Hydrol. Earth Syst. Sci. Discuss., 10, C1292–C1294, 2013
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Physically based modeling of rainfall-triggered landslides: a case study in the Luquillo Forest, Puerto Rico" *by* C. Lepore et al.

R. Greco (Editor)

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Received and published: 29 April 2013

Dear Authors and Referees,

first, thank you all for the efforts you put in the discussion.

The topic of the manuscript is surely of interest for the readership of HESS, and it perfectly fits with the scope of the Special Issue about Landslide Hydrology.

After the comments made by the Referees, my reccommendation is that major revision is needed before the manuscript becomes suitable for publication in HESS.

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Interactive Discussion

Discussion Paper

My own impression is that the Authors should more clearly state the aims of their study, which seems to me more about investigating the potential effects of some of the considered hydrological processes upon slope stability, rather than showing how their model is capable of predicting time and location of slope failures. Such impression is suggested to me by the statement at page 1345, lines 10-12 ("For simplicity and to ensure the occurrence of a useful number of failures, this study will assume spatial homogenous values of cohesive strength (3 kPa) and friction angle (25 degrees) over the entire basin."), and by the way the results are presented (no comparison with actual activation, neglection of the effects of some parameters which may significantly affect slope equilibrium, etc...). If I am right and this is the real aim of the paper, the Authors should better clarify it at the end of Section 1 (Introduction), and maybe also a change of the Title of the manuscript could help.

Conversely, if the aim of the study is the prediction of landslide triggering in the studied basin, then the description of the results and the conclusions should be completely rewritten.

More in detail, these are the main issues raised by both Referees #2 and #3 (and confirmed, although more slightly, also by some of the comments of Referee #1), which should be carefully addressed in the revised manuscript:

- A more detailed description of the adopted distributed model should be given, with special emphasis on boundary conditions (Ref #3) and on how lateral distribution of water is introduced in a basically 1D model (Ref #2 and Ref #3), as they may significantly affect the obtained FS spatial distributions (this point is even more important owing to the discussion about the effects of anisotropy on landlside triggering).

- All the Referees agree that some more details should be given about the simplifying assumptions made about soil physical characteristics: it seems that while the Authors distinguish the four different soils types from the hydraulic point of view, they don't do the same with the geotechnical parameters. As both the aspects affect the equilib-

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rium conditions, it would be worth to give them the same importance, at least in the discussion.

- A clearer discussion should be given also about the factor "chi" accounting for the contribution offered by soil suction to the equilibrium of the slope, which in general depends on the degree of saturation and here is assumed to coincide with it.

- All the Referees point out that model validation suffers from the lack of data about actual landlside activations. Ref #2 and #3 point out that more details about the experimental data used for the validation should be provided (position and dimensions of the probes etc...).

I'm looking forward to receiving the revised version of the manuscript.

Kind Regards, Roberto Greco (Guest Editor)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 1333, 2013.