We would like to thank Reviewer #3 for her/his thorough review of our manuscript. We really appreciate all of the comments that, we believe, will help to improve our work. In the following pages we will try to respond to each one of her/his comments. We hope our responses will help in clarifying and improving the shortcomings she/he pointed out. Some points are common to those listed in Reviewer #2 comments, therefore some answers are a repetition of what posted in that document.

GENERAL COMMENT
1) The paper is very well written. However, I think that there are some shortcomings that need to be addressed and I suggest a revision. The innovative aspect of this paper is the application of a distributed model for assessing the hydrological conditions affecting slope stability in the vadose zone, even when the failure surface occurs in unsaturated soils. Thus, I think that the paper should provide a complete description of the model and the strategy employed for its implementation and parameterisation, by focusing on those modelling issues that are relevant for predicting the hydrological conditions leading to landslides triggering.

We thank the reviewer for her/his kind words. Our paper has already improved following the first 2 reviews and we believe it will greatly improve after these detailed comments.
As mentioned in the reply to Reviewer #2, the paper describes a new rainfall-triggered landslide module developed within an existing physically based spatially distributed framework (page 1334, lines 1-2, *existing* will be added to the text to further clarify which part of the paper is novel), the tRIBS-VEGGIE eco-hydrology model. Several previous studies described the development, parameterization and confirmation of tRIBS-VEGGIE (Ivanov et al., 2008a; 2008b; Flores et al., 2009; Sivandran 2012; Sivandran and Bras, 2012; Flores et al., 2012) hence, also due to the complexity of the model itself, a complete description of the model is not part of the scope of this paper. Instead, we provide a detailed description of the landslide component (section 2.2), which represents the actual novelty of the work. We feel very reluctant to add a section with the description of the model because it is not our personal work and it has been already published in several journals.
In order to facilitate the review of the equations and processes we consider, which indeed are key to a deeper understanding of the paper, we will add two references, **Ivanov (2006) and Sivandran and Bras (2012)**, on page 1338 (slide 6) line 5, at the beginning of the paragraph. These references are cited in other sections of the manuscript and report all the equations, assumptions and strategies adopted by the model.

2) Despite the title, the case study is of limited interest, since the simulated results are not supported by an adequate field data set. A large part of the paper is devoted to the discussion of simulated FS values obtained after a strong simplification of the spatial variability of the soil properties. Simulated FS values (or at least the predicted failing elements) are not compared with observation data of landslides events.

We believe that the title is clear and well in line with the scope of the manuscript: we perform a physically based analysis of rainfall-triggered landslides in the area of the Luquillo forest. In fact we analyze the physics of the process by using a distributed physically based model; given the type of landslides we are focused on (rainfall triggered), we stress the importance of a correct modeling of soil moisture dynamics and, finally, we use forcings, soil properties, geomorphology of the Luquillo forest. The scope of this paper is not to define a real-time forecasting of slope failure just yet, because we acknowledge (page 13 lines 8-9) the limits of the information available at this time. We completely agree with Reviewer #3 in that for a complete application of the model for real-time forecasting or to explain a specific landsliding event recorded in the area, the type of assumptions we made are significant. To properly respond to this comment, and to address one of the points listed by the editor, **we will add a paragraph to the revised manuscript. In particular we will clearly state the limit of the present work, at this stage, in the conclusive paragraph, to make clearer the scope of the work presented in this manuscript**.

SPECIFIC COMMENTS

3) I understand that the study does not develop a new model rather it applies an existing one. However, more information about how the model is implemented and parameterised should be provided to the readers.
According to cited references (Ivanov et al. 2008a), tRIBS+VEGGIE includes several modules, simulating several state variables and fluxes, with several parameters to be identified. Are all model parameters reported in the paper? How relevant are these parameters and the corresponding model components for the prediction of the slope stability?

