Reply to Reviewer #1

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.

We thank the reviewer for the very thorough review, constructive comments, and thoughtful suggestions. We made a conscientious effort to address all issues raised and to improve clarity in the revised manuscript. Below are detailed item-by-item Replies in blue.

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).

The schematic was updated as shown on the right below:

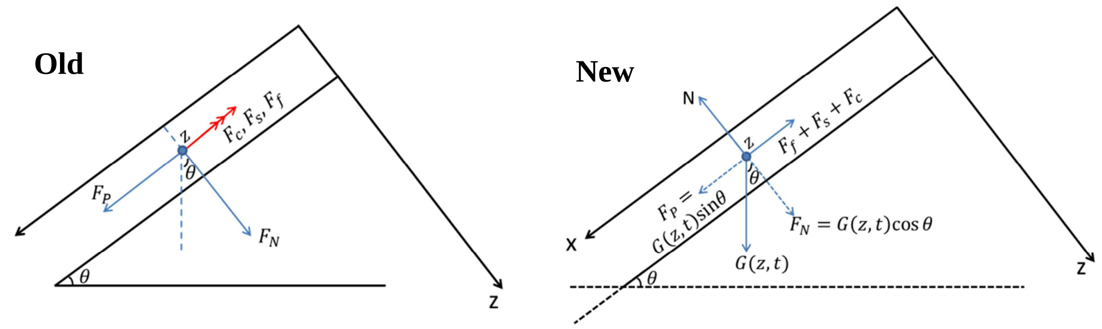

Figure 1R - Conceptual schema of the geotechnical system, explictely showing the essential forces acting on a slope (the original Fig.3 is on the left, and the revised Fig.3 is on the right).

A short description about this schema was added (L1, P8375) as follows:

"Figure 3 shows the diagram of forces acting at a generic location x and depth z on a cross-section of the conceptual infinite slope model. The Z-axis is normal to the surface and positive in the downward direction. The X-axis is parallel to the slope surface, and thus normal to Z. F_N

and F_P are the normal component and along slope components of the weight G, respectively. The normal component of the weight is counteracted by the normal resisting force N. In the along slope direction, F_P is counteracted by the friction F_f, suction F_s and cohesion F_c forces. The Coulomb failure mode occurs when the shear stress at failure on the failure plane equals or exceeds the resultant of friction, suction and cohesion stresses, that is $FS \leq 1$."

Section 2.2.2 was revised throughout according to the Reviewer's suggestions. Specifically, the hypothesis underlying coupled hydrological-stability models, especially for the Infinite Slope model, are explicitly stated and discussed in section 2.2.2 (L1, P8375) as follows:

"The infinite slope model has two critical assumptions. First, it assumes that the slope failure occurs within a thin soil layer of depth H, and second it assumes that the failure plane is of infinite length, i.e. H<<L in Fig. 3. In this study, the "effective" L is the spatial resolution of the model (250m), whereas H is the soil mantle thickness, which is spatially variable ranging 10's cms at higher elevations to 100's cms at lower elevations and in the valleys, thus L/H is always larger than the critical ratio of 25 above which the infinite slope assumption is valid (Milledge et al., 2012)."

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.

The spatial resolution is a compromise between the coarse spatial resolution of available 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 required by the governing hydrologic processes. Although there are hydrological models operating at higher resolution, especially at the hillslope or micro-catchment scale, there are not many models that solve the water and energy balance equations including vegetation and coupled surface-subsurface processes, overland flow and streamflow routing at high temporal resolution (~ 5 min) for long periods of time (months to years) and over relatively large catchments (> 100 km^2) without calibration.

Nevertheless, the Reviewer's point is well taken, especially with regard to the derivation of landform metrics at different scales, and we have added a short discussion (L25 P8372) as follows:

"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)."

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.

The Reviewer's point is well taken. A short paragraph was added to the beginning of Section 3.3.2 (L9 P8382) as follows:

"The soil internal friction angle and cohesion are two important parameters required by the slope stability models. Uncertainty in these parameters can induce very large uncertainty in the resultant FS values. The present study benefited from previous research conducted by Witt (2005) in the same area. Witt examined the same two parameters using SINMAP and SHALSTAB, and reported representative values. We adopted those representative values as well as the ranges reported by this study for slope stability analysis and sensitivity analysis (shown in Table 1)."

Note that Table 1 here is the original Table 3. We have corrected the table numbering as noted by the Reviewer.

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.

