

Interactive comment on “Derivation and evaluation of landslide triggering thresholds by a Monte Carlo approach” by D. J. Peres and A. Cancelliere

Anonymous Referee #1

Received and published: 30 March 2014

This is an interesting paper, that combines in an innovative way existing modelling components for the construction of synthetic time series of rainfall, for rainfall infiltration and the build up of a water table in a slope, and for slope stability analysis. Overall, I have found the paper sound and of potential interest to a large audience of hydrologists and geomorphologists.

However, there are quite a few issues that need to be clarified in the paper.

I have divided my comments into “editorial”, general comments and specific comments. The latter are keyed to page and line numbers. The distinction does not imply a ranking of the importance or relevance of the comments.

It should not be too difficult for the authors to address my comments, and respond to

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my queries.

Editorial comments

Overall, the paper reads well, but in places the text is difficult to read, and follow. Other parts of the text are not really necessary, and could be deleted, shortened, or moved to an appendix. As an example, the description of the single modelling components (rainfall time series, TRIGRS modelling, slope stability modelling), does not add much to what is already known in the literature. The description of these model components should be shortened, or moved in specific appendixes, where they can be properly described.

The Abstract and the Conclusions are not fully clear, and should be rewritten. In the Abstract should state briefly the purpose of the research, the principal results and the major conclusions, and must be able to stand by itself. In the Conclusions, the authors should list only the main conclusions and relevant findings of their work.

In the text, the authors use the term “hyetograph”. A hyetograph is a graphical representation of the distribution of rainfall over time i.e., a rainfall record or a rainfall time series. Indeed, for their work the authors have used rainfall records, and not hyetographs. This should be clarified throughout the text.

In section 3, the definition of the threshold and the evaluation of the performance of the threshold should be separated in two different sections, or sub-sections at least.

Quality of the figures should be improved. Text and labels in the figures are small, and can be difficult to read. When modifying the figures the authors should consider their final size in the journal, considering that figures can occupy one or two columns. Some of the figures (e.g. Figures 4, 5) are very small, and difficult to read. Parts of the charts (e.g., in Figure 4) do not show data. These parts can be removed from the charts, to make them larger. Authors should consider that use of colours is possible, I believe with no extra cost, in HESS. Some of the figures may improve significantly is colours

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are used.

Specific comments

Abstract, Line 1, “Rainfall thresholds are the basis of early warning systems able to promptly warn about the potential triggering of landslides in an area”. This is a vague, and partly misleading statement. First, rainfall thresholds are not the only basis for landslide early warning systems. Second, “prompt” warning or alarm is not base solely on the exceedence (or not exceedance) of a threshold. A substantial amount of human judgment is involved in giving a warning, or an alarm. [Note that the same comment holds for the first sentence of the Introduction].

Introduction, page 2761, lines 19, 20. “Reliability of thresholds derived by the analysis of observed data is generally limited by the quality and availability of such data”. There are other factors that influence the reliability of rainfall thresholds, including the uncertainty associated with the definition of the thresholds. See e.g. doi:10.1016/j.geomorph.2011.10.005.

Introduction, page 2761, line 27. “... critical duration D ...”. The authors should be aware that “critical” is used with different meanings in the literature related to the definition of rainfall thresholds. See e.g., doi:10.1016/j.enggeo.2004.01.007, Govi and Sorzana (1980), Heyerdahl et al (2003), doi: 10.1007/s00703-007-0262-7.

Introduction, page 2763, lines 16-17. “... thus casting some doubts on the use of parametric power-law as a proper functional form in deriving rainfall thresholds”. A conclusion of the work is that the power law threshold model fits well the modelled data. The authors should comment this finding, in view of the findings of e.g., Rosso et al., 2006; Salciarini et al., 2008.

Introduction, page 2764, line 4. “For this last point we adopt a precise rainfall identification criterion.” Precise criterion? What does this mean? It seems to me that the authors have established a criterion, and have used it. There is no evidence that this

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criterion is accurate (or precise).

Introduction, page 2764, line 18. “From their study . . .” Unclear how is “they”.

Introduction, page 2764, line 21. Provide references for the NSRP model.

Monte Carlo modelling, page 2765, lines 8-9, “A stochastic rainfall model, calibrated on observations at a representative site, is used to generate a 1000-years long hourly rainfall time series.” This is a tricky point that may ingenerate some confusion. The rainfall time series is “virtual”, as it is its length of 1000 years. It is not a climatic series that actually spans a 1000-year period. The difference is significant. As far as I can tell, no climatic information was used to generate the series, and the series was generated ignoring known of possible changes in the climatic signal. This should be clarified by the authors.

Monte Carlo modelling, page 2765, line 14. Why have the authors decided to use a day (24 h) to separate rainy from dry (non-rainy) periods? Monte Carlo modelling, page 2765, line 20. What is this “basal boundary”?

