

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

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We would like to thank R#4 for his comments.

General comments

[R#4]: *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*

[A]: We do not fully agree with the synthesis of the reviewer, since Monte Carlo simula-

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tion is not limited to generate synthetic series, but rather to use synthetic series as input to a models in order to obtain stochastic outputs (see, e.g., Salas, 1993; Kottegododa and Rosso, 1997).

[R#4]: *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.*

[A]: This reviewer criticizes our work in two points: a) The need of the hypothesis $\varepsilon = d_{LZ}/\sqrt{A}$ to be valid for application of the hydrological model, and the contrast with assumption of “small” A/B ratios. b) The fact that the derived threshold has been obtained by simulations for an hillslope of fixed parameters. If we understand well, he would prefer to see an application of the model at a regional level, exploiting a DTM.

We believe that some of his criticism may stem from a lack of clarity in the MS. In the following we attempt to clarify the issues raised by the reviewer.

[R#4]: *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”).*

[A]: Of course we agree it is true that $A/B = 0$ implies $d_{LZ}/\sqrt{A} = \infty$, this is not what

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we mean by "small specific contributing area". As is explained in more than one point in our paper (p. 2775; p. 2776, line 3), the $A/B = 0$ case was introduced solely as a "fictitious" assumption, i.e. used to isolate the effect of rain intensity variability from antecedent conditions variability, in order to investigate the controlling factors of the modeled process. In other words, considering even a $A/B \gg 1$ but neglecting the memory one obtains the results of the $A/B = 0$ case. We agree that this may generate confusion, and this case will be more properly called " $h_{in} = 0$ case" (zero initial water table height) or "no memory" case, in future versions of the MS. $h_{in} = 0$ case means that memory is neglected independently from the value of A/B , which could then be fixed arbitrarily big, and hence does not contrast with the $\varepsilon \ll 1$ assumption.

The comment by this reviewer reveals that what is meant by "small" upslope specific contributing area needs some clarification. As clarified in the response to other reviewers R#1 and R#2 what has to be small is the time constant of the drainage model:

$$\tau = \frac{\frac{A}{B}(\theta_s - \theta_r)}{K_s \sin \delta}$$

and in particular, we have seen that when $\tau = 3$ days ID power-law thresholds may lead to acceptable results (ROC-index TSS > 0.8), which for $K_S > 2 \times 10^{-5}$ m/s, and the other parameter values, corresponds to $A/B = 10$ m, which in turn corresponds to $A = 5 \times 10 = 50$ m² for our data, considering a DTM at a 5 m resolution and the D8 method to derive flow directions (see supplement on the case study area <http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>). With a soil depth $d_{LZ} = 2$ m a $\varepsilon = 2/\sqrt{A} = 0.2828$ results, which is small, considering that the TRIGRS model has been applied in areas where ε was up to 0.5-0.6 (see Baum et al., 2010). Thus, we do not agree that there is a paradox in the paper. Conclusions will be written more clearly, specifying better what it is meant for a "small" value of the A/B ratio.

Generally speaking it is noteworthy to say that the two ratios d_{LZ}/\sqrt{A} and A/B may be small independently, since A/B may be small if $A \ll B$, which does not mean than A

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has to be "small" in an absolute manner, but just in a relative manner to B . In particular, B can be large enough to make the ratio A/B small. Hence this is compatible with an upslope area A large enough relatively to d_{LZ} so that it results $d_{LZ}/\sqrt{A} \ll 1$.

[R#4]: *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.*

[A]: Regarding this point, we agree that more information needs to be provided on the case study area. We have posted a specific supplement on this issue (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>) which presents information that could be included in further version of the MS.

Regarding Figure 5 of the manuscript, we think that the additional material on the case study area that we are providing in the supplement (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>), especially the ones regarding the dates and locations of landslides, may finally help to dispel any suspect on the presented results.

We also do not fully agree with the reviewer, when he writes "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." If we are interpreting well this comment what he would like is that we apply the model to a

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Digital Terrain Model of the area (he says: "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"). From our perspective indeed, the sensitivity analysis we performed (Fig.6 of the MS) is more significant than the application to a Digital Terrain Model of the area, *a fortiori* now that also a variation in the K_s and c_d values has been included in such analysis (see response to R#2). The sensitivity analysis of Figure 6 of the MS is more than applying the model to a digital landscape, because the sensitivity to most important parameters which may vary spatially has been analyzed. A worry of the reviewer that may be justified is that variations of slope are not (explicitly) considered (in the paper only a slope of $\delta = 40$ is mentioned). Slope is one of the most important variables in landslide analysis, but its predominant effect is on the geomechanical part (value of FS), and not in the hydrological part (infiltration and drainage). In the ranges of the slopes of interest (roughly, $30 < \delta < 45$, see response to R#1), pressure head response does not change significantly (this is quite intuitive). What changes is the factor of safety, which is very sensitive to slope. This effect of slope is included in the ζ_{cr} parameter which is varied in all his possible range of variation.

In other words, by the sensitivity analysis we have covered a wider variation of parameters than the one that one need to simulate by using a digital landscape, and in a more explicit way.