As replied to the general comment, the tRIBS-VEGGIE hydrological framework has been previously presented in various works describing the development, the parameterization, validation and confirmation (Ivanov et al., 2008a; 2008b; Flores et al., 2009; Sivandran 2012; Sivandran and Bras, 2012; Flores et al., 2012). The water balance module is the most significant for prediction of the slope stability, and thus all the processes related to the water balance; in particular, the infiltration module (Richards equation) and the lateral moisture transfer play a significant role. More details on how this is parameterized is given in response to comment 4) and 5).

4) It is not clear how the Richards equation is integrated, how the boundary conditions are defined and at which depth the bottom boundary condition is located.
The bottom and surface boundary conditions can have a significant impact on the soil pressure head profiles and thus on the stability condition.
For what concerns the surface boundary condition, it is not clear how runoff is simulated, if runon infiltration is simulated and if the numerical integration of the Richards equation manages Dirichlet type boundary conditions imposed by the surface water depth where runoff occurs.

The model utilizes a finite element solution to the 1-D Richards equation for flow within the vadose zone. The boundary condition at the bottom of the soil profile is assumed as free drainage while constant fluxes are used as Dirichlet boundary conditions. The free drainage assumption is consistent with the location of the bedrock (>8m).
The time resolution scheme is based on a backward Euler time-marching scheme; iterations are made based on the Picard iteration method; the spatial approximation is generated by using piece-wise linear Lagrange polynomials as basis functions, which are assumed as weight functions in the used Galerkin finite-element method (to minimize the residual).
A complete and detailed description of the resolution scheme is given in Ivanov, 2006, reference that will be added to the revised text (see response to comment 1).

5) From Ivanov (2008a) I could understand that the hydraulic functions are defined according to Brooks and Corey (1964). This is quite important for a proper interpretation of the soil water content and pressure head profiles, particularly at pressure heads larger than the corresponding air-entry value.
I also read in Ivanov et al. (2008a) that lateral fluxes are simulated by introducing additional source terms in the Richards equation. However, no details I could read about this aspect, which is essential for understanding the role played by the "anisotropy ratio" and the results discussed in sections 5 and 6.

We definitely recognize the importance of definition of the soil retention curve for a proper interpretation of the soil moisture and the pressure head profile. Indeed, **the Brooks and Corey (1964) parameterization is used in the model, and this will be added in the text.**
As reported in the manuscript (lines 26-28, pag. 6), surface and subsurface moisture transfers among the elements are introduced after that dynamics of each computational element are simulated separately. In particular, "subsurface lateral exchange in the unsaturated zone is accounted for by adding sinks/sources

terms into Richards Equation" (Ivanov et al., 2008a). This is done by redistributing the subsurface flux within a soil layer of a contributing cell to the corresponding layer of the receiving cell based upon the unsaturated hydraulic conductivity of the latter one.

The paragraph 2.1, at page 6, lines 28 will be changed and it will read (the new text is in **bold**):
"The dynamics of each computational element are simulated separately, but spatial dependencies are introduced by considering the surface and subsurface moisture transfers among the elements; ***within each soil layer of a cell, the subsurface flux is redistributed to the corresponding layer of the receiving cell along the direction of steepest descent based upon the unsaturated hydraulic conductivity of the latter one, which*** affects local dynamics via the coupled energy-water interactions. ***The unsaturated hydraulic characteristics, both in term of hydraulic conductivity and soil water potential are related to soil-moisture content through the Brooks and Corey (1964) parameterization scheme (Ivanov, 2006; Sivandran and Bras, 2012), as a function of the saturated hydraulic conductivity in the normal to the soil's surface direction (K_{sat}), the air entry bubbling pressure, ψ_b and the pore-size distribution index λ.".***

SECTION 2.2
6) Eq. 3 - P. 1340 L. 26. Parameter "gamma_w" should be the specific weight of the water and not the water density.

Thanks for this observation. It will be changed in "***unit weight of water***".

