Reply to Reviewer #2

We thank the reviewer for the thoughtful comments and suggestions. We made a conscientious effort to address all issues raised. The item-by-item Reply follows in blue.

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 readership of HESS. Moreover, it is based on a good data set. However it needs a careful reorganization 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.

The Reviewer's point is well taken. The specific events studied in the manuscript referred to as "debris flow" in the NCGS survey report of the events are initiated by shallow landslides. Because most of the debris flows in the region of study originate from slope failure (e.g. Wieczorek et al., 2009; Wooten et al., 2008), the debris flow events triggered by shallow landslides are indeed the most common. For instance, about two-thirds of the debris flow initiation sites were in concave slopes in the region, evidenced by scouring of the detachment surface resulted from shallow landslides (Wieczorek et al., 2009). The debris flow developed from the entrainment of sediment by flowing water in a channel or in coalescing rills and gullies in itself is not sufficient to define an 'initiation' location.

The title of the manuscript is clear that our focus is on the "debris flow initiation", which should take place through a shallow landslide at the initiation point followed by rapid mobilization within 10's m to form a debris flow proper. See for example: http://www.geology.enr.state.nc.us/Landslide_Info/Landslides_main.htm. Instead of changing the title, and for consistency with the relevant literature for the region, we introduced a new paragraph in the Introduction that clearly establishes the context for the simulations and includes the reference to Iverson et al. (1997) (L26 Pg.8367).

"Note there are three modes contributing to debris flow mobilization, namely Coulomb failure, liquefaction, and transient/mixed modes of the two (Iverson et al., 1997). The Coulomb failure mode initiates shallow landslide activity which can develop into debris flows. This is the key initiation mechanism in the region of study. Debris flow propagation (post-failure) is not addressed in this study. "

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.

The model simulates the full hydrologic cycle and has been used in various implementations for continuous simulations over a wide range of time-scales from flash-floods to multi-year simulations. Relevant references are included in the manuscript and there is wide documentation of the model and its applications in the peer-reviewed literature including: Devonec and Barros (2002), Yildiz and Barros (2005; 2007; 2009), Garcia-Quijano and Barros(2005), Gebremichael and Barros (2006), Bhushan and Barros (2007), Kang and Barros (2012a and 2012b), Kang et al. (2013) and Tao and Barros (2013).

Initial soil moisture conditions play a critical role indeed. The reason why there is no section on initialization alone in this manuscript is because there is a prior paper that describes in detail flashflood simulations similar to those presented here in the same region, using the same model and ancillary data sources (Tao and Barros, 2013). In addition, Yildiz and Barros (2007) also describe the initialization procedure. They used a spin-up period of two-weeks for a 5-month simulation, whereas Devonec and Barros (2002) and Kang and Barros (2012a; 2012b) used a spin-up period of one year for their multi-year simulations. The spin-up simulations themselves are initialized at various levels in the soil profile and at multiple locations by specifying soil wetness. In the present paper, model initial conditions for the spin-up simulations are summarized in Table 1. Model spin-up allows the model physics to do their work toward consistent initial conditions and is a long established practice (e.g. Cosgrove et al., 2003), especially for long-term simulations in the context of which model complexity and nonlinearities would make either the formal calibration or just trial-and-error of initial conditions virtually impossible. Typically, after the model is initialized with average seasonal soil moisture conditions, the model is spun-up by running it for at least the same duration of the simulation using atmospheric forcing before the forecast proper to allow the model state variables to reach internal consistency as stated in the manuscript. The good agreement between the estimated and observed streamflow at the beginning of the simulation (shown in Fig. 8) provides justification to the methodology. The following paragraph was added to section 4.1 (L22, Pg.8384):

"In order to allow the model state variables to reach internal consistency, model spin-up simulations for the same duration of the simulation were conducted before the event simulation proper. The end of the spin-up period is the beginning of the event simulation. The basin soil moisture conditions for the spin-up simulations were initialized by specifying soil wetness based on seasonal climatology modeled to be consistent with the streamflow at the beginning of the simulation period (shown as in Table 1)."

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

Please see our detailed Reply to major Comment 1 and specific Comment 13 from 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.

The Reviewer's comment is well taken. In the sentence above, the intention was to refer to the scale effects of model resolution.

As stated in our Reply to Reviewer #1, the model spatial resolution is a compromise between the coarse resolution at which atmospheric forcing data and ancillary data (e.g. soil properties) are available and the spatial resolution required to capture the physics. Although the spatial scale over which the initiation takes place ranges between 30 -150 m, in the region of study, slope failure quickly evolves spatially to debris flow. At the model resolution, in the region of study, the events are debris flows. We have added discussion regarding the model resolution (L25 P8372): "The temporal and spatial resolution of model simulations is 5 minutes and 250 meters respectively, which meets numerical stability requirements, and reflects a compromise among the coarse spatial resolution of the atmospheric forcing datasets (1-32 km), the spatial scale of terrestrial ancillary data such as soils properties and vegetation cover (~1 km), and the spatial resolution adequate to capture the governing hydrologic processes(e.g. Tao and Barros, 2013)."