Monte Carlo modelling, page 2766, lines 21-22. What is this “storm origin”? This is not at all clear? “Storms” and “rainfall” can be very different, and the authors should make this clear.

Monte Carlo modelling, page 2767, line 3. What is this “cell origin”? Again, this is not clear.

Monte Carlo modelling, page 2768, line 4. Say something about the “assumptions of Rosso et al. (2006).”

Monte Carlo modelling, page 2769, line 1. “The ratio A/B is the well-known specific upslope contributing area A/B” ? Text is unclear. What does it mean that A/B is . . . A/B?

Threshold derivation, page 2771, lines 4-6. “For a hillslope of given properties, Monte

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Carlo simulations lead to a series of computed failures, i.e. time instants at which the factor of safety drops below the value of 1.” Although the statement is correct, I see a conceptual potential problem. It is well known that so called “deterministic models”, including TRIGRS, underestimate the stability conditions of the areas where they are applied. Indeed, it is common that the application of TRIGRS in a study area results in widespread (predicted) instability, which does not match the actual (real) abundance of event landslides (that is typically less, or much less than what is predicted by TRIGRS, or other similar models). Reasons for this behaviour are manifold, and not really important for this study. What is important is the fact that if TRIGRS predicts “instability” (e.g. $FS < 1$), not necessarily a slope failure occurs. This has an impact on the number of true positive and true negatives, and the related analyses. The author should comment this issue in their work.

Threshold derivation, pages 2771-2. I can think of additional reasons for the scatter of empirical points in a I/D chart that the two listed by the authors. One is the natural variability of rainfall induced landslides. It is well known that a simple (possibly too simple) threshold model, like the I,D model adopted in this work, cannot capture the (large!) natural variability and complexity of rainfall induced landslides. Stating that (only) two factors control the joint presence of I/D events that have and have not resulted in landslides in an I/D log-log plot is too simplistic, and (partly) misleading.

ROC analysis, pages 2771-2774. Most of what is written in this section of the text is not really new, or innovative. This part of the text can be shortened considerably. In the very (very!) large literature on the assessment of forecasts using contingency tables, the zillions of related performance indexes, ROC plots, etc., there is indeed confusion and significant overlaps. Indeed, this does not help. However, attempts to limit the confusion exist. The authors should consider doi: 10.1175/WAF1031.1 and specifically doi: 10.1175/2009WAF2222300.1. The performance index “Delta” used by the authors is not really new, but well known in the literature. I have notices that a public comment to this paper has pointed out the issue already. The authors are

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advised to check the references listed in this public comment <http://www.hydrol-earth-syst-sci-discuss.net/11/C624/2014/hessd-11-C624-2014.pdf>.

ROC analysis, pages 2773, line 21. “. . . model deterministic thresholds . . .”. I have a problem with this definition of a “deterministic threshold”. A (pseudo-)deterministic model like the one used in this work, does not result in a “deterministic” threshold, necessarily. The authors should clarify the meaning of “deterministic threshold”, and if their definition implies that the threshold is accurate, clear-cut, or fuzzy. This is crucial for the paper.

Investigated area and data, page 2774, lines 7-10. More information should be given on the location and size of the study area. Figure 3 is not really sufficient. Accurate or approximate location of the considered landslides should be shown in Figure 3. Depending on the location of the landslides, use of a single rain gauge may be reasonable, or not. This should be clarified.

Investigated area and data, page 2774, lines 15-17. “Based on a preliminary analysis of monthly statistics, six homogeneous rainfall seasons have been identified: (i) September and October, (ii) November, (iii) December, (iv) January–March, (v) April and (vi) May–August.” More should be said on how these “homogeneous rainfall seasons” were identified. Is this statistically significant, given the very short period covered by the rainfall time series? This needs to be clarified.

Results and discussion, page 2775, line 5. Is the number of 19,826 rainfall events in a (virtual) 1000-year period realistic, or not? This is an “average” of 20 rainfall events per year? Is this reasonable for the study area? How does it compare with the number of real rainfall events in the considered period?

Results and discussion, page 2775, lines 12-13. The count of Positives (N) and Negatives (N) can be misleading. See previous comment: Threshold derivation, page 2771, lines 4-6.

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Results and discussion, page 2775, lines 1-2. It is not clear to me how the contributing area, and the specific catchment areas were determined. In spatial modelling, this is usually done exploiting a DEM, of a given resolution. However, this is not the case in this work. This should be clarified.

Results and discussion, page 2776, lines 28-29. “. . . but may still be acceptable.” Why? What do you mean, exactly?

Results and discussion, page 2777. Eq. (12). How does this threshold compare to similar thresholds for the same area, or for nearby areas. As an example, thresholds have been recently proposed for Calabria, to the N and NE of the study area. See: doi:10.5194/nhess-14-317-2014. Other thresholds may be available for Sicily, or for similar areas.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2759, 2014.

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