

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

Anonymous Referee #3

Received and published: 17 April 2013

SUMMARY

This paper presents the application of a distributed hydrological model, called “tRIBS+VEGGIE”, to a small catchment (16.7 km²) in the island of Puerto Rico. Among many other processes, this model simulates the soil water flow in the direction normal to the soil surface by a numerical integration of the Richards equation. Thus, the model also simulates the temporal evolution of soil pressure head profiles at element scale.

The paper shows that the simulated soil pressure head profiles can be employed to assess a factor of slope stability (FS) according to an infinite slope stability model, both

C872

in saturated and unsaturated soils.

GENERAL COMMENT

The paper is very well written. However, I think that there are some shortcomings that need to be addressed and I suggest a revision.

The innovative aspect of this paper is the application of a distributed model for assessing the hydrological conditions affecting slope stability in the vadose zone, even when the failure surface occurs in unsaturated soils. Thus, I think that the paper should provide a complete description of the model and the strategy employed for its implementation and parameterisation, by focussing on those modelling issues that are relevant for predicting the hydrological conditions leading to landslides triggering.

Despite the title, the case study is of limited interest, since the simulated results are not supported by an adequate field data set. A large part of the paper is devoted to the discussion of simulated FS values obtained after a strong simplification of the spatial variability of the soil properties. Simulated FS values (or at least the predicted failing elements) are not compared with observation data of landslides events.

SPECIFIC COMMENTS

SECTION 2.1

I understand that the study does not develop a new model rather it applies an existing one. However, more information about how the model is implemented and parameterised should be provided to the readers.

According to cited references (Ivanov et al. 2008a), tRIBS+VEGGIE includes several modules, simulating several state variables and fluxes, with several parameters to be identified.

Are all model parameters reported in the paper? How relevant are these parameters and the corresponding model components for the prediction of the slope stability?

C873

It is not clear how the Richards equation is integrated, how the boundary conditions are defined and at which depth the bottom boundary condition is located.

The bottom and surface boundary conditions can have a significant impact on the soil pressure head profiles and thus on the stability condition.

For what concerns the surface boundary condition, it is not clear how runoff is simulated, if runoff infiltration is simulated and if the numerical integration of the Richards equation manages Dirichlet type boundary conditions imposed by the surface water depth where runoff occurs.

From Ivanov (2008a) I could understand that the hydraulic functions are defined according to Brooks and Corey (1964). This is quite important for a proper interpretation of the soil water content and pressure head profiles, particularly at pressure heads larger than the corresponding air-entry value.

I also read in Ivanov et al. (2008a) that lateral fluxes are simulated by introducing additional source terms in the Richards equation. However, no details I could read about this aspect, which is essential for understanding the role played by the "anisotropy ratio" and the results discussed in sections 5 and 6.

SECTION 2.2

Eq. 3 - P. 1340 L. 26. Parameter "gamma_w" should be the specific weight of the water and not the water density.

Eq. 3 - P. 1340 L. 1-3. Variable "psi" is defined prior and after Eq. 3, with slight differences. To avoid any confusion, the sign of the third term of Eq. 3 can be changed and "psi" can be univocally defined as "pressure head".

Eq. 3. - The soil depth "h" is not defined in the paper. Please, also note that Eq. 3 is written assuming that the soil depth is measured along the vertical direction, while, according to Ivanov (2008a), the Richards equation is integrated in the direction normal to the soil surface. Could be this difference relevant on steep slopes?

C874

Eq. 3 and P. 1344 L20-23. Parameter "gamma_s" is not defined in the paper. Please specify how "gamma_s" is computed. To my knowledge, "gamma_s" is the depth average unit weight of the soil column above the examined depth, and it should change as the soil water content profile changes (unless additional approximations are applied). These changes can be relatively important for the computation of FS if the bulk density of the soil is low, as indicated for the first 300 mm at P. 1344 L20-23.

Actually, the statement at P. 1344 L20-23 is not clear. What is the bulk density employed in the model simulations?

The large variability of the bulk density is not consistent with the assumption of uniform soil water retention properties and uniform soil mechanical properties.

P. 1346 L. 1-4. The paper does not describe the model employed for simulating the cohesive effect of the roots.

SECTION 4

In many points the paper is rather vague about how the model is implemented and applied:

P. 1344 L24-25. How does the saturated hydraulic conductivity change with depth?

P. 1345 L.10. Please, explain "...to ensure the occurrence...".

P. 1345 L. 27-29. How is the root water uptake parameterised?

Table 2 – The units of "K_s" should be corrected. Given the value ranges for "K_s" in the table, what are the actual values employed for the simulations? Please, also change the symbol indicating the air entry value ("psi" is already employed for indicating the pressure head).

SECTION 5

The strategy employed for validating the hydrological model is not clear.