7) Eq. 3 - P. 1340 L. 1-3. Variable "psi" is defined prior and after Eq. 3, with slight differences. To avoid any confusion, the sign of the third term of Eq. 3 can be changed and "psi" can be univocally defined as "pressure head".

Thanks for this observation. The sign of the third term will be changed and the Eq.3 will look like:

$$FS = \frac{c'}{\gamma_s h \sin\alpha \cos\alpha} + \frac{\tan\phi}{\tan\alpha} - \frac{\gamma_w \psi}{\gamma_s h} \cdot \left(\frac{\theta - \theta_r}{\theta_{sat} - \theta_r} \right) \cdot \frac{\tan\phi}{\sin\alpha \cos\alpha}$$

8) Eq. 3. - The soil depth "h" is not defined in the paper. Please, also note that Eq. 3 is written assuming that the soil depth is measured along the vertical direction, while, according to Ivanov (2008a), the Richards equation is integrated in the direction normal to the soil surface. Could be this difference relevant on steep slopes?

Thanks for this observation. We will add the definition of "h" as the soil depth. The reviewer's observation is correct and pertinent. We have accounted for the difference between normal and vertical direction in the code.

9) Eq. 3 and P. 1344 L20-23. Parameter "gamma_s" is not defined in the paper. Please specify how "gamma_s" is computed. To my knowledge, "gamma_s" is the depth average unit weight of the soil column above the examined depth, and it should change as the soil water content profile changes (unless additional approximations are applied).
These changes can be relatively important for the computation of FS if the bulk density of the soil is low, as indicated for the first 300 mm at P. 1344 L20-23.

Thanks for this observation. As the reviewer stated, the parameter gamma_s is the total unit weight of the soil at each soil layer or depth averaged above the examined depth, and it should change as the volumetric water content profile changes. In fact, gamma_s can be expressed as a function of the oven-dry soil unit weight (gamma_dry) and the volumetric water content (theta) as: gamma_s=gamma_dry+theta*(gamma_w).
Values measured by the Soil Survey of Caribbean National Forest and Luquillo Experimental Forest and provided in the NRCS report (Huffaker, 2002) are for the oven-dry soil unit weight (gamma_dry), with a depth average value of about 12.5 kN/m3 (very low). Then, the gamma_s was computed through the above expression considering a gamma_dry of 12.5 kN/m3 and an average value of theta of 0.45m3/m3, obtaining a

value of 17kN/m3. We verified that the use of such a constant value does not change the results so much when compared with the use of a varying gamma_s. We report here the comparison of two FS profiles, one obtained considering a constant gamma_s (gamma_s= 17kN/m3) and another for a varying gamma (gamma_dry=12.5 kN/m3), for a water content profile that varies as in the right panel.

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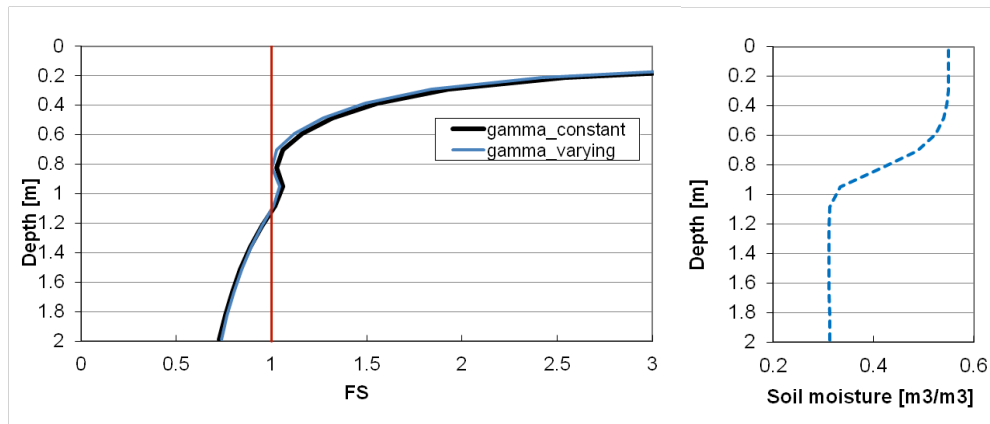
On another note, we realized, and we thank the reviewer for pointing this out, that the explanation of whether the gamma_s and/or other parameters, such as bulk density and the hydraulic conductivity, were varying with depth or not hasn't been carried out properly in the manuscript and it is inconsistent throughout the paper.
tRIBS-VEGGIE has the capability to set all three parameters, gamma_s, bulk density and hydraulic conductivity, as varying with depth. These parameters have been, indeed, considered as varying in the validation example (shown in Section 5), together with the constant case. However the final set up, based on the result of the validation, was with all the parameters set constant. In fact, during the validation phase big differences were not found between the results of the runs with varying parameters and those with constant ones. By considering them constant, without affecting the final soil moisture and FS profile, the general model and the interpretation of the results are simpler. Clearly this is not well stated in the document and we will work on making this point clear.

