

Interactive comment on “Regional rainfall thresholds for landslide occurrence using a centenary database” by Teresa Vaz et al.

Anonymous Referee #1

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The contribution “Regional rainfall thresholds for landslide occurrence using a centenary database” by Teresa Vaz and co-authors presents a procedure for the definition and the evaluation of rainfall thresholds for landslide occurrence, based on a huge database of landslide occurrence and a long series of daily rainfall data gathered from one rain gauge.

The paper is clear, well written and potentially publishable. It follows the IMRaD structure, with a fluent language; as a result, the manuscript is easily readable. Tables and figures are useful for a better understanding of method and results. Actually, the proposed approach is not very new but the use of such a big database is not common in the literature. Moreover, the analysis of the spatial representativeness of the rain gauge data series is well structured and brings novelty and interest to the paper. The use of

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a single rain gauge can be considered a drawback of the work, but I understand that with such a long series of data, it was the only way to present the proposed procedure.

My opinion is that the paper deserves to be published in NHES journal after some revisions. I have some comments and suggestions that should be addressed before the manuscript can be accepted for publication.

I have a main comment regarding the method proposed for defining the thresholds. I partially disagree with the point stated at page 6, line 11, where the Authors say: “The pair with the highest return period was considered as the critical rainfall combination responsible for triggering the landslide events”. This can be true, but not always, also from a physical point of view. Indeed, this can be seen also by inspecting Figure 6. In wide the literature concerning landslides and thresholds, there are several cases in which landslides are triggered by (or are associated with) rainfall events without considering their return periods. In addition, in some cases, the rainfall events associated to the landslide triggering are not very high (e.g., in terms of cumulated rainfall). Moreover, considering the rainfall return period to define the triggering rainfall events imply a somehow “linear” relationship between rainfall and landslide, and we know that this is not exactly true. On the other hand, I acknowledge that in any case and for any proposed method, a simplification is needed. Further on in the text (lines 16-18 and also in the discussion at page 14, lines 3-7), the Authors state that they used only the landslide events with critical rainfall combination having return period > 3 years. This point seems questionable too. Why do not use only landslides for which rainfall triggering is known? Even because the 3-year boundary is arbitrary, as acknowledged by the Authors. Moreover, I cannot understand how this selection may limit the possibility to exclude landslides not triggered by rainfall (e.g. human action). I think that a rainfall event with a return period lower than 3 years can induce a landslide, given that this is a process controlled not only by the triggering rainfall but also by other variables, like e.g. the antecedent rainfall, or the antecedent soil moisture conditions, and so on. As an example, a rainfall characterized by a (cumulated rainfall-duration) combination with

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a low return period can trigger a landslide if the antecedent conditions are worse (negative) for slope stability. Actually, this is my point of view, and I do not want to criticize or reject the method proposed in this work, but I want to stimulate a discussion about this point. Thus, after this long comment, I would ask the Authors to better justify this part of the procedure in order to make it more shareable.

Furthermore, in several parts of the text, the Authors state that they performed a calibration and a validation of the thresholds, by using – briefly resuming – years with landslides and years without landslides. In my opinion, this is actually only a calibration or, at most, an optimization, given that the same temporal rainfall and landslide datasets are used for constructing the contingency matrix. An independent validation could be performed by splitting the database in two non-overlapping periods and testing the performance of the thresholds (calibrated using the first subset) by verifying the number of TP, FN, FP, TN, and all the indexes considering the second subset, as done e.g., by Martelloni et al. (2012), Giannecchini et al. (2012), Segoni et al. (2014), Gariano et al. (2015), Piciullo et al. (2017). Thus, I suggest to better clarify this issue whether performing an independent validation or rephrasing the sentences by using calibration/optimization/optimal-definition/objective-definition instead of validation.

In Section 3.3, the method for drawing the lower and upper limit thresholds is not specified. I see the reference to the work of Glade and co-authors, but I would ask to add more details (e.g. how many pairs were used to draw the thresholds, how they were selected, . . .).

A section or a sub-section with the description of the landslide database is missing (even if some references are provided). However, I suggest adding a subsection (of sections 2 or 3) with a brief description of the landslide database. This subsection could contain, among the others, some information about the percentage of known/unknown types of the landslides, about the precision in the location of the landslides, about the temporal accuracy of the records, and so on.

