We would like to thank Reviewer #2 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.

1) The title, the abstract and the introduction highlight that the work is devoted to the development of a physically-based model for rainfall-triggered landslide analysis. However, the description of the tRIBS-VEGGIE model (section 2.1) is very poor. In detail, the modelling of the lateral moisture transfer among the elements is not clear and, because of its great role in the shown analysis, it has to be commented in detail. Similarly, the VEGGIE module is only qualitatively described but the governing equations are not shown.

The paper describes a new rainfall-triggered landslide module developed within an ***existing*** physically based spatially distributed framework (page 1, 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 do agree with Reviewer#2 that the governing equations are very important for a deeper understanding of the results, therefore to facilitate the review of the equations we will add two references, **Ivanov (2006) and Sivandran and Bras (2012), on page 6 line 5**, at the beginning of the paragraph. These references are cited in other sections of the manuscript and report the equations adopted by the model.

We agree with the reviewer in highlighting the significant role of the lateral moisture transfer; 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. A complete description of the formulation of the unsaturated hydraulic conductivity is given in response to comment 4).

Following the reviewer's suggestion we will modify the paragraph 2.1, at page 6, lines 28. 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."***.

The VEGGIE model focuses on the vegetation component and has the ability to model a variety of processes, some listed in the manuscript (page 7, line 9 to 19). Moreover, as we state in page 13 line 13-15, we do not fully use this part of the model because we consider a static vegetation, i.e most of the vegetation characteristics do not change in time, as for example the rooting profile. We will add on page 7 line 18: ***The coupled model can simulate biophysical energy processes (short- and long-wave radiation interactions,***

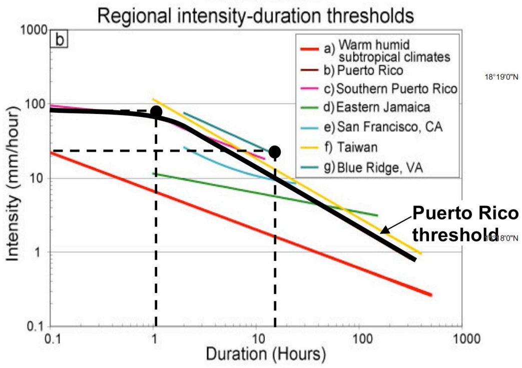
canopy and soil evaporation, energy flux partitioning, and transpiration), biophysical hydrologic processes (interception, stemflow, infiltration, runoff, run on, and unsaturated zone flow) and biochemical processes (photosynthesis, plant respiration, tissue turnover, vegetation phenology, and plant recruitment) [Ivanov et al., 2008a, 2008b] The role of vegetation in this application is the extraction of soil moisture for the purposes of transpiration. The dynamic model allows for a realistic representation of the controls of this soil moisture sink. Transpiration is primarily controlled by: vapor pressure deficit, soil moisture levels, rooting profile, leaf area and available energy.

2) The authors stated that the work is devoted to the stability analysis of rainfall-induced landslides and at this aim they introduce within the tRIBS module a modified form of the FS equation able to take into the account the positive effect of matric suction on shear strength. However, in the stability analysis performed they do not differentiate, from a mechanical point of view, the three different soil types (which range between a fine-grained soil and a coarse-grained soil!) and they assume that all the slopes are in homogeneous soils (absence of stratigraphic sequence). At basin scale this latter assumption could be accounted for simplicity by considering the predominant soil type (as the authors do), but the first assumption is not acceptable since they should know that even small changes in cohesion, or even in the friction angle, can determine remarkable variations in the safety factor. This implies that all the results shown in terms of FS are invalidated by this assumption.

The assumption that all slopes have homogeneous soil columns (absence of stratigraphic sequence, in this case to a depth of 200 cm), is consistent with the characteristics of shallow landslides, here modeled, which often occur in homogenous soils. With regard to the mechanical properties of the three different soil types, we are of course aware that "even small changes in cohesion or even in friction angle can determine remarkable variations in the safety factor". However, we disagree with the reviewer comment, "the results shown in term of FS are invalidated" by the assumption of homogenous values parameters; in our work we analyzed from an hydrological point of view the relationship between the soil moisture dynamics (mainly due to infiltration, lateral redistribution and other hydrological processes, all correctly modeled in tRIBS-VEGGIE) and the failure phenomenon. Therefore, even if a specific landslide is strictly dependent on the local geotechnical parameters, these are not fundamental for a correct interpretation of the overall relationship between hydrology and the slope failure process.

