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Earth Surface Dynamics Discussions

Interactive comment on "A hydro-climatological approach to predicting regional landslide probability using Landlab" *by* Ronda Strauch et al.

Anonymous Referee #2

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Dear Editor, dear Authors,

I've read this article for possible publication in Earth Surf. Dynam. The work describes an integrated python-based tool for the evaluation of regional landslide probability initiation throughout a hydro-climatological approach.

The work is of high scientific value and the applied methodologies are scientifically robust. However, to my opinion, the paper ended up being excessively long, sometimes not immediately clear and the overall application not well focused on clear and simple targets. Moreover, given the numerous details of the developed numerical model system, I wonder if a journal which addresses to models development or environmental software would be more appropriate. Anyways, to my opinion, it needs major revision for it to be published in order to make it clearer, more fluent, to better define the aims of the application and to improve the literature review which lacks of some important contributions. Please, read in the following my main concerns.

1. The manuscript is very long and sometimes repetitive, with English style a bit verbose. I have to read it a couple of times to get to the point. Some parts can be synthesized and stated more directly.

2. Literature review is well done and comprehensive of various aspects involved in this work. However, it also lacks of some important contributions in the specific field of physically based modeling for rainfall-triggered landslides, also with regard with the parameters uncertainty.

Please read throughout the following comments some suggestions.

3. My main concern is whether simpler and more computationally efficient statistical approaches for susceptibility evaluation could be more appropriate for such regional and long term analysis. The methodology is based on the use of various simplified models which make it complex. The ultimate model performances are not very satisfactory in terms of ROC and AUC. The approach is classified as dynamic and processed-oriented; however it is not able to reproduce and simulate specific events due to the simplifications and the large temporal scale and can be used only for long term analysis, thus becoming a kind of 'static' approach. Statistical models are very robust and able to guarantee very satisfactory results (e.g. Lepore et al., 2012; Lee and Pradhan, 2007).

Lepore C, SA Kamal, P Shanahan, RL Bras (2012). Rainfall-induced landslide susceptibility zonation of Puerto Rico. Environmental Earth Sciences 66 (6), 1667-1681

Lee, S., Pradhan, B. (2007). Landslide hazard mapping at Selangor, Malaysia using frequency ratio and logistic regression models. Landslides 4, 33-41

4. In the model system there is a mix of temporal and spatial resolutions (soil depth evolution at yearly scale, hydrological model at daily and 6 kmq square, geomorpho-

logical model at 30 m). If I understood well the finest temporal resolution is the daily scale of the annual maximum recharge. However, the daily resolution misses the most intense events and moreover, the daily annual maximum recharge does not guarantee the worst ‘hydrological conditions’ since the antecedent soil moisture conditions are also influent. This why I am skeptical on the advantage of this approach instead of others (comment 3).

5. Soil depth evolution: it is not totally clear to me whether the soil depth evolution model is run in conjunction with the stability module or it is run ‘off line’ and the final map is then fed to the slope stability module. Also, how the soil depth evolution influences the hydrological module? Theoretically, the soil evolution model itself should take into account the change in elevation due to the landslide. Is this done? Please make it clear.

6. Authors do not explicitly discuss the importance of the effect of matric suction and the ‘apparent’ cohesion which arises under unsaturated soil moisture conditions (e.g., Simoni et al., 2008; Baum et al., 2002) and which can be much higher than soil and also root cohesion (e.g., Arnone et al., 2016). They discuss clearly hypothesis of steady state conditions, but this does not justify the neglecting of the matric suction. Moreover, several procedure have been also proposed to predict shear strength under unsaturated soil (based on modified Mohr-Coulomb failure criterion (Vanapalli et al., 1996; Fredlund et al., 1996)), which have been used in various works (Montrasio and Valentino, 2008; Lepore et al., 2013). I suggest referring to Lepore et al., (2013) for a discussion on this point.

Arnone E, Caracciolo D, Noto LV, Preti F, Bras RL (2016) Modeling the hydrological and mechanical effect of roots on shallow landslides. Water Resour Res 52(11):8590–8612

Baum, R. L., Savage, W. Z., and Godt, J. W. (2008) TRIGR-a Fortran program for transient rainfall infiltration and grid-based regional slope-stability analysis, US Geological Survey Open File Report 2008-1159, 75 pp.

Fredlund, D. G., Xing, A., and Barbour, M. D.(1996). The relationship of the unsaturated soil shear strength to the soil water characteristic curve, Can. Geotech. J., 32, 440–448

Lepore C, Arnone E, Noto LV, Sivandran G, Bras RL. (2013). Physically based modeling of rainfall-triggered landslides: a case study in the Luquillo forest, Puerto Rico. Hydrology and Earth System Sciences 17: 3371–3387. DOI: 10.5194/hess-17-3371-2013.