R: We agree with the Reviewer about the statement on the failure mechanism. We believe the confusion resulted from lack of clarity in our writing. Our intent was to emphasize the role of transient lateral subsurface flow processes across the basin, specifically interflow, that play a crucial role in destabilizing the slope, which are not typically addressed explicitly in the literature. Areas of large interflow are characterized by lower pore pressure conditions. As opposed to considering soil moisture dynamics at point (in the local soil column), by focusing on the mass flux fields associated with interflow, we can isolate neighborhoods of grid elements where the likelihood of failure is higher, and thus reduce ambiguity. The statement was revised to improve clarity.

"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."

Section 1

2. L14 P8368: please, specify better what you mean with "take most of the static factors into consideration".

R: Here we were talking about the static factors considered in the steady-state models such as SHALSTAB and SINMAP, including soil properties, slope, vegetation characteristics, and generic soil moisture content derived from terrain topography and surface geomorphology. These models simplify the dynamic hydrological processes and thus cannot predict the timing of slope failure. We have specified these specific factors in this sentence. Thank you.

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.

R: Clearly these models represent soil moisture dynamics to some degree. We meant to say that such models do not address the concurrent flood response (i.e. streamflow prediction), and model evaluation is often limited to the location of landslide activity. The statement was revised to improve clarity 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 evaluating the landslide initiation indices or prognostics independently of the underlying hydrologic states."

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);

All done as suggested.

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 cohesionless 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.

R: The Reviewer's point is well taken, and this is stated in the revised manuscript (P8733 L26) as follows:

"Based on the assumption that the water table follows topography at small scales, and thus is parallel to the slope, and that the soil material is cohesionless, Dietrich et al. (1993) proposed a simplified infinite slope stability model using the conventional limit equilibrium method (i.e. in equilibrium, driving forces are equal to resisting forces)"

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.

R: Although the effective degree of saturation has a wider range of variability indicating the relative level of soil wetness overall, for saturated or semi-saturated conditions, the favorable scenario for the triggering of debris flows, the differences are not significant.

7. L17 P8374: what exactly do the authors mean with "the SSI method cannot provide quantitative information"?

R: The SSI method is based on quantitative information about instability conditions in the soil column. But this information is then processed through a threshold-based classification algorithm to establish broad, and thus potentially ambiguous, classes of potential of slope failure. We understand the Reviewer's concern, and we have rephrased this sentence as follows (L17 P8374):

"Even though the SSI method is based on quantitative information relating soil moisture and slope static properties, this information is aggregated into broad qualitative categories using a threshold-based classification, which creates ambiguity as many different slope states belong to the same category."

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.

R: The beginning of section 2.2.2 has been updated as follows (L22 P8374):

"Although the SSI can provide spatio-temporal instability information, it neglects the soil cohesion and suction effects, as well as the relational position of a grid element with respect to its neighbors. In order to quantitatively analyze the debris flow triggering mechanisms accounting for all the dominant factors, the spatio-temporal distribution of a factor that can represent the dynamic net forces acted on the slope should be determined explicitly."

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).

R: We eliminated the ‘Dynamic’ and changed the tittle of section 2.2.2 to “Factor of Safety”. Thank you.

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).

R: The Reviewer’s comments are well taken. Figure 3 was revised. Please see the reply to the major comment 1.

11. L9 P8375: A is then the area of the slice.

R: We updated L9 as follows:
“A is the nominal area where the force is applied (i.e. the area of the slice shown in Fig.3), that is the spatial resolution in our model.”

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. Changed as suggested.

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.

R: We have defined the FS at the beginning of section 2.2.2:

“The Factor of Safety (FS), defined as the ratio between resisting forces and the driving forces, is a widely used factor for slope instability analysis.”

Also, the following sentenced was added (L19, P8375):

“At the parallel direction to the surface, the resisting forces include the friction force, suction force and cohesion force, where the driving force is the gravitational force. The FS is equal to one when the slope is in equilibrium. Rearranging Eq. (4) to separate the resisting forces from the driving forces in the direction parallel to the slope, and then dividing the two sides of the equation by the driving forces, we can obtain the final form of the FS equation for unsaturated conditions."

14. In eqs. 6 and 8, tanphi should be out of the parenthesis, according to the Mohr-Coulomb failure criterion.

R: Corrected. Thank you.

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.

R: Table numbers are correct now. Thank you. Please see our reply earlier referring to Witt (2005) work as the source for the parameters.

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.

