

Dear Editor,

Dear Authors,

I reviewed the manuscript HESS 2012-512 thoroughly. The manuscript describes the upgrade of a physical-based model (tRIBS-VEGGIE) for the hydrological and stability analysis of rainfall-triggered landslide and its application to a basin in Puerto Rico where in situ measurements were available. The manuscript is very well written and structured and should eventually be published even though, I think that the following main shortcomings should be addressed.

- 1) The title, the abstract and the introduction highlight that the work is devoted to the development of a physically-based model for rainfall-triggered landslide analysis. However, the description of the tRIBS-VEGGIE model (section 2.1) is very poor. In detail, the modelling of the lateral moisture transfer among the elements is not clear and, because of its great role in the shown analysis, it has to be commented in detail. Similarly, the VEGGIE module is only qualitatively described but the governing equations are not shown.
- 2) The authors stated that the work is devoted to the stability analysis of rainfall-induced landslides and at this aim they introduce within the tRIBS module a modified form of the FS equation able to take into the account the positive effect of matric suction on shear strength. However, in the stability analysis performed they do not differentiate, from a mechanical point of view, the three different soil types (which range between a fine-grained soil and a coarse-grained soil!) and they assume that all the slopes are in homogeneous soils (absence of stratigraphic sequence). At basin scale this latter assumption could be accounted for simplicity by considering the predominant soil type (as the authors do), but the first assumption is not acceptable since they should know that even small changes in cohesion, or even in the friction angle, can determine remarkable variations in the safety factor. This implies that all the results shown in terms of FS are invalidated by this assumption. As a matter of fact, did the authors verify if some slope failures have been triggered by the rainstorm of April 2008 and if their localization correspond to the black pixels in their analysis (figures 7 and 8)?

An effort should be made in order to introduce the variability of the mechanical properties of the soils even with data from literature referring to soils type characterized by grain size distributions similar to those of the soils under investigation. Differently, they should clearly declare, modifying the title, the abstract and, in general, the paper, that the analysis is devoted only to analyse the effects of hydraulic characteristics, with particular reference to anisotropy, on slope stability.

- 3) The model validation (section 5) is not so clear in my opinion. A number of points should be clarified.
 - First, the saturated conductivity anisotropy ratio a_r adopted in the analysis (100-300) seems unrealistic: usually the variability of this parameter in nature is in the order of 2-10. Could the authors refer to data from literature to support this choice which deeply influences the analysis? Or such high values have been introduced exclusively to reproduce the in situ water content measurements? With this respect, as already pointed out, the authors do not provide any detail about the model of the lateral fluxes. This makes the reader suspect that such extremely high anisotropy ratios are due to the adopted assumptions. Moreover, it could be also assumed that such high values of a_r are necessary to simulate the presence in the stratigraphic sequence of a soil layer more permeable than the modelled one. Are some borehole investigations available for the instrumented sites? In this case, please add this information along with a synthetic description of the soil-layers.

- The performed back-analysis of the in situ water content measurements is not clear. The measurements have been taken at three locations, each instrumented with three TDR probes installed at the same depth of 30cm: so, at each location, the authors have three water content measurements representing the spatial variability that this parameter assumes. Is it correct? Is each location along a slope or in a flat area? In the first case, should the variability of the water content be representative of the different position of the devices along the slope?
 - in describing the “validation exercise” the authors state that the saturated hydraulic conductivity and the anisotropy ratio are the most important parameters in order to reproduce the observed data (page 15, lines 1-9). Did they perform a sensitivity analysis to say that? In this case, please describe it clearly. Otherwise comment the sentence. Finally, what does the sentence “The different simulated series are obtainedkept constant” mean? (page 15, lines 17-19). I’ve deduced that the soil type is the same, hence the hydraulic characteristics do not vary in the three simulations, but different values of a_r have been adopted to reproduce the measurements time series. Is it true? In this case this seems in contrast with the previous sentence (page 15, lines 4-5) where the authors say that “several simulations were run varying saturated hydraulic conductivities.....100 and 300”. Please clarify and give the adopted value of a_r for each simulation.
- 4) Finally, information about unsaturated hydraulic characteristics of the modelled soils are completely missing (section 4): how did the authors model the water retention characteristics curves of the soils and the unsaturated conductivity functions? Did the authors assume anisotropy coefficients for the k_{unsat} equal to those assumed for k_{sat} ? Please detail and comment on it. The parameters reported in table has to be verified: please, check the unit for k_{sat} and add in the text what ψ and λ should represent. Then, please insert a column with the range of variation of k_{unsat} .

Specific comments

Section 2

Page 6, line 3. The tRIBS-VEGGIE model is defined as an “eco-hydrological” model. I have not found anything in the manuscript with reference to “ecological” aspects of the investigated phenomena.

Page 6, lines 26-29. The statement “The dynamics of each computational element via the coupled energy-water interactions” should be better explained introducing the adopted equations.

Page 9, lines 2-3. In the equation (3) ψ is described as matric suction (assuming positive value) in unsaturated conditions and pressure head (once again assuming positive values) in saturated conditions: such a way the third term at second member in eq. (3) always assumes a positive value, whereas in saturated conditions the presence of positive pore pressures acts decreasing the safety factor of the slope. Please verify.

Section 4

In describing “surface data and parameters” no information is available regarding the thickness of the soil deposits and the depth and nature of bedrock. These information are very important

when hydrologic and stability analysis are faced. The authors should add these information if available or the assumption made in their regards.

Page 12, lines 24-27. Is the saturated hydraulic conductivity assumed linearly decreasing with depth? Did the authors assume the same for unsaturated hydraulic conductivity?

Page 13, line 10. The sentence "...to ensure the occurrence of a useful number of failures.." is obscure. Please clarify.

Page 14, lines 1-2. Please explain how the cohesive effect of roots have been incorporate into the soil cohesion term since it can strongly affect the FS in the first 40cm of the soil deposit.

Section 5

What did you assume in terms of initial conditions? Have you done some hypothesis on the position of the ground water table?

Section 6

Page 16, lines 9-11. Why did you use a_r coefficients (equal to 300 and 500) highly greater than those adopted for the validation of the numerical model?

Technical comments

Page 6, line 10. Insert "resolution" after "very fine temporal".

Page 14, line 23. Fig. 2a not 1a.

Page 16, line 21. Fig. 2b not 2a.

Across the entire section 6 there is no correspondence between the time chosen to describe the analysis (t_a , t_b , t_c , t_d and t_e) in the text and within the figures. Please check.

Table 1. In the caption the authors refer to two meteorological stations but in the text only the Bisley tower is mentioned. Please check.

Figures 7 and 8. Enlarge the contour labels.