Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-371-RC1, 2018

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The role of antecedent soil moisture conditions on rainfall-triggered shallow landslides" *by* Maurizio Lazzari et al.

Anonymous Referee #1

Received and published: 28 December 2018

GENERAL COMMENT

The manuscript prepared by Lazzari and co-workers addresses an important scientific and technical question, i.e. the prediction of rainfall induced landslides with the use of antecedent soil moisture conditions obtained from a hydrological model. The topic fits the scope of NHESS and is interesting for the readers. The work is based on a dataset of landslides occurred in a southern Italian region and adopts a hydrological model already presented in the literature. The work follows a relatively new research line in the definition of empirical/hydrological thresholds for the prediction of rainfall-induced landslides. The paper is well-structured, in a sufficient English language, and follows the IMRAD structure.

Despite the good intentions, I believe that the work is missing the goal, for a number

of reasons. I have found several lacks and drawbacks in the whole manuscript, which does not allow for its acceptance in the present form. Moreover, the conclusions are not supported by the obtained results. It seems to me that the Authors want to express strong criticisms to the "classical" methods used to calculate empirical rainfall thresholds for operative landslide prediction, but without operating the long, detailed and rigorous process needed for their adoption and definition.

My opinion is that the paper needs a strong improvement before being reconsidered for discussion and eventually publication. For this reason, I believe that now it should undergo through major revisions.

In the following, I list some specific comments and a list of technical corrections. All these comments should be carefully addressed and all the corrections should be done before the paper can be reviewed again.

SPECIFIC COMMENTS

First, the Authors start from a consideration about the "classical" approach for the definition of empirical rainfall thresholds. They state that it "is affected by a large number of false positives" (page 1, line 31 and elsewhere in the conclusions). This is not generally true. The number of false positives, as the number of false negative, is strongly related to the values (let me say, the height) of the rainfall thresholds. High thresholds produce many false negatives and few true positives, while low thresholds result in several false positives and limited true negatives. Therefore, the number of false positives is related to the values adopted for the definition of the thresholds. The definition of high or low thresholds is due to several issues.

There are classical thresholds that are low, therefore resulting in several false alarms, and other thresholds that are high, producing less false alarms. This should be acknowledged, also referring to some papers dealing with threshold validation. I suggest a huge literature review on this topic.

Second, regarding the landslides dataset, the Authors do not describe anything about the records. A description of several details would be very useful for understanding the quality of the used dataset. As an example, the description could be focused on: (i) the landslides types, (ii) the annual and monthly distributions of the landslides, (iii) their geographical distribution, (iv) the temporal (is the time or the day of occurrence known for all the records?) and spatial (are the coordinates of the landslides known?) accuracy in the identification of the landslides.

Moreover, always regarding the landslide dataset, the Authors often refer to “landslide events”. What does it mean? How these landslide events were defined?

Third, moving to rainfall, nothing is said regarding the reconstruction of the triggering rainfall events. Nothing is said about the selection of the rain gauges necessary to associate the rainfall data to the landslide trigger. Nothing is said about the separation of rainfall events and about their association to the landslides (even if the Authors cite in several places these “rainfall events”). This is a great drawback that should be solved and discussed.

The same goes for the reconstruction of the soil moisture conditions associated to the landslides. In the section describing the method, nothing is said about it. On the other hand, in the “result” section, Authors state that the degree of saturation is related to the start day of the triggering rainfall events (whose definition is murky). This is another key point that should be better presented in the “method” section.

Fourth, regarding the AD2 model, nothing is said about the all the variables and parameters reported in equation 1. As an example, how the evapotranspiration is calculated/estimated? What about the infiltration? This is another point that should be better discussed.

Fifth, since nothing is reported about the rainfall data and rainfall events, it seems – looking at figure 2 – that daily rainfall measurements are used. This is not feasible when working with shallow landslides (as reported by in Section 2). On the other hand,

Authors state that detailed measurements are available (page 2, line 26). This issue should be clarified and discussed.

