

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

Anonymous Referee #4

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General comments

This paper deals with the definition of rainfall intensity-duration thresholds based on a combined approach which uses Montecarlo simulations to generate synthetic rainfall series, and a physically-based model to simulate pore-pressure dynamics and estimate the factor of safety of hillslope. Specifically, the Authors introduce a new way to assess the quality of empirically-based power-law ID thresholds. The paper is within the scope of HESS and surely of interest for both hydrologists and geomorphologists. However, for my perspective, this work - as it stands - suffers from two major limitations: 1) The scientific methods and assumptions are not always valid (at least for the “hydrological” part of the work); 2) The results are not sufficient to support the interpretations and conclusions of the work.

C1232

About the first point, the assumption to consider two different time-scales for vertical and lateral flow is valid if the ratio between soil depth and the square-root of the upslope contributing area is small: $d_{lz}/A^{0.5} \ll 1$. Based on this assumption, the Authors used the TRIGRS model (1D model) to simulate vertical rainfall infiltration during rainfall events, and a drainage model to simulate lateral flow after the end of rainfall. The Authors agree with the hypothesis (section 2.2), but then, paradoxically, they perform their experiments by varying the specific upslope area from 0 ($d_{lz}/A^{0.5} = \text{infinity}$) to $20/B$. For my view this is a strong weakness of the paper, as the major conclusions of the work are related exactly to this point (“power-law ID equations can adequately represents the triggering conditions.for a hillslope with small specific contributing area”).

About point 2, the Authors say that “. . .the proposed methodology is applied to the landslide-prone area of Peloritani Mountains, Northeastern Sicily, Italy.”. Actually, for my perspective, this work represents a modeling exercise on an artificial-unique-planar hillslope, characterized by 40 degrees constant slope, uniform soil depth, and mechanical- and hydrological- properties. I am not sure that results and conclusions can be generalized and validated against the real events (figure 5), and I am wondering why the Authors did not use the “real” landscape (digital elevation model) of the landslide-prone area of Peloritani Mountains to perform their experiments.

Therefore, in my opinion a major revision is needed before the manuscript can be considered ready for publication in HESS.

Specific comments

Abstract (page 2760, lines 21-23): This conclusion “power-law ID equations can adequately represents the triggering conditions.for a hillslope with small specific contributing area”) is not consistent with the hypothesis of the “TRIGRS-Drainage” model (see General comments). Paradoxically, This is also what the Authors argue at section 2.2. This point absolutely needs to be addressed.

C1233

Introduction (page 2762, lines 14-20): I think that this part should be moved into the results and discussion section.

Section 2 (page 2765, lines 8-9): Please, clarify what you mean with “a representative site”. As also stated by referee #1, all this part needs to be clarified.

Section 2 (page 2765, lines 18-20): This is an assumption that the Authors make. After a long dry period, pore pressure at the soil-bedrock interface may also assume negative values.

Are the Authors assuming a steady-state initial pore-pressure (i.e., suction head) profile?

Section 2 (page 2766, lines 5): Please, clarify that you are making the assumption that the failure surface coincides with the soil-bedrock interface. Is this consistent with the landslide-prone area of Peloritani Mountains?

Section 2.1 (page 2767, lines 8): Please define cdf.

Section 2.2.2 (page 2769, line 17): I do not see α_1 in the equation.

Section 2.2.2 (page 2769, line 23): This is a very “particular” explanation. I would remove this sentence from the text.

Section 2.2.3 (page 2771, lines 1-2): Actually, the equation tell us that for gentle slope (and/or thin soil depth) the critical wetness ratio is also high with poor mechanical soil-properties. I would remove this sentence.

Section 4 (page 2774, lines 23-26): I think this is a too simplistic assumption (“we consider as representative for the case study area a hillslope of slope $d=40^\circ$ and. ...”). What about the characteristics (slope angle, soil depth, etc.) of the four (are the Authors sure on this value?) landslides that the Authors used to validate the derived threshold? Much more information about the investigated area and data should be provided in the paper.

C1234

Section 5 (Results and discussion): The ID thresholds are derived by considering a “rigid” hillslope configuration and this makes the comparison between model results and reality quite flawed. What about to use the “real” landscape (at least a portion where landslides occurred) instead of the “rigid” hillslope schematization?

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

C1235