As a matter of fact, did the authors verify if some slope failures have been triggered by the rainstorm of April 2008 and if their localization correspond to the black pixels in their analysis (figures 7 and 8)?

Unfortunately we have little details about the most recent failure events; the available landslide database, eg. Lepore et al. 2011 and reference therein, does not report the time of failure, but only the location, moreover the Bisley tower was not deployed yet at that time.
The April 2008 event did cause failures, some landslides were measured outside of the basin, but we were not aware of specific landslides within the basin. The rainfall intensity, however, falls well above the regional intensity duration thresholds, for both short and long durations, as shown in the figure below (extracted from Kirschbaum PhD thesis and obtained as a personal communication in 2009). Therefore it is reasonable to believe that this rainfall represents an event capable of causing slope failure.



An effort should be made in order to introduce the variability of the mechanical properties of the soils even with data from literature referring to soils type characterized by grain size distributions similar to those of the soils under investigation.

In the phase of the definition of the experiments, the authors have thoroughly reviewed the literature available on this area, in order to characterize with as many details as possible the characteristics of the soils under investigation. However, to our knowledge, there is no more information that what we have stated in the manuscript (page 13 line 6-13):

Simon et al. (1990) and Lohnes and Demirel (1973) reported values for cohesive strength and friction angle for some of the geological units, and illustrated the expected high variability of these two quantities. The uncertainty associated with these parameters is large when we consider their spatial distribution across four soil types within the domain.

We, of course, agree with Reviewer#2 that the more data and parameters we have about the area the more our results will be closed to reality. However, as stated above, at this stage of the model, the choice of homogenous values of cohesive strength and frictional angle allows for an easier understanding of the links between hydrological and failure processes in case of shallow rainfall triggered-landslides, which in turn provide a more efficient assessment of the proposed methodology.

Differently, they should clearly declare, modifying the title, the abstract and, in general, the paper, that the analysis is devoted only to analyse the effects of hydraulic characteristics, with particular reference to anisotropy, on slope stability.

We beg to disagree with the reviewer on the consequences of the intent of the paper. We believe that both the title and the abstract are 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 (page13 lines 8-9) the limits of the information available at this time.

We will add more text to stress the specific intent of this work but we don't think we need to modify the title or the abstract in an aggressive way. In particular (new text in ***bold***):

- Page 2 line 17, will read: The new modeling framework couples the capabilities of the detailed hydrologic model to describe soil moisture dynamics with the Infinite Slope model creating a powerful tool for the assessment of ***rainfall triggered*** landslide risk.
- Page 5 line 17: This model can be utilized to test different rainfall scenarios at the catchment scale to identify high-risk storm characteristics***; in the future, when more data and measurements will be available, it*** can possibly be integrated into an early warning system.

3) The model validation (section 5) is not so clear in my opinion. A number of points should be clarified.

- First, the saturated conductivity anisotropy ratio a_r adopted in the analysis (100-300) seems unrealistic: usually the variability of this parameter in nature is in the order of 2-10. Could the authors refer to data from literature to support this choice which deeply influences the analysis? Or such high values have been introduced exclusively to reproduce the in situ water content measurements? With this respect, as already pointed out, the authors do not provide any detail about the model of the lateral fluxes. This makes the reader suspect that such extremely high anisotropy ratios are due to the adopted assumptions. Moreover, it could be also assumed that such high values of a_r are necessary to simulate the presence in the stratigraphic sequence of a soil layer more permeable than the modelled one. Are some borehole investigations available for the instrumented sites? In this case, please add this information along with a synthetic description of the soil-layers.

The choice of such high values was made to make the model able to describe the complex 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, geomorphology and geotechnical point of view this area would be the same, 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, as explained below.

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.

With respect to the model of lateral fluxes, we agree with the reviewer in adding a more detailed description, as already discussed in response to comment 2).

- The performed back-analysis of the in situ water content measurements is not clear. The measurements have been taken at three locations, each instrumented with three TDR probes installed at the same depth of 30cm: so, at each location, the authors have three water content measurements representing the spatial variability that this parameter assumes. Is it correct? Is each location along a slope or in a flat area? In the first case, should the variability of the water content be representative of the different position of the devices along the slope?

The reviewer has correctly interpreted the description of the available measurements, as reported in the manuscript. The measurements are within a very small area with very low variability in terms of slope angle, mostly flat.