Sixth, in the results, the Authors state that they were able to “derive critical rainfall threshold functions” (Sections 3 and 4). However, the only threshold function visible in the manuscript is the equation 2, reporting the general form of an intensity-duration threshold. Then, in Table 1, one can read only a list of numbers (named “rainfall thresholds” by the Authors) for which not even the unit of measurement is provided. Sincerely, I do not understand how this list of values (of what? This is not clear) could be considered rainfall threshold functions. This is another key point that needs to be improved and discussed.

Finally, the “Conclusions” section is written in a way worse than the rest of the text, with several repetitions (see e.g, page 5 lines 6-7 and lines 21-22; line 14 and line 27; lines 17-19 and lines 30-32). It should be completely rewritten with more accuracy. Moreover, in my opinion, is full of wishful thinking, not supported by the obtained results. The Authors state that “the calculation of soil saturation should be the first step for an effective prediction of real-time landslides risk decreasing the uncertainties tied to the application of the rainfall thresholds only”. This is a strong statement (and I could also agree with this aim) but it is not supported by any other statement, and not even by the obtained results. How the proposed method can be implemented in an “operative” landslide warning system? How the uncertainties can be reduced? Please note that the uncertainties related to the presented method are not even evaluated in the paper. Therefore, I cannot understand how this can reduce the uncertainties in the whole process of landslide prediction.

I have some other more specific comments, which are listed below.

Page 1, line 19: I would suggest adding a final statement in the abstract to describe the results and main findings of the work.

Page 1, line 24: there are some papers describing global datasets of landslides induced by rainfall, even with fatal consequences, that should be mentioned.

Page 1, line 26: please use definitions more precise than "intensity-duration, event-duration and event-intensity thresholds". As an example, "intensity-duration" could be "rainfall mean intensity-rainfall duration". Furthermore, does "event-duration" mean "cumulated event rainfall-rainfall duration"?

Page 1, line 28: I would remove the self-citation here, since the mentioned paper by Lazzari et alii is not a review paper like the other two cited works. The reference to the work by Lazzari et alii could be moved elsewhere.

Page 1, line 30: please note the classical thresholds are based on "rainfall" measurement, not "precipitation" (e.g., usually snow or hail are not considered).

Page 2, line 12: please define "landslide event" and "rainfall event".

Page 2, lines 24-26: I would move this description of the hydro-meteorological network at line 22, after the description of the precipitation regime and before the description of the landslide dataset.

Page 3, lines 26-27 and page 4, lines 1-2: this part should be moved to the method section.

Page 4, lines 8-11: this sentence is quite vague. I see the point, but I believe that could be improved.

Page 4, lines 18-21: here the Authors state that the correlations found are "clear". However, I can't grab it. From the figure I can just see some best fit lines (I guess) without any correlation coefficient useful to quantitative assess the goodness of the correlation. This point should be better discussed.

Page 4, lines 30-31 and page 5, line 10: what about for relative soil saturation equal to 0.7?

Page 5, line 1: here the Authors refer about three levels of probability equal to 0.6,

0.75, and 0.9. I do not understand: (i) how the probability is defined and to what is related. (ii) how the three levels are defined and why these values are chosen.

Page 5, lines 6-33: please read and re-write accurately the whole paragraph, since it is full of repetitions of sentences, in some cases exactly alike. Please add the main findings of the work and how they can be used quantitatively for operative landslide prediction.

FIGURES AND TABLES

Figure 1.

The figure is very dark and very difficult to read. I would suggest using colours or a brighter DTM.

Moreover, I would suggest deleting the table with the names of the stations and the labels of the stations in the map. They are not useful to the discussion. Just leave the indications of the three sites. Please add scale bar and coordinates.

On the other hand, a similar figure with the distribution of the 326 landslides would be useful.

Figure 2.

I would suggest using the same maximum values on the y-axes for the three panels.