R: The number of soil layers in the 3D-LSHM is left to the modeler to specify for any given application based on local conditions and available data. In the implementation presented in this manuscript, there are four soil layers, including three unsaturated soil layers and one saturated soil layer. The soil depth of the top layer is uniform (10cm) all over the basin. The depths of the second and third layers vary spatially across the basin as described in Section 3.3.2. The ratio of the thickness of the second to the third layer is 2:3, to roughly account for root density distribution. The base layer (saturated layer) is 1m deep at elevations above 1300 m, and 4m deep below 1300m to represent thicker alluvial deposits in the valleys. For this we rely on reports from USGS an NCGS, point observations, and previous work in the Appalachians (e.g. Yildiz and Barros, 2007). Please see Section 2.1 for the description of soil geometry set up in the 3D-LSHM, and see Section 3.3.2 for the soil depth calculation.

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).

R: We don't have systematic measurements of the thickness of the colluvium deposits and the depth of bedrock, but we agree with the Reviewer about the importance of this information. The only information available is from the literature, USGS and NCGS reports, photographs of cross-sections of alluvial fans, and technical notes over decades, such as for example (Miller, 1999). These are cited in the manuscript. Please also see reply to previous comment.

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.

R: We do not have the landslide total area. However, we do have the track distance estimates for the event in 2011 (shown in Table 2) from field surveys, which range from 350ft (~ 100 m) to 500ft (150 m) for the three locations (Rick Wootten, NCGS, pers. communication). These distances are smaller than the model resolution (250m), but of the same order of magnitude. However, the focus of this work is on the initiation location of the debris flows, and specifically the conditions triggering the failure. The model does not simulate landslide propagation.

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.

R: The Reviewer's point is well taken. We agree with the reviewer about the importance of hydrological and geotechnical parameters. However, because these are headwater catchments with very fast rainfall-runoff response times (i.e. times of concentration are very short), the uncertainty induced by spatio-temporal varying rainfall largely exceeds the uncertainty associated by ancillary parameters as long as they have a physical basis (STATSGO). See uncertainty analysis in Tao and Barros (2013) for rainfall. If the rain does not fall in the right place, at the right time, and with the right intensity, models cannot produce reliable simulations. This is the point emphasized here.

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.

R: The ratio of the thickness of the second to the third layer is 2:3, to roughly represent root density distribution, which we have added in the manuscript. The base layer is 1m deep at elevations above 1300 m, and 4m deep below 1300m to represent thicker alluvial deposits in the valleys. Also see the response to comment 16.

21. L3-17 P8383: are thus hydrological properties constant along depth?

R: No, the hydrological properties are varying in 3D (x,y,z) space. Please see Fig.7 which shows the saturated hydraulic conductivity, soil porosity, field capacity, and wilting point for the four soil layers from the top to the base layer.

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).

R: The dominant soil types over the catchments of interest are gravelly loam, sandy loam, fine sandy loam and moderately permeable loam as shown in Fig. 1. Detailed discussion of soils can be found also in Tao and Barros (2013). The soil porosity and hydraulic properties maps are shown in Figure 7, which can help to illustrate the soil moisture limits. However, the positive or negative interflow is not directly related with soil moisture per se but with the gradients of the potential flow surfaces, please see the response to comment 26 below.

23. L21-24 P8386: please, describe and discuss Fig.11a. Define the vertical red line even in the text.

R: We updated the related sentence as follows (L21, Pg8386):

"The histograms of soil moisture, interflow, slope and rainfall rate for the three events are shown in Fig. 11. The solid red lines indicate the local conditions in the unstable grid element selected for analysis (corresponding to the gray solid line in the upper interflow time series). The histograms of these variables provide an alternative view of the same data that illustrates the concurrency of slope steepness, high rainfall intensity, and large and fast interflow response especially from the top two layers at the unstable locations. As it can be seen from Fig. 11a, the histograms of rainfall and total interflow show uneven distributions, skewed to the left and with very long tails on the right. For the conditions when and where the debris flow initiated (marked by the vertical solid lines), rainfall, total interflow and the slope as well, show intermediate high values on the right of the distribution."

24. L24-26 P8386: authors have to support this sentence by discussing the results.

R: The related sentence is "Indeed, the simulations are clear in demonstrating that rainfall thresholds are not sufficient to detect slope instability." We thank the reviewer for pointing out this. Actually this sentence applies to Fig.11b and 11c, not Fig. 11a. The sentence was deleted.

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.

R: We agree with the reviewer. We added this point into L21 on Pg.8387.

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).

R: As shown as the figure below, the total flow at pixel (i,j) is the sum of flows from eight boundary cells. The sign convention is as follows: positive values indicate incoming flow; negative values represent outgoing flow. Thus, the positive or negative values are not directly relevant to soil saturation locally, but rather to potential flow lines, that is gradients.

