Review Report

Manuscript ID:

Coupled prediction of flood response and debris flow initiation during warm and cold season events in the Southern Appalachians, USA
J. Tao and A. P. Barros

Dear Editor, dear Authors,

I have reviewed this paper for possible publication in Hydrology and Earth System Sciences.
The manuscript describes the results of the application of a coupled hydrological-slope stability model on three different basins during one warm and two cold season events, which triggered debris flows. Two versions of slope stability model are also compared.

The main objectives of the work are properly and clearly stated and the proposed methodology is of good scientific interest. However, to my opinion, some concerns and some statements regarding the slope stability analysis, the chosen parameters and resolution need to be carefully revised and clarified for the paper to be published. In particular:

1. Derivation of FS equation and representation of forces reported in the "conceptual schema of the geotechnical system" of Fig.3 is confusing and misleading at some points and section 2.2.2 needs careful revision (see specific comments). The used failure criterion to estimate the resisting force is totally ignored. Also, although the use of the Infinite Slope model is quite common within the coupled hydrological-stability models, the authors should at least mention the restrictive hypothesis of the model, especially with regard to the hypothesis of 'infinite slope' which requires a geometry of slope where the slope length *L* is much longer than the soil mantle thickness *H* (which applies for shallow landslides).
2. Hydrological simulation and stability analysis were conducted at 250m x 250m spatial resolution, which is a quite poor resolution for both hydrological models and landside classification at catchment scale. The impact of the DEM raster resolution on model results is mainly caused by its effect on landform parameter derivation, i.e. slope, aspect, curvature, etc…. A few studies have specifically addressed this issue by analyzing the possible impacts either for the only hydrological model response or for landside classification (Kuo et al., 1999; Claessens et al., 2005; Tarolli and Tarboton, 2006, among the others). Authors should discuss and justify the choice of such a resolution, even in relation with the observed landslide events.
3. The authors recognize the importance of soil properties parameters (and in particular geotechnical parameters, cohesion and friction angle), providing a sensitivity analysis section but only after they have shown the model results. They should emphasize the importance of the parameter values even in the description of the study cases, by discussing and justifying from the beginning the chosen values for the final model setup.

I will lay out my specific concerns below, referring to section, page and line numbers.

Abstract:

1. L8 P8366:I wouldn't describe as an 'hypothesis' the fact that "soil moisture redistribution" plays an important role in the initiation of failure mechanism; it's the physic of the failure mechanism, which depends on the pore pressure conditions (and thus the soil moisture

dynamics), as widely proved and confirmed. I would say “to investigate the relationship” or similarly.

Section 1

2. L14 P8368: please, specify better what you mean with “take most of the static factors into consideration”.
3. L22 P8370: the author provide a detailed and clear review of exiting works, but they conclude with the statement “*One common trait of these studies is the separation between the simulation of hydrologic response to rainfall forcing (typically neglected) and debris flow initiation indices or prognostics*”. What do you mean with ‘separation’? Hydrological response to rainfall forcing (in term for example, of groundwater dynamics, or soil moisture) is the most important dynamic component used for the evaluation of the instability initiation. Please clarify or revise the sentence.
4. L4 P8371: see comment 1.

Section 2

Section 2.2 needs to be deeply revised, with particular regard to the derivation of the FS equation. Also, note that both the methods presented in section 2.2.1 and 2.2.2 are based on the same stability model that is the Infinite Slope model. The one presented in section 2.2.1 is just a simplified version:

4. Please provide the reference when mention the Infinite Slope model (commonly referred to Taylor, 1948);
5. L1-14 P8374: please note that even Eq. 1, from Dietrich et al., 1993 is derived from the equilibrium of forces (not specified) under the hypothesis of cohensionless terrain and subsurface flow parallel to the slope (correctly specified by the authors). Simply, Eq. 1 refers to the incipient failure, that is at FS=1 (or, similarly, resistance forces equal to destabilizing forces), so that the 4 stability classes can be derived. Then, similarly to the procedure described in section 2.2.2, even this approach is based on the Limit Equilibrium Method.
6. Authors modified Eq. 1 by substituting the soil wetness term h/z with the saturation degree (L7 P8374), defined as the ratio between the simulated volumetric soil moisture and soil porosity (eq. 2, L9 P8374). However, if soil moisture never reaches values lower that the residual value, the above mentioned saturation degree cannot assume values equal or close to zero and the dried conditions are thus neglected. The use of the effective degree of saturation (i.e. the normalized saturation degree) would be more correct, to my opinion.
7. L17 P8374: what exactly do the authors mean with “the SSI method can not provide quantitative information”?
8. L22 P8374: technically, the spatio-temporal FS distribution can be easily derived even from Eq. 2; the main lack of this approach is that it neglects the cohesion and the effect of the suction in unsaturated soils. Please, discuss and clarify this.
9. Titles of subsections 2.2.1 and 2.2.2 (Stability index mapping and Dynamic Factor of Safety) could mislead the reader thinking that the SSI is not dynamic (the soil moisture changes dynamically).
10. L1 P8375 on and Fig.3: please define the axis normal to the Z direction (there is no label). Then, the equilibrium should be made considering a generic slice of the infinite slope, to take advantages of the hypothesis of infinite slope (e.g. the interslices forces are equal and opposite, due to symmetry). Based on Fig3. Fp and FN are the parallel and normal component of the gravity force (they act at the barycenter of the slice). Instead, the resisting forces act at the potential failure surface that, in the sketch of Fig.3, I guess is the second line parallel to the slope. Moreover, according to the used Mohr-Coulomb failure criterion

(not mentioned in the text) the shear force (reported as a sum of Ft, Fs and Fc) depends linearly on the normal force (usually named N, not reported in the Figure) which acts in the Z direction and negative versus (opposite to the gravity component). In fact, the forces are unbalanced in the diagram. Then, the soil friction component (Ft) is a function of the normal effective force N' which in turn is equal but opposite to the normal component of the gravity. I warmly suggest the authors to revise the derivation and definition of forces (see, for example, the cited works Rossi et al., 2013; Arnone et al., 2011; or Montraisio and Valentino, 2008, among others).

