Natural Hazards
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Discussions

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

Maurizio Lazzari et al.

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Dear Editor, here is a note of reply to the comments of the reviewers. We thank the referees very much for their hard work and constructive criticism. Many observations are worthy of consideration and will allow us to improve and rebalance different parts of the work. At the same time we must point out that being this a short communication, we had to summarize many of our sentences resulting in lack of information deemed necessary by the same referees. Based on your indication, here we don't provide a revised manuscript, but we just reply to ACs and SCs comments by following the suggested structure ((1) comments from Referees, (2) author's response, (3) author's changes in manuscript). We have effected a fusion of point (2) and (3) into one.

Reply to Mirus (Referee 2)

My primary concern when evaluating the technical merit of this work is the lack of details available about the data, modeling, and analysis used to develop the thresholds and arrive at the conclusions. Therefore, the manuscript should undergo major revisions to address the following general question and issues before the work can be evaluated further.

Comment 1. Information is needed about the size of the field area and the actual variables measured.

Reply 1) Please refer to replies 1,2,3 to referee 1

2. Details are missing about the model equations, input/output, physical parameters, parameterization or calibration, and spatial and temporal resolution. The soil water balance has been published elsewhere and the mathematical equation can be found on the manuscript by Manfreda et al. (2017). We consider useless to include all the details of the model in a short communications. The aim of the manuscript is not to validate a physically consistent hydrological model, but it to identify the value of the antecedent soil moisture condition in landslide predictions. Reviewer can argue that as long as the model is not calibrated the result are not valid, but it would be hard to validate a model like this with soil moisture measurements that are not available anywhere in the world. So, the only option available is to reconstruct numerically a reference value of the antecedent soil moisture using all the physical information available. This is exactly what we have done. We may agree on the fact that critical functions should not be considered for operative purposes, because the soil moisture values are not validated. Nevertheless, the value of the research is mainly of the potential of including this information in the actual methodologies rather than in the operative value of the model.

3. The simulated soil moisture does not seem to be compared to any observed soil moisture data, so the accuracy of the model output is highly uncertain. Please refer to

the above comment.

4. It is unclear how the various antecedent saturations were quantitatively evaluated to select the significant value of 0.70. This value was set based on a sensitivity analysis that leaded to the two distinguished sub-samples. We agree with the reviewer that this choice will be better emphasized in the final version of the manuscript.

5. It appears that the thresholds for >0.70 and <0.70 were optimized by identifying a best-fit line through the landslide data, rather than identifying a threshold that distinguishes between landslide and non-landslide events. This is highly unusual and needs to be justified. We agree with the reviewer. The functions reported in the graph are not the threshold functions associated to a given return period and are not based on any optimization process. The functions are just regression linear functions that have been plotted to describe the dependence of triggering rainfall threshold from antecedent soil moisture.

6. Furthermore, there does not appear to be any quantitative analysis of the threshold performance for predicting landslide events, such as ROC analysis or other statistical metrics. We agree also on this point. The manuscript only want to emphasize the role of antecedent soil moisture on the rainfall triggering events. The subsequent step will be the development of a landslide prediction model that will be calibrated considering both landslide and non-landslide events. Any reference to operative methods will be removed from this manuscript for sake of clarity.

Specific comments. The abstract needs to focus more on the actual study and results. The motivation is important, but an informative abstract should include a clear description of the methods and state the primary contributions.

Reply 7) The abstract will be rewritten taking into account both the referees concerns

P1,L11. Preventing landslides seems like a nearly impossible goal and beyond the scope of this study, but reducing the losses and impacts is more achievable through

developing better landslide thresholds for early warning. That is the focus of this work.

Reply 8) We agree with the referee comment and we will adjust the paragraph.