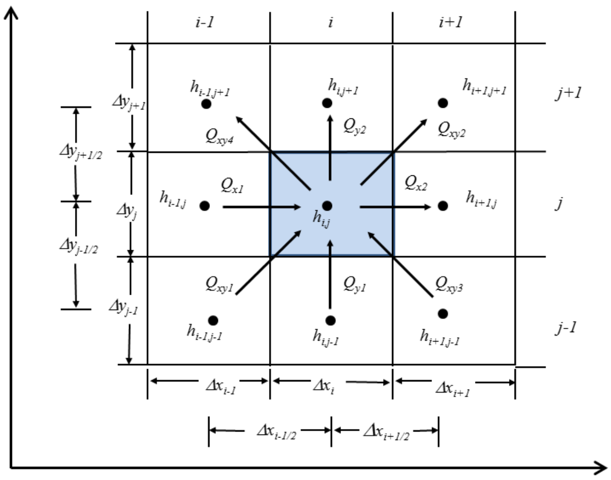

Figure 2R - Discretization of the multiple cell system and the water balance components (revised from Yildiz, 2001)

As the Reviewer pointed out, the different response among the soil layers (as shown in Fig. 9b and 9c) highly depends on the hydraulic conductivity. The lateral hydraulic conductivity Kh depends on soil moisture and saturated hydraulic conductivity Ksat (shown in Fig. 7) and the anisotropic scaling factor for the hydraulic conductivity (Tao and Barros, 2013). Besides, the subsurface flow also depends on soil depth (i.e. flow cross sectional area) in each layer. Actually, the scaling factor for Kh in the first layer is always larger than in the second layer due to compaction. It is the combination of larger soil moisture (shown in Fig.9b and 9c) and the relative larger soil depth compared to the top layer that causes the large interflow.

27. Fig9: initiation of debris flow seems to be mostly related to the saturation of the second layer (which then determine the interflow).

R: That is true for the Johnathan Creek Basin, but it is not the case in the Big Creek Basin where the top layer controls the interflow magnitude.

28. L27-30 P8389: again, authors should discuss at the beginning the uncertainty of soil properties.

R: As suggested, we have added the paragraph shown below to Section 3.3.2 (L17, Pg.8383):

"Although there is certainly uncertainty in these soil properties, which in turn affect the calculation of both the hydrological response and also the slope instability analysis, we did not perform further investigation addressing these uncertainties, and assumed that the values extracted from the STATSGO are physically based and are representative of the actual soil properties in the region as in Tao and Barros (2013)."

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.

R: The related sentences are: “Yet, there is still a large number of unstable or nearly unstable locations at each time, which is an indication of spatial ambiguity. On the other hand, Figs. 13b and 12b show that interflow peaks locally at the time of initiation, which can be used as an additional constraint in assessing local instability.”

In fact, the spatial distributions of soil moisture, interflow for each soil layer and total interflow in the basins at the particular time when the debris flow occurred are shown in Fig. 10. We cannot show the spatial distribution of soil moisture and interflow for all time steps, unless through a movie. Nevertheless, for the spatial maps of SSI and FS shown in Fig.14, there are many pixels classified as unstable across the basin. The vertical profile of the factor of safety along the soil depth is shown in Fig. 13.

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.

R: The reviewer raises a very good question. One of the important points we wanted to make is that rainfall thresholds alone and, or high soil moisture are not strict indicators of failure potential. The common thread to the different applications is that shallow landslide initiation is triggered when the interflow peaks independently of watershed or storm type. We added the following discussion into Section 5 (L17 P8393), and also revised the associated sentences:

“Although the debris flow initiation time with respect to the beginning of the storm differs for warm and cold seasons and from basin to basin, interflow magnitude controls the flow responses for all the events and is closely related to the trigger mechanism of shallow landslide initiation followed by debris flow mobilization. We demonstrated that for all the three case-studies the interflow reaches the peak magnitude around the time when debris flows occurred at the initiation locations. That is, timing of debris flow initiation is that when the interflow peaks independently of watershed or storm type. Thus, we propose that the spatial ambiguity in FS prognostics can be addressed, at least in part, by monitoring the temporal evolution of interflow virtually using a modeling system such as described here. ”

2. L19 P8392: as said, the stability model is the same. SSI is a simplified version and implicitly defines failure at FS=1.