Regarding the threshold equation that was derived $I = 71.52D^{-0.8}$ and its validity, related data, please see again the posted supplement (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>), where we present more data regarding the case study area and more detailed information. Briefly, the choice of $\delta = 40$ and $A/B = 10$ m results from the analysis on a pre-event DTM of the landslides occurred on 1 October 2009; moreover triggering of cells with $\delta \geq 40$ and $A/B \geq 10$ m correspond to a significant portion of the Pelorini area, for which it is worth to issue a warning. Furthermore, the validation test of Figure 6 of

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the MS indicates that the threshold is reasonable, as well as the comparison with the empirical threshold derived by Gariano et al. (2013) (see response to R#1).

Specific comments

[R#4]: *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.*

[A]: See above.

[R#4]: *Introduction (page 2762, lines 14-20): I think that this part should be moved into the results and discussion section.*

[A]: It is true that this sentence may be written in a better way and may appear as a result, but we do not agree that the part has to be moved to the results and discussion section, because it is a statement that is true in general. We will instead re-write the sentence to avoid this confusion.

[R#4]: *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.*

[A]: A representative site means that the rain gauge is located at a point that measures rainfall amounts that can be considered valid for the study area. Of course, since rainfall may vary spatially, ideally one should have a spatially continuous measurement of rainfall, but seldom one has this information (e.g. from a radar), but this is an issue that goes beyond the scope of our work. The analysis of effect of spatial variability of rainfall goes beyond the scope of our work as well. For the other points of this part that need to be clarified, please see our response to R#1 and the supplement on the

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case study area (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>).

[R#4]: *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.*

[A]: It is true that after a long dry period pore pressure at the soil-bedrock interface may also assume negative values, but it is very difficult to model this aspect. Indeed this is a (slightly) conservative hypothesis commonly adopted (see Rosso et al., 2006, Baum et al., 2010).

[R#4]: *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?*

[A]: This is a reasonable assumption for the Peloritani Mountains since core samples collected in the area show a depth of the erodible strata of about 2 m, that covers a fractured bedrock layer. (again see supplement on the case study data (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>)) A comment on these details will be added in future versions of our paper.

[R#4]: *Section 2.1 (page 2767, lines 8): Please define cdf.*

[A]: "cdf" is a commonly adopted abbreviation for "cumulative distribution function". This detail will be added to future versions of our paper.

[R#4]: *I do not see α_1 in the equation.*

[A]: It is true that α_1 does not appear in the eq. (3). In fact it may be more appropriate to show the equation that is obtained using the Gardner (1958) SWCC in Richards'

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equation. In future versions of the MS we will replace eq. (3) with the following:

$$\frac{\alpha_1(\theta_s - \theta_r)}{K_S} \frac{\partial K}{\partial t} = \frac{\partial^2 K}{\partial Z^2} - \alpha_1 \frac{\partial K}{\partial Z} \quad (1)$$

where α_1 appears.

[R#4]: *Section 2.2.2 (page 2769, line 23): This is a very "particular" explanation. I would remove this sentence from the text.*

[A]: Probably reviewer means "peculiar" instead of "particular". What we meant to explain is a storage effect of a reservoir in which the output flux is less than the input flux. In our case the storage is given by the unsaturated layer, the input flux is the infiltrating flux, while the output flux is the flux to the saturated zone $q(d_u, t)$, which results damped and smoothed. In other terms it is what it is explained by the TRIGRS v.2 manual at Figure 8 ("*Plot of example input and output of unsaturated zone infiltration model. Partial absorption of water infiltrating at the surface results in damping and smoothing of the signal at the base of the unsaturated zone*"). Hence we do not agree that the explanation is peculiar, but perhaps it needs to be clarified. Moreover, as responded to R#3, the word "lamination" is a bad and erroneous translation of the Italian word "laminazione", and hence it is not appropriate.

In future versions of our work we will explain better these concepts.

[R#4]: *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.*

[A]: We agree with the reviewer that for gentle slope (and/or thin soil depth) the critical wetness ratio is also high with poor mechanical soil-properties. In general, a soil with given cohesion and internal friction angle may be unconditionally stable or unstable at the same time depending on soil depth and slope. We meant "Good"/"poor" mechanical

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properties in a "relative" sense. We will not remove the sentence, instead we will explain better what we meant.

[R#4]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 $d=40$ 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.*

[A]: The assumption of a hillslope of slope $\delta = 40$ and $A/B = 10$ m as the reference case for the case study area derives from the analysis of these properties for the landslides occurred on 1 October 2009, on the basis of a 5 m resolution DTM, as illustrated in the supplement on the case study area (<http://www.hydrol-earth-syst-sci-discuss.net/11/C2076/2014/hessd-11-C2076-2014-supplement.pdf>).

About reliability of the landslide dates, the four landslide events occurred in the area and indicated period, can be found in local newspapers (see e.g. <http://gazzettadelsud.virtualnewspaper.it/gdsstorico/>). It seems noteworthy at this point to clarify that the four (regional) "landslide events" generally include widespread failures, i.e. of more than one slope.

These details and more data will be added to future versions of the MS.

[R#4]: *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?*

[A]: The choice of $\delta = 40$ and $A/B = 10$ m results from the analysis on a pre-event DTM of the landslides occurred on 1 October 2009; moreover triggering of cells with $\delta \geq 40$ and $A/B \geq 10$ m correspond to a significant portion of the Peloriani area,

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for which a warning should be issued. Furthermore, the validation test of Figure 5 of the MS indicates that the threshold is reasonable, as well as the comparison with the empirical threshold derived by Gariano et al. (2013) (see response to R#1).

References

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