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

Anonymous Referee #2

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Evaluation

A coupled hydrology-slope stability model is described in this work. The novelty of the work is represented by the application of the model to simulate both the flood response at the catchment scale and the hillslope stability processes, thus enabling a multi-response validation. The work is interesting and well suited to the readereship of HESS. Moreover, it is based on a good data set. However it needs a careful re-organisation and attention to a number of issues to be acceptable for a major scientific journal.

General comments

1. I found the title misleading: it deals with "debris flow initiation" and it turns in the paper that the only physical process considered is shallow landsliding. The authors should made clear that initiation mechanisms can be broadly grouped into flows originating from landslide initiation, or from the entrainment of sediment by flowing water in a channel or in coalescing rills and gullies (e.g., Iverson et al. 1997). It may be the case that all the debris flows in the study region are originated as landslides; however, it is arguable that not all failing hillslopes will mobilize to form debris flows. I think the title should reflect more accurately the content of the paper, by focusing on 'shallow landsliding'. The model doesn't include any debris flows propagation module. Note also that the confounding overlapping between shallow landsliding and debris flows is not limited to the title and is widespread in the work.

2. The hydrological model is very poorly presented, as well as its application. One aspect that requires specific attention from the co-authors is the description of the specification of the initial conditions. As it is described here, the model is not suited for continuous simulation of the hydrological cycle, and requires soil water content to be specified at various level in the soil profile and at multiple locations. On the other hand, initial soil moisture conditions play a critical role for flash flood modelling (Marchi et al., 2010) with model results that can range from useless to almost perfect by simply playing with the initial wetness parameters. Arguably, a similar sensitivity is affecting the simulation of the hillslope instability. All this points to the need for a good section on the initialization of the coupled model.

3. In a similar vein, the hillslope stability model requires a much more careful description. Please take into account the comments by Reviewer 1.

4. Accurate topographic representation is of key importance in shallow landsliding prediction. Nevertheless, a 250m grid size is used in the model exercise described here. Even more surprising, this choice in neither discussed or commented. Instead,

the choice of using a rough DEM resolution and its implications requires careful discussion, with reference to the relevant literature. The comment reported in the conclusions “In addition, we hypothesize that there should be a scaling effect associated with the spatial resolution of the model itself, that in turn suggests that there should be utility in investigating the scaling behavior of slope instability criteria in the future. Specifically, the ability to represent heterogeneity and subgrid scale variability in subsurface flow dynamics should have a strong impact on the magnitude of interflow at small scales” is surprising, since the scaling effect is neither identified or commented before in the paper.

Details

P8366, L7-9: “This suggests that the dynamics of subsurface hydrologic processes play an important role as a trigger mechanism, specifically through soil moisture redistribution by interflow. The first objective of this study is to investigate this hypothesis.” Tons of papers have already explored this hypothesis. This shouldn’t be an objective for this work.

P8369, L11-13. “Safaei et al. (2011) argued that coupling dynamically distributed hydrologic models with slope stability models is necessary to quantitatively model or predict the occurrence of debris flow both in space and time.” The reference cited here: Safei et al. (2011), is not listed in the References. The co-authors should note anyway that the need for the coupling was stressed much earlier (Montgomery and Dietrich, 1994 and references therein).

P8370, L22-25. “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. Mirus et al. (2007) investigated the role of subsurface flow based on a three dimensional numerical solution of Richards’ equation using the control volume finite-element method combined with an infinite-slope equation (Dutton et al., 2005). They demonstrated that pore-water pressures, and thus slope stability

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are underestimated without taking into account convergent subsurface flow." This is the place where the co-authors could establish what is new with this work: the validation of the coupled response is carried out both for the flood response and for the hillslope instability. However, this is written here in a way which is barely understandable. Moreover, the sentence starting with 'Mirus et al.. . .' should be anticipated to the sentence starting with 'One common. . .', to make sense.

P8371, L19-23. "physical hydrology". Drop 'physical'. "Nowcasting": the model is not used here for any nowcasting purpose: this should be substituted with 'prediction'.

P8382, L23-25: "However, the Z-method tends to underestimate soil depth at very high elevations, while the S-method overestimates soil depth in the valleys (Fig. 6)." The terms "overestimation" and "underestimation" are commonly used to compare and evaluate observations versus model results. Do you have observations of soil depth to evaluate how the model behaves with respect to reality?

P8384, L1-4: "..air temperature,air pressure, wind velocity, downward shortwave and longwave radiation and specific humidity". This data are not required in the hydrological model description described in Section 2.1. Please specify.

References

Iverson, R.M., Reid, M.E., and LaHusen, R.G., 1997: Debris-flow mobilization from landslides. Annual Reviews of Earth and Planetary Sciences, 25, 85–138.

Marchi, L., Borga, M., Preciso, E., Gaume, E., 2010: Characterisation of selected extreme flash floods in Europe and implications for flood risk management. J. Hydrol., 394(1-2), 118-133, doi: 10.1016/j.jhydrol.2010.07.017.

Montgomery DR, Dietrich WE. 1994. A physically based model for the topographic control on shallow landsliding. Water Resources Research 30: 1153–1171.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8365, 2013.