10) Actually, the statement at P. 1344 L20-23 is not clear. What is the bulk density employed in the model simulations?
    The large variability of the bulk density is not consistent with the assumption of uniform soil water retention properties and uniform soil mechanical properties.

We clarified the issue in the previous answer. We will definitely correct the text to make the statements consistent throughout the paper.

11) P. 1346 L. 1-4. The paper does not describe the model employed for simulating the cohesive effect of the roots.

As already responded to reviewer #2, we have used one cohesion parameter that includes the cohesive effect of roots; the vegetation was set homogeneous to the whole area. Current research is focused on the extension of this work to dynamic vegetation with dynamic rooting which can be expressed with a separate cohesion component.

SECTION 4
12) In many points the paper is rather vague about how the model is implemented and applied: P. 1344 L24-25. How does the saturated hydraulic conductivity change with depth?

Similarly to what stated in point 9, the saturated hydraulic conductivity is not assumed to change with depth.

Instead, as explained in point 4), the unsaturated conductivity is assumed to be function of the saturated conductivity, according to the Brooks and Corey (1964) parameterization scheme.
The text will be corrected.

13) P. 1345 L.10. Please, explain "..to ensure the occurrence. . . ".
It will be taken out, and it will read:
"For simplicity this study will assume spatially homogenous values of cohesive strength (3 kPa) and friction angle (25°) over the entire basin".

14) P. 1345 L. 27-29. How is the root water uptake parameterised?

The root water uptake (i.e., transpiration) is treated as a sink of soil moisture. The manner by which this sink is extracted from the soil profile depends on the rooting architecture of vegetation, described with a root-depth profile that is dictated by the type of vegetation being modeled. Thus, the Richards solution incorporates the transpiration (and evaporation and infiltration) as sink (and sources) distributed within the soil moisture profile (Ivanov et al., 2004, Sivandran and Bras, 2012). Transpiration is computed as:

$$T_i = V_f E_T^{veg} B_i r_i$$

where T_i (mm) is the transpiration from layer i, V_f (-) is the fraction of the computational element that is vegetated, and B_i (-) is the transpiration efficiency of layer i. r_i (-) is fraction of the roots in layer i.
For complete details on the transpiration parameterization refer to Ivanov et al., 2008a and Sivandran and Bras, 2012.

15) Table 2 – The units of "K_s" should be corrected. Given the value ranges for "K_s" in the table, what are the actual values employed for the simulations? Please, also change the symbol indicating the air entry value ("psi" is already employed for indicating the pressure head).
Thanks for these observations. Units of K_s and symbol of psi will be corrected and modified.