R: We agree with the Reviewer that the underlying fundamental theory of SSI and FS is the same, but as noted in the manuscript, the formulation of SSI does rely on an explicit relationship between soil wetness and slope. We understand the Reviewer’s concern, thus we revised the sentence as follows:

“Two slope stability models were utilized in this study derived from the infinite slope model using the limit equilibrium method, one is the modified slope stability index (SSI) calculated from soil wetness and slope neglecting cohesion and suction effects, the other is the factor of safety (FS) accounting for most of the dominant factors controlling slope instability.”

3. L20-22 P8392: I do not agree that is qualitative method. It's still dynamic and physically based, even if extremely simplified.

R: The Reviewer's point is well taken, as per our previous reply to Major Comment 3. We have revised the sentence as shown below:

"The SSI is based on tempo-spatial quantitative information about instability, but the subsequent aggregation of this information into threshold-based classes, introduces ambiguity. For instance, pixels classified as unconditional unstable are not necessary always highly susceptible to slope failure."

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.
R: We added this point into L5 P8393.

Minor issues:
L24 P8370: delete "of" Changed as suggested.
L25 P8373: delete and in Dietrich and et al. (1993). Changed as suggested.
L12 P8374: Fig.1 is not previously mentioned in the text. So, Fig.2 should be Fig.1. Then change figures numbering accordingly. Fig. 1 was mentioned after Fig.2. Now we mentioned Fig.1 in the introduction before the mention of Fig. 2. Therefore all the figure numbers are kept as same as before.
L4 P8375: define A, z and theta. Done as suggested.
L4 P8375: define tanphi. Done as suggested.
L17 P8375: What is (L) ? The L means the unit of pressure head. We included it there to indicate the pore-water potential is given as the head potential (m or mm), not the pressure potential (Pa). To eliminate potential confusion, we have eliminated it from the sentence.
L15 P8378: check consistency of table numbering. We have updated the table numbers.
L21 P8379: have been or are used? We deleted 'are'.
Fig.9: change colors accordingly for layers, between top and bottom plots. Also, specify a, b and c in the caption. Done as suggested. The revised Fig.9 is shown below.
Fig.10: specify a, b and c in the caption. Done as suggested.
Fig.14: specify 'FS' in the legend. Done as suggested.

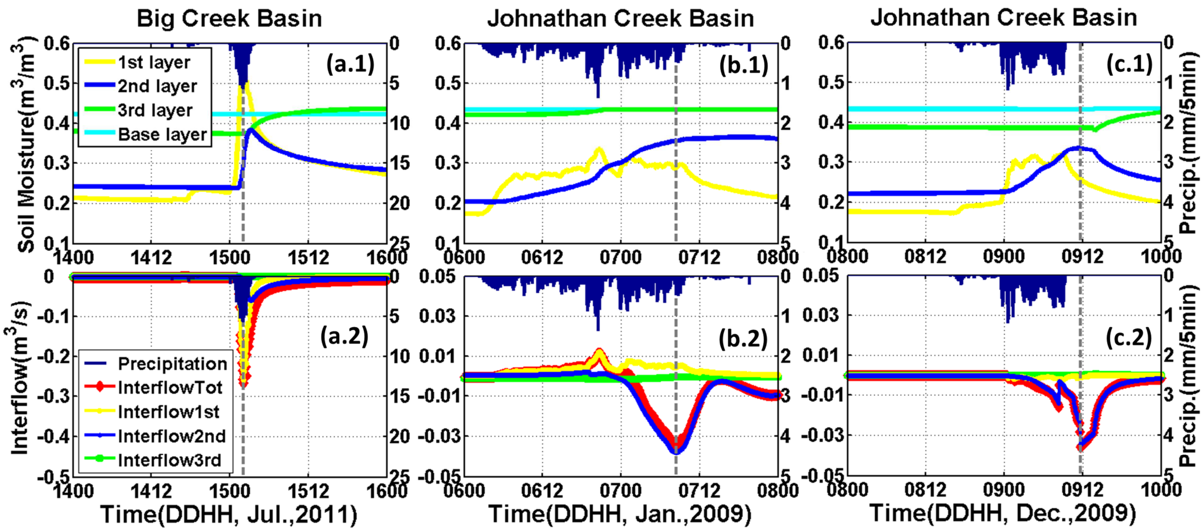

Figure 3 (Revised Figure 9 in manuscript) - The time series of soil moisture (top) and interflow (bottom) produced at each soil layer at the pixel in which debris flow occured. The x-axis is zoomed into the rainfall period to show details more clear. The dash lines indicate the time when the magnitude of total interflow reaches its peak.

References

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Thank you.