Montrasio, L. and Valentino, R. (2008) A model for triggering mechanisms of shallow landslides, Nat. Hazards Earth Syst. Sci., 8, 1149–1159.

Simoni, S., Zanotti, F., Bertoldi, G., and Rigon, R. (2008) Modelling the probability of occurrence of shallow landslides and channelized debris flows using GEOtop-FS, Hydrol. Process., 22, 532–545

Vanapalli, S. K., Fredlund, D. G., Pufahl, D. E., and Clifton, A. W.(1996) Model for the prediction of shear strength with respect to soil suction, Can. Geotech. J., 33, 379–392

7. Distribution of soil and mechanical parameters are assumed triangular and then distributions of FS are estimated by means Monte Carlo approach. The approach is fine but clearly it increases the computational effort. Other approaches to estimate probability of FS have been proposed in the literature. For example, the First-Order Second Moment (FOSM) (Benjamin and Cornell, 1970) is commonly used to estimate analytical approximations of the spatio-temporal FS statistics (i.e. mean and variance), that can be used to fit a theoretical probability distribution for FS and estimate the spatio-temporal dynamics of probability of failure. Moreover, mechanical parameters are normally assumed to be described by the Normal distribution (Abbaszadeh et al., 2011; you can refer to Arnone et al., 2016 and references therein). Please, briefly discuss.

Abbaszadeh M, Shahriar K, Sharifzadeh M, Heydari M. 2011. Uncertainty and reliability analysis applied to slope stability: a case study from Sungun copper mine.

Geotechnical and Geological Engineering 29: 581–596.

Arnone E, Dialynas YG, Noto LV, Bras RL (2016) Accounting for soils parameter uncertainty in a physically-based and distributed approach for rainfall-triggered landslides. Hydrol Process 30:927–944

8. Model application is a bit confusing. My impression is that it is mainly addressed to demonstrate the model capabilities instead of producing reliable landslide hazard maps for the study areas (AUC are low and FS parameters are not site-dependent). Please, state clearly the main targets of the model application.

Please, read in the following my specific comments.

1. P9L6: I suggest moving this info (soil density) in the model application section. Please, specify what type of soil density this value accounts for (total, dry, wet, bulk density...)

2. P9L20: how do you justify this low resolution of the hydrological model? Clearly, this is not able to simulate the 'local' moisture dynamics at hillslope scale . . .

3. P15L7: working resolution is 30 m. However, if I understood well some components of the system (e.g. VIC) work at coarser resolution . . . Is any interpolation method being used?

4. P16Table16: maximum values of friction angle seem to be very high . . . Do you have references?

5. P16L9: The estimation of root cohesion belong to a further 'branch' of scientific literature of this field which here seems to be significantly simplified (e.g. Pollen and Simon 2005; Preti et al., 2010; Schwarz et al., 2013). . .

Schwarz, M., F. Giadrossich, and D. Cohen (2013), Modeling root reinforcement using a root-failure Weibull survival function, Hydrol. Earth Syst. Sci., 17, 4367–4377.

Pollen, N., and A. Simon (2005), Estimating the mechanical effects of riparian vege-

tation on stream bank stability using a fiber bundle model, Water Resour. Res., 41, W07025

6. P19sec3.2.2: please give some synthesis of the characteristics of the hydro-climatology forcing for the area (e.g. some characteristic time series ...).

7. Figure 4: how about the map of soil evolution model?

8. Figure 5: which soil properties did you use for this figure? I don't see much difference in concavity between zone (2) and zone (3). I suggest adding the degree axis in y, slope is not easily readable. Please, specify what the angle values stand for.

9. P20L7: please, specify the color of the dot lines.

10. P21L15: why is it tan(theta)< $\frac{1}{2}$ tan(phi) .... From eq. (1) it should be simply tan(theta)<tan(phi).

11. P21L18: I don't see where theta=17 degree is in the figure 5

12. P22L5: specify color of the lines?

13. Figure 6a,c: Relative frequency is in time or space?

14. Figure 6b,d: consider to cut the FS values ad significantly 'stable' values, e.g. > 10 (no matter if Fs is 10 or 200)! Otherwise make FS in logarithm scale (interesting values are those close to 1).

15. Figure 6: Interesting questions here could be: which is the soil depth which causes a 'critical change in FS, i.e. that lead the FS going from stability to instability. And in which time window this is reached?

16. Figure 12: make figure 5 and figure 12 consistent to facilitate the comparison.

17. Please, note that the obtained values of AUC are very low ... Are you able to identify which landslides are you missing?

18. Figure 15: I suggest reporting the AUC values of the ROC curves.

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