Regarding the range of K_s used in the paper, again this is due to the confusion stated in point 9).
The values used in the *validation exercise* were, i.e. for Clay-Loam, three, a constant with the low end value (20), a constant with the high end value (50) and a varying case ranging from 20 to 50 mm/hr. As stated in point 9) the final set up was a constant one, in particular, with the highest conductivity for all cases. We will definitely improve this table once we address the comments in point 9) and 10).

SECTION 5
16) The strategy employed for validating the hydrological model is not clear.
For the validation, the distributed model has been applied to a smaller basin "for faster simulation time and a finer mesh" (page 1346 L.22). I understand that the Authors aimed at reducing the scale mismatch between simulation and observation support scales. However, the Authors should provide more evidences about the validity of their approach.
Due to the well-known scale effect issues, the results of a distributed model at elemental scale are often highly influenced by the spatial resolution with which the model itself is implemented. Validating the results of a model applied with a finer computational mesh does not necessarily provide relevant information about the model performance with a coarser computational mesh.

We completely agree with Reviewer #3 comment on scale resolution and its effects on model results. This is a very important topic that is well known to any modeler. However we want to point out why this is not an issue in our application. The "validation" we perform has the sole intent to show the ability of tRIBS to correctly reproduce soil moisture dynamics in a very wet environment. In fact, the model had had been extensively used in previous works only within dry climate. Our validation exercise (see response to reviewer #2 for a more in depth characterization) showed that tRIBS is able to correctly reproduce the soil moisture dynamics and this gives us confidence in its ability to correctly model the soil moisture dynamic in tropical environment. By using a smaller mesh for the validation exercise we tried to get *closer* to the point

measurement scale. Surely, the results obtained at larger scales will differ from those at smaller scale (although some preliminary results seemed not to show a great difference); however the same differences would be seen when comparing point measurements with spatially averaged measurements. Therefore we do believe that the results presented in the validation exercise are valid and representative of the ability of tRIBS-VEGGIE in reproducing correctly the soil moisture dynamics of the area. To address this comment and clarify the final scope of the paragraph, we will add on page 1346 and line 14 the following text: ***This confirmation has the intent to investigate the capabilities of tRIBS-VEGGIE to correctly reproduce the soil moisture dynamics in a tropical environment.***

17) Why only three out of nine observed soil moisture time series (shown in Fig. 3a) are compared with the simulated ones? What are the parameters employed during the model validation? Do the simulated time series in Figs. 3c, 3d and 3e correspond to three different anisotropy values and what are the relevant values? What are the "different simulated series . . . obtained by varying the anisotropy ratio" (P.1347 L. 17-19)?

As already responded to reviewer #2, the validation exercise required a large amount of runs and data analysis before being able to reproduce the data as good as we show; indeed, there was a sensitivity analysis although it wasn't carried out quantitatively. It analyzed the effects on the soil moisture dynamics of: (1) using the hydraulic soil properties considering both Rawls et al. (1982) (RAWLS) tables and Clapp and Hornenberger (1978) (C&H) tables, (2) slightly varying the saturated water content to match the measured values, (3) considering the saturated conductivity listed in RAWLS and C&H papers and listed in the local NRCS publication, (4) varying the anisotropy ratio values from 1 to 300 with increments of 50. Of all these runs the parameters that allowed the best fit are those listed in table 1 and in the text.
We agree with the reviewer that we can be more specific about these and we will clarify which are the final ones in the revised manuscript. Moreover we agree with the reviewer that this section is a bit confusing, we will improve the explanation and the general structure of this paragraph.

18) How is the terrain in the location where soil moisture probes are located (i.e., slope, upslope contributing area, curvature. . . .)? Is the lateral flux expected to be relevant in the locations where the soil moisture is observed? What is the length of the CS616 probes? How were inserted (vertically or horizontally)?
The information about these probes was limited. They were used in another work (cited in the manuscript as T. Wood personal communication 2010) that hasn't been published yet. These measurements weren't the main focus of the authors' work – whose main interest is the ecosystem ecology and the biogeochemistry of the soil, therefore the mapping and the details of locations weren't completely available. They were inserted vertically, we believe that the lateral flux is relevant throughout the whole area (see response to comment 22).

