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Interactive comment on "Debris flow initiation characteristics and occurrence probability after extreme rainfalls: case study in the Chenyulan watershed, Taiwan" *by* Jinn-Chyi Chen et al.

Anonymous Referee #2

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General comments

The paper deals with an empirical/statistical analysis of debris flow occurrence base on a variable the authors introduce, the rainfall index RI, defined as the product between the maximum 24-h rainfall and the maximum hourly rainfall of a rainfall event. It has a specific focus on changes of debris flow probability due to the occurrence of previous events (landslide reactivation); so, this makes the MS innovative at some level. The manuscript certainly fits within the scope of the NHESS journal. The methods are overall valid, though the rationale underlying RI is not that clear and supported (see specific comments below). The MS requires an overall revision of the language, as an

too much of symbols and acronyms are used, which makes it very difficult to follow. Finally, I suggest **major revisions** for the manuscript.

Specific comments

P2 L 3-5 These are quite strong statements on climate change impacts. Are the authors really sure that the cited papers allow to make these statements?

P2 L 9 ["... and increase the volume of loose debris within a watershed"] The paper analysis is based on the assumption that after an extreme event causing landslides, the probability of landslides increases as a feedback effect. I suppose that in other cases, the opposite may be observed, as the occurrence of landslides can bring to a stabilization of affected slopes, and thus a lower probability of subsequent initiations. The authors should discuss better this issue.

P2 L 28 ["...hydraulic design."] Authors should here add some literature on previous studies focused on the assessment of debris flow/landslide triggering return period. For instance:

M. Borga, G. Dalla Fontana, F. Cazorzi, Analysis of topographic and climatic control on rainfall-triggered shallow landsliding using a quasi-dynamic wetness index J. Hydrol., 268 (1–4) (2002), pp. 56-71

D.J. Peres, A. Cancelliere, Estimating return period of landslide triggering by Monte Carlo simulation, Journal of Hydrology, Volume 541, 2016, Pages 256-271, ISSN 0022-1694, http://dx.doi.org/10.1016/j.jhydrol.2016.03.036.

P. D'Odorico, S. Fagherazzi, R. Rigon, Potential for landsliding: dependence on hyetograph characteristics J. Geophys. Res.: Earth Surf., 110 (F1) (2005)

L. Schilirò, C. Esposito, G. Scarascia Mugnozza, Evaluation of shallow landslide-triggering scenarios through a physically based approach: an example of application in the southern Messina area (northeastern Sicily, Italy), Nat. Hazards Earth Syst. Sci., 15 (9) (2015), pp. 2091-2109

Bogaard, T. and Greco, R.: Invited perspectives. A hydrological look to precipitation intensity duration thresholds for landslide initiation: proposing hydro-meteorological thresholds, Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-241, in review, 2017.

Section 2. The data section lacks of some essential information: what kind of rainfall data was available (a continuous series? Hourly? Sub-hourly?), how was the rain gauge selected to compute the RI (the "nearest" rain gauge?)

P3 L18-20 the computation of the RI requires a criteria for identifying what is a "rainfall event". The authors should specify the criterion that they have adopted to single-out rainfall events from a rainfall sequence.

Figure 2. In this figure it seems that an "ad hoc" criterion has been used to plot the RI corresponding to events ("10 or more debris flows"). Since it does not seem that the RI has a physically-based/hydrological rationale, the authors should at least better prove if the RI works well in separating triggering and non triggering events. So: what happens if the "10 debris flow" threshold changes (e.g. to 5, or another number)? What happens if the RI index values for NON-triggering events are plotted?

Figure 3. It is unclear to which data points the curves are fitted (or where the curves come from)

Figure 4. It is unclear how this curve has been determined

P1 L18-20; P 6 L4-8; P6 13-15 : (not exhaustive) list of sentences difficult to follow because an excessive use of symbols and acronyms. Write more in terms of "concepts" rather than in terms of "symbols". Perhaps the authors should rewrite the MS with the support of a native-english writer

P 10 L 1: n is the number of years only if one value per year is in the sample (e.g. annual maxima data). From table 2 it seems that multiple values of RI can be present within a year. Please clarify

Technical corrections

P2 L 28 warranted is not appropriate. Perhaps use "needed"

P3 L28 replace "it had" with "they had"

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