- in describing the "validation exercise" the authors state that the saturated hydraulic conductivity and the anisotropy ratio are the most important parameters in order to reproduce the observed data (page 15, lines 1-9). Did they perform a sensitivity analysis to say that? In this case, please describe it clearly. Otherwise comment the sentence. Finally, what does the sentence "The different simulated series are obtained ….kept constant" mean? (page 15, lines 17-19). I've deduced that the soil type is the same, hence the hydraulic characteristics do not vary in the three simulations, but different values of a_r have been adopted to reproduce the measurements time series. Is it true? In this case this seems in contrast with the previous sentence (page 15, lines 4-5) where the authors say that "several simulations were run varying saturated hydraulic conductivities…..100 and 300". Please clarify and give the adopted value of a_r for each simulation.

The scope of this paragraph was to show the capability of the model to reproduce the soil moisture dynamics in a very wet climate. Therefore we limited the size of this paragraph to the results.

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 measured quantitatively. It analyzed the results 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 review that we can be more specific about these and we will clarify which are the final ones. Moreover we agree with the reviewer that page 15 is a bit confusing, we will improve the explanation and the general structure of this paragraph.

4) Finally, information about unsaturated hydraulic characteristics of the modelled soils are completely missing (section 4): how did the authors model the water retention characteristics curves of the soils and the unsaturated conductivity functions? Did the authors assume anisotropy coefficients for the k_{unsat} equal to those assumed for k_{sat}? Please detail and comment on it. The parameters reported in table has to be verified: please, check the unit for k_{sat} and add in the text what ψ and λ should represent.

Then, please insert a column with the range of variation of k_{unsat}.

The unsaturated hydraulic characteristics, both in term of hydraulic conductivity and soil water potential (water retention curve) are related to soil-moisture content through the Brooks and Corey (1964) parameterization scheme (Ivanov, 2006; Sivandran and Bras, 2012). The following equations are used by the model:

$$K_{unsat} = K_{sat}\left(\frac{\theta-\theta_r}{\theta_s-\theta_r}\right)^{\frac{2+3\lambda}{\lambda}}$$

$$\psi = \psi_b\left(\frac{\theta-\theta_r}{\theta_s-\theta_r}\right)^{-\frac{1}{\lambda}}$$

where K_{sat} [mm/hr] is the saturated hydraulic conductivity in the normal to the soil's surface direction, ψ_b is the air entry bubbling pressure [mm] and l is the pore-size distribution index [-]. Values of those parameters for each soil type are shown in the Table 2 of the manuscript. These last two equations are completely described in the two references mentioned in the response to point 1) (**Ivanov (2006) and Sivandran and Bras (2012)**). However, we recognize the importance of this information for a correct interpretation of the model behavior; therefore we will mention the used parameterization scheme in Section 4. We assumed the same anisotropy coefficient for both the conductivities.
We verified the parameters reported in the table, thanks for the observations. In particular, values of K_{sat} are in mm/hr; while ψ_b (which in the table is indicated as ψ) and λ are respectively the air entry bubbling pressure [mm] and pore-size distribution index [-]); we will add it in the text.

Specific comments
Section 2
Page 6, line 3. The tRIBS-VEGGIE model is defined as an "eco-hydrological" model. I have not found anything in the manuscript with reference to "ecological" aspects of the investigated phenomena.

As specified in the text (page 13, lines 13-15) and in response to comment 1, in our specific application, "we do not fully use this part of the model [VEGGIE component] because we consider a static vegetation", without explicitly considering "the ecological aspects" of the investigated phenomena. The role of vegetation is not explicitly analyzed with regard to the failure process, but it indirectly affect it because of the extraction of soil moisture by roots for the purposes of transpiration.
tRIBS-VEGGIE model has been developed following the idea that the hydrologic, ecologic, and atmospheric systems are not isolated but rather part of a more complex series of multidirectional interactions (Sivandran and Bras, 2012). Its framework and structure reflects this idea and its capabilities can encompass ecological aspects of hillslope hydrology (as stated above).

Page 6, lines 26-29. The statement "The dynamics of each computational element …. via the coupled energy-water interactions" should be better explained introducing the adopted equations.

tRIBS-VEGGIE reproduce a variety of processes and the equations behind the energy-water interactions are many. The focus of this paper is on the development of the slope stability module and for this reason we have no reported many details about the whole model. We addressed the corrections we will make in the response to comment 1).

Page 9, lines 2-3. In the equation (3) ψ is described as matric suction (assuming positive value) in unsaturated conditions and pressure head (once again assuming positive values) in saturated conditions: such a way the third term at second member in eq. (3) always assumes a positive value, whereas in saturated conditions the presence of positive pore pressures acts decreasing the safety factor of the slope. Please verify.