19) It is advisable to add some quantitative evaluations (compute indices of the differences between simulated and observed soil moisture) to support the qualitative statement that "the model does a very good job".
As explained in point 16 and 17, the goal of the validation was to investigate the ability of the model to reproduce correctly soil moisture dynamics in tropical environment rather than back extrapolate the exact correct set of parameters. We have performed various simulations, as listed in point 17), and then we showed the best results that could reproduce the highly variable point measurements. In order to evaluate which of the many simulated time series was the best one, we have used the RMSE as performance metric.

20) P. 1347 L. 10-13. Provided that validation data are rather scarce, the effect of the spatial resolution on the predicted slope stability could be one focus of the study.
We thank the reviewer for her/his input on this. We are moving forward with this work and we are looking into this as well. Very preliminary results seem to show not an obvious or well-defined difference among different resolutions, we will definitely look more into this in our future work.

21) P. 1347 L. 22. ". . . the maximum value of the soil moisture". This is not so relevant provided that soil water content is a bounded variable and the maximum value coincides with the saturated soil water content (P.1347 L.3-4).

The saturated soil water content was not clearly known, its values ranged largely depending on literature, therefore we used the maximum soil moisture content measured in the field (time series presented in section 5) as a proxy for a measurement of saturated soil water content.

22) P. 1348 L. 3-6. Do the Authors really expect that the differences between the observed and simulated values can be solely attributed to the uncertainty in the anisotropy ratio?

We reproduce here the response to comment #3 of Reviewer #2, which fully address comment #22:

The choice of such high values was made to make the model able to describe some in situ water dynamics, which are not only affected by the soil texture characteristics (easy to be modeled) but also by the particular environment (animal activity and vegetation), more difficult to be reproduced even by a complex eco-hydrological model.

In fact the literature related to the analyzed area, has often pointed out the high variability of infiltration rates (Harden & Delma Scruggs 2003, NCRS 2002). In particular, as mentioned in Harden & Delma Scruggs (2003) – and partially reported in our manuscript on page 12 and line 17: "*Jetten et al. (1993) found the sample variance of infiltration rates for tropical rainforest soils to be so large that it was not possible to predict infiltration rate as a simple function of soil properties. They found that variations in infiltration rates were not explained by soil texture and suggested that animal activity, vegetation, and climate strongly affected the distribution of infiltration rates*".

Later in the same paper: "*During one sustained rainstorm, we climbed around in the Bisley 1 watershed with a soil auger to study the response to natural rain. We observed little to no runoff flowing across the surface but did observe a consistently wet zone in the top 3–5 cm of soil (sometimes as deep as 10 cm), where water was visibly draining downslope through a near-surface fine root zone. Soil below this depth was not saturated, in spite of the steady rain. Throughout our experiments in the Luquillo Experimental Forest, we found numerous earthworms in our samples. According to National Forest personnel, earthworms are considered to be the faunal species with greatest biomass in the Luquillo Experimental Forest.*" These earthworms are believed to create a network of macro-pores that allow for a quick redistribution of the moisture.

The experimental results reported by Harden & Delma Scruggs (2003) are consistent with the 9 measurements we have used for our validation: although they are recorded within a limited area, their variability is particularly high; the soil type of the area is mapped as homogeneous, and the slope angle variability is not high. Therefore under a mechanic and geomorphology point of view this area would be considered as uniform; however the measured variability cannot be explained by a simple soil function, as reported by Harden and Delma Scrugg (2003).

For all these reasons we have to introduce some variability in the parameters, different anisotropy ratios within the same soil type, and we need a higher than measured in laboratory value for this parameter. In particular, the values have been chosen after a sensitivity analysis of the soil moisture dynamics to the anisotropy ratios, during the validation exercise.

