

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

### **Anonymous Referee #2**

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#### General comment

The paper deals with the definition of rainfall Intensity-Duration thresholds to be used as tools for early warning of shallow landslides. Such a topic is surely of interest for the readership of HESS, and is somewhat innovative, in the sense that it makes use of already known models and approaches in an innovative way. Overall, the manuscript is well written and concise. Nonetheless, some parts are maybe even too concise and some information is missing, making in some cases difficult to judge the significance of the major obtained results (as I better explain in the following detailed comments). Therefore, in my opinion, moderate revision is needed before the manuscript could be accepted for publication in HESS.

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Discussion Paper

## Detailed comments

As my comment is being posted after the comments made by two other readers, I prefer not to repeat all the already raised issues (in particular, in section 3, the potentially misleading symbols used in the ROC analysis, which description could be shortened because is not novel).

Section 2 (page 2765, lines 10-16). The Authors should discuss the implication of the choice of the inter-event time. In fact, some of their conclusions about the validity of the identified thresholds have to do with the role of the initial (pre-event) conditions on the triggering of the landslide. It is quite obvious that the choice of the inter-event time affects the “memory” of the previous event at the beginning of the new one. Such a memory depends also on the hydraulic properties of the soil cover under study (see another related comment below), so I expect that, for a given soil cover, the (arbitrary) choice of the inter-event time may hide the effects of the previously fallen rainfall. Indeed, the small number of events identified in the synthetic rainfall series used for the Monte Carlo simulation (less than 20 rainfall events a year) makes me think that the choice of a dry interval of at least 24 hours results in few long-lasting events separated by dry intervals long enough to allow the drainage of most of the previously infiltrated water from the soil cover (especially considering the high value of the hydraulic conductivity assumed for either the regolith or the fractured bedrock).

Section 3 (page 2773, equation (9) and following discussion). As already pointed out by comments made by others, the chosen objective function is not novel. A possible improvement of such a commonly adopted approach, could be taking into account that in many real cases it is not obvious that a false alarm and a missing alarm have the same importance (the losses deriving from a missing alarm may be much more serious than the costs deriving from a false alarm), and it would be more effective for the decision-maker to define an objective function which accounts for such weights.

Section 4 (page 2774, lines 13-20). More information should be provided about the

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calibration of the NRSP model, especially because the resulting synthetic series may affect the significance of the obtained I-D threshold (see the above comment about the effects of the choice of the minimum inter-event dry interval). Such concern about the NSRP calibration is motivated by the sentence at the beginning of page 2778, where the Authors say that in five years 190 events were recorded: nearly 40 events per year, which is around the double of the average yearly number of events of the synthetic generated series.

Section 4 (page 2774, lines 22-26). Much more information about soil properties should be provided (it is not even written which kind of soil is studied). It seems that the Authors consider the obtained thresholds representative of an area as large as several tens of square kms. The variability of soil properties and slope morphology within such a large area could completely reduce the obtained results to a mere modeling exercise. The value of the critical wetness ratio corresponding to the assumed geotechnical soil properties and slope geometry should be given here.

Section 5. The results of the sensitivity analysis to variations of geotechnical soil properties and soil cover thickness are quite interesting, but they should be completed also with the analysis of the effects of variations of the hydraulic properties (in particular the hydraulic conductivity) and, even more, of the ratio  $c_d$  between the hydraulic conductivity of the fractured bedrock and that of the soil cover (as far as I understand, arbitrarily set to 0.1 in absence of experimental data). Indeed, as the failure is a-priori assumed to occur at the soil bedrock interface, I expect the results to be extremely sensitive to the variation of such parameters.

#### Editorial issues

Page 2769, last three lines, and page 2770, first line. The explanation of the meaning of A, B and their ratio is unclear and should be reformulated.

Page 2774, lines 9-10. Possibly the events of 25 October 2009 and 1 October 2009 are the same event (with wrong dates): somewhere else in the paper it is written that

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only four landslides occurred during the considered period.

Page 2776, line 8. Replace “correspond” with “corresponds”.

Page 2778, line 19. Insert “as” between “soon” and “soil”.

Page 2779, line 2. Delete the word “for” after “Traditionally”.

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

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