Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-371-SC1, 2019

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.

Gianvito Scaringi

g.scaringi@qq.com

Received and published: 10 February 2019

Dear authors,

I've read your manuscript with interest, both because it concerns a research topic that is relevant to my studies, and because a landslide-rainfall inventory from my native region has been employed to validate the model you proposed.

I must say that I agree with the remarks of the two reviewers about the format of your submission. In fact, I believe more details should be provided to understand and discuss your model, that cannot be contained by a short communication and seem better suited for a full research paper. Alternatively, I may suggest to prepare a substantial supplementary information file, to be attached to the paper, in which all the relevant

details about the landslide-rainfall inventory and about the model can be presented exhaustively.

About the model itself, I believe that the success of I/D thresholds resides in their simplicity and in their empirical nature, so that large amounts of data are readily available for calibrating them, and monitoring data can be used straightforwardly in near real-time early warning systems. On the other hand, I/D thresholds do not say anything, directly, about the actual mechanism of slope failure that leads to the occurrence of landslides of any type.

I believe, and I agree with you in this, that including information on soil moisture obtained from records of antecedent rainfall is one possible strategy to move from fully empirical to at least partly physically-based models, while maintaining the simplicity and immediacy of empirical-only models. On the other hand, I am sure the authors are aware that there is no straightforward connection in most cases between changes of soil moisture and slope instability. Soil moisture is not only markedly variable in space on the slope surface, but might also present significant gradients with depth.

Furthermore, slope instability does not occur, in most cases, as a bulk instability, i.e. as a complete collapse of an entire soil or rock column. Especially in Basilicata, a region characterized by abundant clayey outcrops, the occurrence of this mechanism is probably limited if not absent (while it is typical, for instance, in loess formations, or in loose coarse deposits). On the contrary, slope failure in soil slopes occurs through strain localization at a specific depth (that depends on local geometry, lithology and structure of the slope, and pore water pressures/suctions and soil moisture distribution) which causes the formation of a continuous shear zone or surface, and results eventually in landsliding.

Of course, I understand that the model must be simplified when applied at regional scale, but at the same time I wish that the authors include some discussion about this point: i.e., when you speak of soil moisture, how do you relate it to the hydro-

mechanical condition in the subsurface, where the strain localization that initiates landsliding actually occurs?

One additional remark I wish to make concerns the actual relevance of changes of soil moisture to landsliding. In fact, for landslides in which the shear zone is located even just a few meters deep, and which occur in clay-rich materials (which, again, is a common condition in Basilicata), the variation of soil moisture below the first 1-2 metres from the surface might be small or negligible throughout the hydrological year, and the shear zone might be always fully saturated. In such case, changes of soil moisture above the (potential) shear zone only have a limited effect on the stress state of the shear zone material, by changing the weight of the (potential) landslide body. However, this is only a marginal reason for landsliding, the most important one being the loss of suction or the increase of pore water pressures, that cause a decrease in the effective stress and consequently a decrease in the available resistance to shearing. I think this is a point worth of discussion for the significance and applicability of the model, also in relation to the landslide data set employed for its validation.

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