However, we don't think this is a drawback for our analysis: this confirmation exercise, as mentioned in our manuscript (at the beginning of section 5, page 14), was carried out to show how tRIBS-VEGGIE is capable to handle climates that are very different from those within which it has been already largely validated and studied (Ivanov et al., 2008a; 2008b; Flores et al., 2009; Sivandran 2012; Sivandran and Bras, 2012; Flores et al., 2012). With this purpose in mind we wanted to show that, without altering the architecture of the model, without changing the governing equations, and with using parameters that are consistent with the available literature in the area we could successfully reproduce the measured data.

The results presented in section 5 were probably not well explained, but the statement the reviewer refers to ("The model captured most of the observed soil moisture dynamics, but the high variability of soil measurements suggested that is not, at present, possible to identify a unique value of anisotropy to use in other locations") is definitely supported by the results. The variability of the measurements is striking and our validation exercise showed that the best way to match as many as possible measurements was to vary the anisotropy. We will modified the section 5 of the manuscript to make it clearer, but we the current version already states: "The different simulated series are obtained by varying the anisotropy ratio values only, all the other parameters are kept constant".

SECTION 6

24) In Section 6.1, the criteria employed for the selection of the three elements are not clear. Beside the slope, I think it is important to mention other terrain attributes of the elements which are relevant for the simulated lateral flow (e.g., curvature, upslope contributing area, etc.). The computed FS values are of limited interest per se, since no validation data (e.g. data of landslide events) are examined and a strong simplification has been adopted for the soil mechanical properties. Similarly, I do not think that it is relevant to compare FS values of different elements characterized by different slopes. I would rather compare the evolution of the dimensionless ratio (gamma_w*psi*chi)/(gamma_s*h) which describes the effect of the soil suction in FS, applied to elements characterised by similar terrain attributes. Note that the product "psi*chi" can be univocally defined as function of "psi".

As clearly stated in the manuscript (P1348, L18-24) the three elements were selected so that they fell in the three different soil types and could show interesting FS dynamics in time, in depth and by changing the anisotropy ratio (e.g. dynamics from stable conditions to unstable conditions). Moreover, the elements have been selected in a way to have similar steep slope values; kept in mind that was impossible to have exactly the same values. Note also that there is a typo in the manuscript: in fact, the slope value of the clay element, C, is **56° (0.98 rad)** and not 28° (0.49rad), as can be observed in the maps of Figg. 2a and 2b of the manuscript. Therefore, we believe that the reviewer statement "I do not think that it is relevant to compare FS values of different elements characterized by different slopes" is a deduction from our wrong sentence. The considered values of 0.98, 0.90 and 0.82 belong to the same class in the slope classification reported in fig. 2b.

As reported in the manuscript, "The CL and C elements are located upslope, while the SL element is closer to the river network" (P1348, L21-22).

We agree that "the computed FS values are of limited interest per se"; in fact, we focus our discussion on the dynamics of the FS values as a function of the soil moisture dynamics, which are not invalidated by the adopted simplifications. We have compared three elements representative of three different hydrological properties, which lead to different responses to the rainfall forcing (in time and in depth) in term of water content, suction and finally factor of safety. We appreciate the reviewer's suggestion about comparing "the evolution of the dimensionless ratio (gamma_w*psi*chi)/(gamma_s*h)" and defining "psi*chi" univocally as function of "psi", but we believe that would not change the outcomes of the results discussion so much in spite of a deep reorganization of the paper.

25) It is difficult to follow the discussion of figures 4-6: please check the correspondence between the times "t_b-t_e" cited in the manuscript and in the figures.

Thanks for this observation. We have uploaded a previous version of the figures. The figures within section 6 should report *ta*, *tb*, *tc*, *td* **and not** *tb*, *tc*, *td*, *te*. Therefore whenever in the text we referred to *ta* the corresponding figure/plot is the one labeled with *tb*, when in the text we referred to *tb*, the figure is labeled with *tc*, and so on. We will upload the correct figures and check that text, figures and captions are all consistent.