11. L9 P8375: A is then the area of the slice.
12. L11 P8375: Pressure head is commonly defined as positive pressure; here is meant to be negative (suction) so then I would not say pressure head, but matric suction or potential.
13. L1 P8376: authors first need to define the FS (ratio between resisting forces and driving forces) in order to obtain the 'final form of FS' (eq.6), by substituting eq. 4 into the definition.
14. In eqs. 6 and 8, tanphi should be out of the parenthesis, according to the Mohr-Coulomb failure criterion.
15. L19 P8376: author should discuss and justified the values of geotechnical parameters (c, phi). Note that you recall here Table 3 that instead should be Table 1.
16. Author should discuss in this section how the soil depth (z) is treated in the model (constant or layered), and, if layered, which depth is considered for the final FS value at each computational cell.

Section 3

17. L21-25 P8377: do you have information about the thickness of the colluvium deposits and the depth of bedrock? These information are fundamental for the correct hydrological and stability modeling, (e.g. it provides the location of the potential failure surface).
18. L20-25 P8378: do you have information about the landslide total area? Resolution used in the model should be comparable to the landslide area value.
19. L14-18 P8379: quality of spatio-temporal rainfall distribution is certainly an important factor in such modeling approach. However, I believe that author should emphasize here also the importance of hydrological and geotechnical parameters which can have even a more important role, as also discussed by the authors in section 4.3 and in the conclusions.
20. L13-on P8383: Thicknesses of second and third layers are not clear, as well as the base layer. Please specify even with an example at a selected pixel. Such information is crucial for correctly interpreting the model results.
21. L3-17 P8383: are thus hydrological properties constant along depth?

Section 4.2

22. L12 P8386: Fig.9 - specifying the corresponding soil type would help the interpretation of results, even indicating the soil moisture limits (or by plotting the effective degree of saturation).
23. L21-24 P8386: please, describe and discuss Fig.11a. Define the vertical red line even in the text.
24. L24-26 P8386: authors have to support this sentence by discussing the results.
25. Fig15: note that one of the reasons why SSI approach significantly overestimates the number of unstable pixels is because it neglects the cohesion and the suction effect, which have an important weight in FS computation.
26. L10-23 P8388: it is hard to follow the matching between soil moisture and interflow without reporting the values at saturation (which are distributed and varying with depth). Interflow at first layer is always positive, meaning that the layer has not reached the saturation yet. How do you justify that? Is the 3rd layer at saturation? Why it does not produce negative

interflow? The different behavior among the layers significantly depends on the hydraulic conductivity values (I believe that the second layer has a really high value of hydraulic conductivity).

27. Fig9: initiation of debris flow seems to be mostly related to the saturation of the second layer (which then determine the interflow).
28. L27-30 P8389: again, authors should discuss at the beginning the uncertainty of soil properties.
29. L22-24 P8390: It is hard to justify this without looking at the soil moisture and interflow patterns (not shown). Fig 9c shows an increase of soil moisture at the 3rd layer after rainfall stops. If FS is computed at this depth, this justifies the increasing number of unstable which cells.

Section 5

1. The study analyzed debris flow events at warm and cold season: did a general behavior come out from the different applications? Please discuss.
2. L19 P8392: as said, the stability model is the same. SSI is a simplified version and implicitly defines failure at FS=1.
3. L20-22 P8392: I do not agree that is qualitative method. It's still dynamic and physically based, even if extremely simplified.
4. L3-5 P8393: see comment 25. The different higher sensitivity to the soil moisture at local scale is due to the simplified used equation, which emphasize the role of soil moisture.

Minor issues:

L24 P8370: delete "of"
L25 P8373: delete and in Dietrich and et al. (1993).
L12 P8374: Fig.1 is not previously mentioned in the text. So, Fig.2 should be Fig.1. Then change figures numbering accordingly.
L4 P8375: define A, z and theta.
L4 P8375: define tanphi.
L17 P8375: What is (L) ?
L15 P8378: check consistency of table numbering.
L21 P8379: *have been* or *are used*?
Fig.9: change colors accordingly for layers, between top and bottom plots. Also, specify a, b and c in the caption.
Fig.10: specify a, b ad c in the caption.
Fig.14: specify 'FS' in the legend.

Referenes

Claessens L., Heuvelink G.B.M., Schoorl J.M., Veldkamp A. (2005) DEM resolution effects on shallow landslide hazard and soil redistribution modelling. Earth Surface Processes and Landforms 30:461-477. DOI: 10.1002/esp.1155.

Kuo W.-L., Steenhuis T.S., McCulloch C.E., Mohler C.L., Weinstein D.A., DeGloria S.D., Swaney D.P. (1999) Effect of grid size on runoff and soil moisture for a variable-source-area hydrology model. Water Resources Research 35:3419-3428. DOI: 10.1029/1999wr900183.

Montrasio, L. and Valentino, R.: A model for triggering mechanisms of shallow landslides, Nat. Hazards Earth Syst. Sci., 8, 1149–1159, doi:10.5194/nhess-8-1149-2008, 2008.

Tarolli, P., Tarboton, D.G., 2006. A new method for determination of most likely landslide initiation points and the evaluation of digital terrain model scale in terrain stability mapping. Hydrology and Earth System Sciences 10, 663–677.

Taylor, D.W., 1948. Fundamentals of Soil Mechanics. J. Wiley, New York