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In Section 4.1, a description and a comment of the values reported in Table 2 is provided. Actually, the differences of the Ratio a/b seems very little: only in the second decimal place. This is not reported in the text, and I think that should be acknowledged. The same is for what reported in Section 4.2 (Page 10, Line 31) where Authors state that “regression thresholds, linear or potential can be used as acceptable thresholds to predict landslide events. . .”. Actually, this can be considered true, but only the index FAR returns very good values (as can be noted in Table 4). This should be acknowledged by the Authors too.

At the end of section 4.4, Authors state that “thresholds identified for the Lisboa-Geofisico rain gauge can be applied with reasonable confidence for an area within 50 km distance”. This sentence is very difficult to justify, and I think that the ratio a/b or the ratio FN/(TP+FN) cannot be used to justify that assumption. In my opinion, the number of TP and FN are related to the efficiency of prediction model rather than to the representativeness of the rain gauge. I suggest to better justify that sentence or to modify it.

In the discussion, a section/paragraph with a brief discussion about the possible application of the thresholds in an operational or prototypal landslide early warning system could introduce more appeal to the work, also in the perspective of the topic of the Special Issue. I list some works that could be mentioned at this regards: Aleotti (2004); Tiranti and Rabuffetti (2010); Segoni et al. (2015); Calvello et al. (2015); Piciullo et al. (2017).

I have also some comments about some words used in the text.

First, I would suggest using the word “method” (i.e., a systematic way of doing something, implying an orderly logical arrangement, usually in steps) instead of the word “methodology” (i.e., a system of methods followed in a particular discipline). Likewise, I would suggest using “type” (a subdivision of a particular kind of thing) instead of “typology” (a classification according to general type).

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Moreover, according to the words “precipitation” and “rainfall”, I would ask if the Authors know the type of data recorded by the Lisbon rain gauge. If it includes all types of precipitation (i.e., also snow) it is fair to use the term “precipitation”; otherwise I would suggest using simply “rainfall”.

Finally, I am somewhat dubious about using the word “quantity”, as for “rainfall quantity”. I would suggest the most used “cumulated rainfall”.

Moreover, some specific comments are listed below.

Page 2, Line 4: I suggest to add “and variables” after “rainfall measurements”.

Page 2, Line 10: I suggest to change “triggered during periods of rainfall of short durations” into “triggered by short and intense rainfall”.

Page 2, Line 31: Nikolopoulos et al. (2014; 2015) addressed the issue of rain gauge representativeness.

Page 4, Lines 3-5: this sentence is quite complicate and hard to follow. Please reword.

Page 5, Line 16: the equation is quite ambiguous. Please clarify it and add an example.

Page 9, Lines 19-20: I suggest to rephrase the sentence “The relationship between critical duration and month...” as “The monthly distribution of critical durations”. It seems clearer to me.

Page 10, Line 3: the sentence “however none of them ensure the false negatives occurrence” is not clear. Do the Authors mean “none of them ensure a low number of FN”?

Page 11, Line 2: does the PPr index indicate probability of landslide occurrence or probability of landslide prediction?

Page 14, Lines 11-13: in the cited works, the criteria used to select the representative rain gauges were based not only on the topographic distance between gauge and

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landslide, but also to elevation difference and morphological settings.

Tables

Tables 3, 4, and 5 could be merged into one figure (by transposing tables 4 and 5 and by merging them to table 3). They have the same number of rows and columns. Moreover, I suggest using only the acronyms (TP, FN, FP, TN, TPr, FPr, FAr, TS, PPr) instead of the entire names of these parameters and indexes.

In Table 3, I suggest to use a specific letter for indicating the variable of the rainfall in the threshold equation, e.g. “R” instead of “y”.

Figures

Figure 2.

I would suggest adding a moving average, other than the MAP line.

The red dots represent only the landslides in the 10 km buffer?

Figure 3.

In order to avoid misunderstanding, I suggest using another shape or another colour to symbolize the 30-day cumulative antecedent rainfall, instead of the red dots (used in figure 2 to represent the landslides).

Please correct “Dez” and “Mai” in the labels of x-axes.

Some technical corrections:

Page 3, Line 20: delete the space after the open bracket.

Page 5, Line 16: add a “+” before “...” in the equation.

Page 9, Line 14: delete “the” before “Fig. 3”.

Page 9, Line 16: correct “70 percentile” and “90 percentile”. I suggest to use “70th” or “70%”.

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Page 9, Line 27: delete “the” before “Fig. 6”.

Page 9 and followings (also in Table 3): I suggest to use a specific letter for indicating the variable of the rainfall in the threshold equation, e.g. “R” instead of “y”.

References (some of them are already cited in the manuscript)

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