26) Anyway, it would be more appropriate to analyse FS profiles by examining the temporal evolution of pressure head rather than of the soil moisture content, particularly for saturated soils or soils close to saturation. The soil gets saturated when the pressure head is equal to the air entry value. In the range of pressure head values between the air entry value and zero, the effective saturation ("chi") is always equal to 1 and only the pressure head controls the variability of FS. The effect of the soil suction on FS at shallow depths can be still relevant for pressure heads larger than the air entry value.

We understand the concern of examining the pressure head instead of the soil moisture in relation to the temporal evolution of the FS; however, considering that the two variables are uniquely linked via the soil retention curve (with no hysteresis), here modeled by means of the Brooks and Corey (1964) parameterization, and that the soil moisture gives an order of the amount of water infiltrating into the soil, we believe that the analyses of the soil moisture dynamics are definitely appropriate as well. Again, given all the parameters of the soil retention curve (reported in table 2 of the manuscript), it is possible to estimate the corresponding psi value.

27) Figures 4-6. To my experience, contouring the results of a dynamic process in a spacetime plane can introduce spurious results in the maps, such as transferring back in time the effect of a wetting front moving forward.

The results shown in figg. 4-6 are extrapolated from results at basin scale. Indeed, the corresponding values of soil moisture at given depth can be also read in the corresponding map of soil moisture spatial distribution. The figures may be interpreted as screenshots in time and space of a 3D dynamic pattern.

28) Figures 7-8. Again, in order to assess the effect on FS, I would look at the pressure heads rather than at soil water content maps, particularly when comparing soils characterised by different soil water retention curves. P. 1351 L. 28. A comparison of the effect of the soil suction for different soils (characterised by different water retention functions) can be easily assessed by examining "psi*chi" as function of "psi".

We have already replied to this in our response to comment 26)

29) P. 1352 L. 1-2. This is quite obvious: what could one expect from flat areas?

Yes, it is quite obvious, but still it gives a key-lecture of the map.

30) P. 1348-1355. Please clarify the meaning of expressions such as "fully saturated conditions" or "full saturation". Do these refer to a complete saturation of the soil column above a given depth?

Yes, they do.

31) P. 1354-1355. Please check expressions such as "impermeable soil" and "permeable soil". I think it is more appropriate to refer to soils with different permeability. Also consider that some results are representative of the examined synthetic case study and reflect the hypotheses behind the model and its implementation (e.g., boundary conditions, parameters, etc.).

With permeable soil we refer to soils with high saturated hydraulic conductivity and so the opposite. We will change the text as: "*in the more permeable soil*"; " *in the less permeable soil*". We agree that the results reflect the hypotheses behind the model, but it is also a general understanding the infiltration dynamics and consequently the soil moisture content mainly depends of the hydrological parameters that are not hypothesis behind the model rather are representative of a soil characterization (which again are not dependent on the model)

32) P. 1354 L. 23-25. No simulation is required to support this statement. As stated above, this can be obtained by examining "psi*chi" as function of "psi" for soils characterized by different water

The statement ("in a impermeable soil, the effects of the rainfall event on slope stability are delayed") describes the evidence that the model is able to reproduce, i.e. the relation between hydrological properties (as response to a rainfall event) and the slope failure process.

33) P. 1355 L. 10-12. What are the soil parameters? I think that the anisotropy ratio as well as the hydraulic conductivity can be also considered as "soil parameters".

With soil parameters here we meant the parameter of the soil retention curve. We agree with this observation, so it will be changed as follows: "*The results of such a process driven methodology highlight the importance of the parameterization of hydrological processes, not only for hydraulic conductivity and **parameters of soil retention curve,** but also in term of anisotropy coefficient for lateral redistribution*".

REFERENCE (in bold the new references)

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