Thanks for this observation. This is a typo. We will substitute the equation with the following:

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

where the third term is now negative, and the term ψ is the matric suction assumed as negative value (in fact, in the Table 2 values of ψ_b are negative) in unsaturated conditions, or positive pressure head in saturated conditions.

Section 4

In describing "surface data and parameters" no information is available regarding the thickness of the soil deposits and the depth and nature of bedrock. These information are very important when hydrologic and stability analysis are faced. The authors should add these information if available or the assumption made in their regards.

We definitely agree with the reviewer that the thickness of soil deposits and bedrock location are important in stability analysis, especially for shallow landslides. In our scheme, the failure mechanism is not affected by the presence of the bedrock, since it is known to be at 8m deep or deeper (Simon et al., 1990). We consider the whole vertical computational mesh as a homogeneous soil deposit of 2000mm, with failure surfaces corresponding to whatever depths the FS approaches 1, mostly at shallow depths (consistent with the type of landslides we focus on; see figure 7-9).

Page 12, lines 24-27. Is the saturated hydraulic conductivity assumed linearly decreasing with depth? Did the authors assume the same for unsaturated hydraulic conductivity?

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.

Page 13, line 10. The sentence "…to ensure the occurrence of a useful number of failures.." is obscure. Please clarify.

It will be taken out, and it will read:
"For simplicity this study will assume spatial homogenous values of cohesive strength (3 kPa) and friction angle (25) over the entire basin".

Page 14, lines 1-2. Please explain how the cohesive effect of roots have been incorporate into the soil cohesion term since it can strongly affect the FS in the first 40cm of the soil deposit.

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 5

What did you assume in terms of initial conditions? Have you done some hypothesis on the position of the ground water table?

The basin has been imposed to achieve stable conditions after a spin-up period of simulation. It is an initialization process during which a model adjusts itself and moves from the initial conditions to an equilibrium state (Yang et al., 1995). In particular, we ran tRIBS for a spin up time of one year and a half with different soil moisture initial conditions ($\theta_0 = \theta_r, \theta_0 = \theta_s$ and $\theta_0 = \left(\theta_s + \theta_r\right)/2$). All runs would converge onto each other after about 6 months in the run, providing the initial conditions of the basin both in term of soil moisture and ground water table. All the results we presented are extracted after the spin up time.

Section 6

Page 16, lines 9-11. Why did you use a_r coefficients (equal to 300 and 500) highly greater than those adopted for the validation of the numerical model?

The high values of anisotropy coefficients were used to 'stress' the model and bring the basin to very different conditions in term of soil moisture spatial distribution. We analyzed how high anisotropy values could possibly affect the soil moisture redistribution, to evaluate how a very rapid lateral moisture exchange, which is common in the basin area (as specified in comment 3), may affect the landslide assessment of the area. Moreover, the high variability of the 9 available measurements in such a small portion of the territory, suggest how the soil characteristics of this area could be very heterogeneous. Therefore including a higher range of values of this important parameter made sense.

We will add a sentence in the description of the experiment to explain our reasoning in picking this range of values (as mentioned in our response to comment 2). It will read (page 16, line 9): "In order to explore the sensitivity to anisotropy ratio of the slope stability model, three anisotropy coefficients, a_{r1}, a_{r2} and a_{r3}, equal to 1 (isotropic soil), 300 and 500 respectively, were simulated***. The high values of anisotropy coefficients are used to stress the model and bring the basin to very different conditions in term of soil moisture spatial distribution, and thus to evaluate how a very rapid lateral moisture exchange, which is common in the basin area, may affect the landslide assessment of the area."***

Technical comments

Page 6, line 10. Insert "resolution" after "very fine temporal".

It will be done

Page 14, line 23. Fig. 2a not 1a.

It will be done

Page 16, line 21. Fig. 2b not 2a.

It will be done

Across the entire section 6 there is no correspondence between the time chosen to describe the analysis (ta, tb, tc, td and te) in the text and within the figures. Please check.

Thanks again for catching this. Indeed, 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.

Table 1. In the caption the authors refer to two meteorological stations but in the text only the Bisley tower is mentioned. Please check.

Yes, this is a typo, and it will be corrected.

Figures 7 and 8. Enlarge the contour labels.

The quality of the figures attached to this version of the manuscript is quite lower than the resolution in the original figures. We believe that everything will be clearer in the final version.

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