l.15: I don't understand this sentence: "Moreover, the cross-correlation is not helpful when there is no more than one event per day." Could you explain this point ?

16) P.1347, l.18-20: The sentence "(similar assessments could be made with other combinations of the rate of correlated data and the level of correlation)" is not understandable. I suggest removing it from the conclusion.

17) p.1356, Table 6: The value of the slope of the regression line (proportionality coefficient) is also an important result. It should be presented in Table 6.

18) p.1359, Figure 2: The figure concerning the Auvergne site presents some anomalies. a) A freezing period seems to be missing in curve (e) when the minimum of temperature (curve c) is negative. b) The curve (c) clearly shows 4 annual cycles when it should correspond to a 3 year period. It is surprising that the weather could be so different between Bourgogne and Auvergne. This anomaly should be explained.

TECHNICAL CORRECTIONS

Technical corrections are proposed in the pdf file.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/2/C648/2014/nhessd-2-C648-2014-supplement.pdf>

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