We also added the short discussion below to the Section 2.1(L18, Pg.8373):

"Note that, in principle, the higher the spatial resolution the more rigorous the coupling between the hydrological and slope stability models, and the more accurate representation of governing processes and spatial gradients. Therefore, a scale effect is expected with simulated hydrologic variables displaying smoother spatial distributions at coarser model resolutions (see for example, Yildiz and Barros, 2009)."

In addition, the sentence above cited by the Reviewer was revised for clarity as follows:

"In addition, we hypothesize that there should be a scale effect associated with the spatial resolution of the model itself, and thus there should be practical utility in investigating the dependence of simulated soil moisture and interflow conditions at the time of landslide initiation on model resolution. Furthermore, 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"

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.

R: This point was addressed in our Reply to Reviewer #1. We believe the confusion with this statement comes from lack of clarity in our writing. The role of soil moisture dynamics in slope failure is well established. Geomechanical models typically account for suction forces in slope stability analysis (Lu and Godt, 2008; Lu et al., 2010). Our explicit focus is on interflow, and in particular the transient mass fluxes across the basin, which allows us to take a watershed view or hillslope scale view, that is a "regional" approach rather than a "local" approach to slope stability analysis. Interestingly, the simulations show that independently of the watershed or storm type, the initiation takes place not after the stability criterion defined on the local equilibrium of forces is exceeded, but when interflow reaches its peak for the event. The writing was revised to clarify this matter.

"This suggests that the dynamics of subsurface hydrologic processes play an important role as a trigger mechanism, specifically through soil moisture redistribution by interflow. We further hypothesize that the transient mass fluxes associated with the temporal-spatial dynamics of interflow govern the timing of shallow landslide initiation, and subsequent debris flow mobilization. The first objective of this study is to investigate this relationship."

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).

R: We have added the reference suggested above. We also revised the related sentence to include additional references to previous work. Thank you for pointing this out.

"The need for coupling dynamically distributed hydrologic models with slope stability models required to quantitatively model or predict the debris flow occurrence both in space and time has been articulated earlier (Baum et al., 2010; Iverson, 2000; Montgomery and Dietrich, 1994; Safaei et al., 2011; Simoni et al., 2008)."

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 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.

R: The Reviewer's comment is well taken. We have updated the related sentences as follows:

"One common trait of these studies is that the simulated hydrologic response to rainfall forcing (e.g. the flood hydrograph) is not evaluated, and the focus is on the landslide initiation indices or prognostics independently of the underlying hydrologic states. However, Mirus et al. (2007) investigated the role of subsurface flow using a three dimensional numerical solution of Richards' equation based on the control volume finite-element method combined with an infinite-slope equation (Dutton et al., 2005), and demonstrated that pore-water pressures, and thus slope stability, are underestimated without taking into account convergent subsurface flow. In this study, we will further investigate the critical role of subsurface flow (especially interflow) in triggering the debris flow occurrence. Both the flood response and the debris flow initiation produced by a coupled hydrological-stability model are validated against streamgauge observations and the survey report on the debris flow events provided by NCGS geologists (Dr. Richard Wooten, personal communication), respectively."

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

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?

R: We do not have systematic observations of soil depth except from our own field work maintaining a hydrometeorological network in the region. The ranges of soil depth are based on personal inspection, and a survey of the literature and previous detailed field studies in the Appalachians (Price et al., 2010; Price et al., 2011). But the Reviewer raised a good point. To avoid confusion, we have revised the sentence as follows:

"However, the Z-method tends to result in too thin soil depth at very high elevations, while the soil depth in the valleys calculated by the S-method tends to be too thick (Figure 6), based on the authors' observations in the field."

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.

R: The hydrological model description in Section 2.1 has a focus on rainfall-runoff and subsurface processes. However, the hydrologic model solves the energy balance equations and also predicts soil temperature profiles as well as sensible latent and ground heat fluxes, and outgoing longwave radiation. These elements of the model are described in Devonec and Barros (2002) for example. For completeness, the following sentences were added to Section 2.1 (L16 P8373):

"Sensible and latent heat fluxes are estimated based on the Monin-Obukhov similarity theory which provides dimensionless variables expressing the buoyancy effects resulting from the vertical density gradients in the stable atmosphere with modifications for unstable boundary layer conditions, and are calculated using the input air temperature, air pressure, wind velocity and specific humidity. Radiative forcing is calculated based on the input downward shortwave/longwave radiation from the atmospheric forcing data set, and landscape attributes such as albedo and emissivity. Further details on the representation of land-atmosphere interactions in the model are described in Devonec and Barros (2002)."

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Thank you.