C875

For the validation, the distributed model has been applied to a smaller basin “for faster simulation time and a finer mesh” (page 1346 L.22). I understand that the Authors aimed at reducing the scale mismatch between simulation and observation support scales. However, the Authors should provide more evidences about the validity of their approach.

Due to the well-known scale effect issues, the results of a distributed model at elemental scale are often highly influenced by the spatial resolution with which the model itself is implemented. Validating the results of a model applied with a finer computational mesh does not necessarily provide relevant information about the model performance with a coarser computational mesh.

Why only three out of nine observed soil moisture time series (shown in Fig. 3a) are compared with the simulated ones? What are the parameters employed during the model validation? Do the simulated time series in Figs. 3c, 3d and 3e correspond to three different anisotropy values and what are the relevant values? What are the “different simulated series . . . obtained by varying the anisotropy ratio” (P.1347 L. 17-19)?

How is the terrain in the location where soil moisture probes are located (i.e., slope, upslope contributing area, curvature. . .)? Is the lateral flux expected to be relevant in the locations where the soil moisture is observed? What is the length of the CS616 probes? How were inserted (vertically or horizontally)?

It is advisable to add some quantitative evaluations (compute indices of the differences between simulated and observed soil moisture) to support the qualitative statement that “the model does a very good job”.

P. 1347 L. 10-13. Provided that validation data are rather scarce, the effect of the spatial resolution on the predicted slope stability could be one focus of the study.

P. 1347 L. 22. “. . . the maximum value of the soil moisture”. This is not so relevant pro-

C876

vided that soil water content is a bounded variable and the maximum value coincides with the saturated soil water content (P.1347 L.3-4).

P. 1348 L. 3-6. Do the Authors really expect that the differences between the observed and simulated values can be solely attributed to the uncertainty in the anisotropy ratio? The statement at page 1348 L. 3-6 is not supported by the results presented in section 5.

SECTION 6

In Section 6.1, the criteria employed for the selection of the three elements are not clear. Beside the slope, I think it is important to mention other terrain attributes of the elements which are relevant for the simulated lateral flow (e.g., curvature, upslope contributing area, etc.). The computed FS values are of limited interest per se, since no validation data (e.g. data of landslide events) are examined and a strong simplification has been adopted for the soil mechanical properties. Similarly, I do not think that it is relevant to compare FS values of different elements characterised by different slopes. I would rather compare the evolution of the dimensionless ratio $(\gamma_w \cdot \psi \cdot \chi) / (\gamma_s \cdot h)$ which describes the effect of the soil suction in FS, applied to elements characterised by similar terrain attributes. Note that the product “ $\psi \cdot \chi$ ” can be univocally defined as function of “ ψ ”.

It is difficult to follow the discussion of figures 4-6: please check the correspondence between the times “ t_{b-t_e} ” cited in the manuscript and in the figures.

Anyway, it would be more appropriate to analyse FS profiles by examining the temporal evolution of pressure head rather than of the soil moisture content, particularly for saturated soils or soils close to saturation. The soil gets saturated when the pressure head is equal to the air entry value. In the range of pressure head values between the air entry value and zero, the effective saturation (“ χ ”) is always equal to 1 and only the pressure head controls the variability of FS. The effect of the soil suction on FS at shallow depths can be still relevant for pressure heads larger than the air entry value.

C877

Figures 4-6. To my experience, contouring the results of a dynamic process in a space-time plane can introduce spurious results in the maps, such as transferring back in time the effect of a wetting front moving forward.

Figures 7-8. Again, in order to assess the effect on FS, I would look at the pressure heads rather than at soil water content maps, particularly when comparing soils characterised by different soil water retention curves.

P. 1351 L. 28. A comparison of the effect of the soil suction for different soils (characterised by different water retention functions) can be easily assessed by examining “ $\psi \cdot \chi$ ” as function of “ ψ ”.

P. 1352 L. 1-2. This is quite obvious: what could one expect from flat areas?

P. 1348-1355. Please clarify the meaning of expressions such as “fully saturated conditions” or “full saturation”. Do these refer to a complete saturation of the soil column above a given depth?

P. 1354-1355. Please check expressions such as “impermeable soil” and “permeable soil”. I think it is more appropriate to refer to soils with different permeability. Also consider that some results are representative of the examined synthetic case study and reflect the hypotheses behind the model and its implementation (e.g., boundary conditions, parameters, etc.).

P. 1354 L. 23-25. No simulation is required to support this statement. As stated above, this can be obtained by examining “ $\psi \cdot \chi$ ” as function of “ ψ ” for soils characterised by different water retention functions.

P. 1355 L. 10-12. What are the soil parameters? I think that the anisotropy ratio as well as the hydraulic conductivity can be also considered as “soil parameters”.

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