Moreover, I would suggest using the labelling "d/m/y" for the x-axis and adding the final date.

As already mentioned, I cannot understand the use of daily rainfall, in particular given that more detailed measurements are available.

Figure 3.

The caption of the figure says "Rainfall intensity/duration" and the label of y-axes "Rainfall I/D (mm/h)". The unit of measurement is related to an intensity. So, is it just rainfall intensity? Please explain.

Please correct “landslide events” in the caption.

Moreover, are the lines reported in the figure the best fit lines of the point clouds? If yes, please add the equations and the values of the correlation coefficients. If not, please explain what they are.

Finally, for an optimal representation of the different groups of rainfall durations, I would suggest to use different colours (the same of the lines) for the points pertaining the each of the three groups.

Figure 4.

What about for relative soil saturation equal to 0.7?

Does the x-axis represent the duration? In any case, use only “h” instead of “hours”. Please add “s” in the caption. There are two squares in the right part of the figure. What they are?

Moreover, are the lines reported in the figure the best fit lines of the point clouds? If yes, please add the equations and the values of the correlation coefficients. If not, please explain what they are.

I appreciate the use of the colour scale for indicating the different values of soil saturation; however, I suggest adding the description in the caption and not only in the text. Finally, for an optimal representation of the figure, I would suggest to use different symbols (e.g. circles and squares) to represent points with soil saturation higher or lower than 0.7 (but please consider also values equal to 0.7).

Table 1.

Please explain what the numbers included in the table represent. Are they values of rainfall mean intensity? Are they values of cumulated event rainfall? Please explain and add the description in the caption.

What are H1, H2, and H3? Please explain.

Please use only “h” instead of “hours”. Please use point as decimal separator. Please justify the use of two decimal places.

TECHNICAL CORRECTIONS

Page 1, line 13: I would replace “rainfall intensity/duration” with “rainfall mean intensity and duration”.

Page 1, line 15: please avoid repetition of “critical” in the line. Please delete “the” before “critical rainfall thresholds”.

Page 2, line 3: please define “I/D”.

Page 2, line 4: please replace “Mirus et al. 2018a and 2018 b” with “Mirus et al. 2018a, b”. Also at page 4, lines 9-10.

Page 2, line 5: I would replace “regional landslide thresholds” with “regional thresholds for landslide prediction”.

Page 2, line 11: please replace “record” with “dataset”.

Page 2, line 12: please add “January” before 2001.

Page 2, line 19: please use “types” instead of “typologies”.

Page 2, line 28: please correct “course”.

Page 3, line 2: please note that “Farmer et al. 2000” is reported with the year 2003 in the reference list. Please check.

Page 3, line 3 and line 6: I would suggest using the most common acronym “ET” to refer to the evapotranspiration, in order to avoid confusion with E – evaporation.

Page 3, lines 4-6: please check the subscripts of the variables.

Page 3, line 20: I would suggest using “methods” instead of “methodologies”.

Page 4, lines 3-5: please correct the punctuation in the sentence.

Page 4, line 5: please note that in the text the date is “31/12/2015” while in the Figure 2 (and caption) is “31/12/2016”.

Page 4, line2 6-7: please avoid repetition of “occurred/occurring”.

Page 4, line 11: please correct “show”.

Page 4, line 13: please correct “Bogart” into “Bogaard”.

Page 4, line 16: I would suggest using “rainfall events” instead of “rainy events”.

Page 4, line 23: is the “rainfall intensity” the “mean rainfall intensity”?

Page 4, line 23: please correct “are associated the simulated. . .”

Page 4, line 26: please replace “threshold” with “thresholds”.

Page 5, line 6: please change “thresholds triggering shallow landslides” into “thresholds for the triggering of shallow landslides”.

Page 5, line 7: please correct “build”.

Page 5, line 11: please replace “class” into “classes”.

Page 6: please add DOI where available.

Page 7, lines 13-14: I’ve found this paper with a different title.

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