P2,L1&4. The Mirus et al. 2018a,b references might be more appropriate to cite in line1 as we actually developed new thresholds that improve predictive capabilities, though they do not explicitly consider rainfall I/D. Also consider adding Thomas et al., 2018, Geophysical Research Letters, doi:10.1029/2018GL079662, which uses a deterministic approach with infiltration simulations to identify rainfall-saturation thresholds. P2,L8-9. Thomas et al., (2018) also does a nice job of quantifying the sensitivity of thresholds for different hydraulic and strength properties in relation to rainfall-saturation thresholds, though it doesn't deal with I/D directly. Godt et al., 2006 directly examines how antecedent moisture index affects the accuracy of an I/D threshold. It is not clear how/why your objectives should be distinguished from these prior advances.

Reply 9) We will rewrite the paragraphs by following the referee suggestions and by also considering the new insights of Thomas et al., 2018. This last paper will be cited in the Reference list

P2,L9. Question seems repetitive. To put it more simply: "How does the initial saturation impact ID thresholds?" However, upon reading the analysis, it seems it would be more accurate to state that the study "evaluates correlation between antecedent saturation and rainfall intensity during landslide events."

Reply 10) The statement will be rewritten

P2,L24. Consider providing the inventory with dates here and a table or link to appendix. Plotting landslide locations on the map in Figure 1 would also be great, though perhaps too busy. Figure 1. A scale bar or lat-long coordinates are needed for readers who are not familiar with this region.

Reply 11) The Figure 1 has been redrawn by taking into account both the referees comments

P2,L26. Specifically, which meteorological variables are measured? At what timescale? P3,L3. How are the variables calculated in equation 1? Is the model 1D or distributed in 2D or 3D? What is the spatial and temporal resolution of the model application? P3,L7. What are the physical parameters?

Reply 12) Some additional information about the hydrological model will be included in the revised version of the manuscript. Just for clarity, the model is a conceptual model applied at the catchment scale, its parameters are associated to the soil texture, and it is applied at the daily scale.

P4,L17. The statement ". . . a linear decreasing trend between the rainfall thresholds and initial soil moisture. . ." is somewhat confusing because it is unclear whether Figure 3 intended to illustrate possible thresholds, or merely the decreasing linear trend between rainfall ID and antecedent saturation for landslide events? Are these colored lines in Figure 3 supposed to be thresholds or merely plots that relate ID to antecedent saturation during landslide events? The y-axis is labelled I/D, but with units of mm/h. It seems that different I/D lines are plotted for uniform duration and different intensities, but the data portion of the plot is unclear. Are the dots data? Are they plotted for a specific timescale/duration? If these are thresholds, they aren't very useful as it seems that about half the landslide events are below the lines.

Reply 13) The objective of Figure 3 is to show the behaviour of rainfall thresholds in function of initial degree saturation and the event duration. The colored lines plot the relation between ID and antecedent saturation during landslide event. Thus, as the referee marked, the units of mm/h is wrong (it is mm).

P4,L18. What is "maximum rainfall" in this context?

Reply 14) It is an error. It simply refers to rainfall

P4,L27-28. How did you test different saturation thresholds to arrive at the value of 0.70?

Reply 15) We performed a sensitivity analysis in order to identify the threshold able the better distinguish between the two groups of events. This point will be better discussed in the manuscript.

P5,L1. Probability of what? Revise for greater clarity. Table 1 is not clear what the probabilities apply to. What does H1,H2,H3 represent?

Reply 16) Please refer to reply 5 to referee 1

Figure 4. Should label that the color bar represents antecedent saturation. I note that for longer duration storms the two thresholds cross, which means that longer storms are more likely to generate landslides when the soil is dry. I cannot imagine a reasonable physical explanation for this, which is problematic.

Reply 17) The colorbar represents the relative soil saturation ranging from 0 to 1. The two functions plotted in the graph have significantly different slopes. This implies that they must cross somewhere in the space of rainfall intensities and event duration. In the present case, we observe that they cross in a point corresponding to duration of about 200h. At such duration, the impact of antecedent soil water content becomes not relevant and this part of the curve should not been considered. This is a very good point that will be also mentioned in the discussion of the manuscript.