

## AUTHORS RESPONSE TO REFEREES

**TITLE:** Estimation of debris flow critical rainfall thresholds by a physically-based model

**AUTHORS:** M. Papa V. Medina F. Ciervo and A. Bateman

Please find enclosed the response to all comments made by the two Referees to the manuscript “Estimation of debris flow critical rainfall thresholds by a physically-based model”.

Thanks to the comments of the two referees we became aware that some points were not clearly explained in the paper. We have made the revisions suggested by the two Referees and we wrote in clarification about some specific points. We are convinced that this resulted in a great improvement of our paper and we are sincerely grateful to the referees for giving us this opportunity.

In the following we reply, one by one, to the comments of the two referees. The comments of the Referees are numbered 1 to 28 and formatted in italic. Following each comment, R indicates the response of the authors.

### Referee #1

#### *1. Evaluation*

*This paper presents an interesting methodological framework in order to assess rainfall induced debris flow hazard with CRT methods. Nevertheless, there are serious problems concerning several points suggesting accepting the paper but with major revision (seeing comments below). The major problems concern: (1) the type of flow-like phenomena the authors are dealing with (see comments below), (2) the presentation of data, (3) the interpretation of some results and their link with other references. 1.Does the paper address relevant scientific questions within the scope of HESS? YES 2.Does the paper present novel concepts, ideas, tools, or data? NO 3.Are substantial conclusions reached? NO 4.Are the scientific methods and assumptions valid and clearly outlined? YES 5.Are the results sufficient to support the interpretations and conclusions? NO 6.Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? YES 7.Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES 8.Does the title clearly reflect the contents of the paper? NO 9.Does the abstract provide a concise and complete summary? YES 10.Is the overall presentation well structured and clear? YES 11.Is the language fluent and precise? NO 12.Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES 13.Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? YES (see comments) 14.Are the number and quality of references appropriate? NO (see comments) 15.Is the amount and quality of supplementary material appropriate? YES*

**R:** a documented description of the flow-like phenomena addressed by the paper has been added in the paper and commented below (see reply to comment #4). The presentation and interpretation of data has been deepened and clarified (see reply comment #10 and #11). Specific references have been added as suggested.

We are convinced that that the paper presents a novel tool that may be used with advantage in early warning systems.

In our opinion, the results obtained are sufficient to support the conclusion that the proposed tool is adequate for the set purpose.

We think that the title of the paper reflects the contents of the paper, even if the proposed method is addressed only to one of many triggering mechanism of debris flow. With reference to the classification proposed by Hungr et al. (2001) our approach is suited for debris flows and mud flows triggered by debris avalanches or shallow landslides. Please refer to the reply to comment #4 for in-detail discussion of this point.

*2. Introduction. Authors should first indicate the main debrisflow triggering mechanisms (e.g. rainfall, earthquake, rapid snow/ice melting, etc.) in the Introduction and then make a focus (with key references) on landslides induced debris flows. Some key references about debris flows triggered from landslides are for example: Baum and Godt (2010), (Landslides) Crosta and Frattini, (2008), (Hydrological Processes) Hungr et al. (2001), (Environmental and Engineering Geoscience)*

*Malet et al. (2005), (Geomorphology) A key reference about rainfall induced debris flows and landslides (with specific emphasis on CRT curves): Guzzetti et al. (2008), (Landslides)*

**R:** We agree that this part was missing in the introduction of the paper. We added a wider introduction to the debris flow triggering mechanisms and to the landslide triggered debris flows.

*3. 'The conoid of mountain torrents: : :'. Please replace conoid with fan, which is the correct term in a geomorphological point of view;*

**R:** Thank you, we amended the paper following your suggestion

*4. This study is limited in a single kind of a phenomenon which is not representative of the most part of debris-flow events. For instance, scouring and entrainment are responsible of 50 to 90% of the volume of material involved in the debris-flow. To my point of view, the authors are dealing with shallow landslides or mud-flow triggering, not debris-flow triggering. If they want to clarify the term, they should provide information about the rheology of the flow, the grain size distribution, the morphology of the deposits, etc.*

**R:** From the authors point of view the proposed method may be employed to estimate rainfall critical threshold for debris flows and mud flows triggered by debris avalanches or shallow landslides. Our purpose was to reproduce the threshold curves usually obtained by empirical methods (for example by Guzzetti et al. 2008), but using physical models. Rainfall threshold curves are often at the base of early warning systems, but when the historical database of paste events is not wide enough, the derivation of the curves is problematic.

We are not trying to model DF events, we try just to capture the triggering. As shallow landslide and debris avalanche are typical initiation mechanisms of DF, we think that if we identify rainfalls critical for those events we are providing a useful tool for DF early warning systems. Of course other tools must be identified for the warning of DF triggered by different mechanisms.

Many studies demonstrated the importance of the amplification of the debris volume due to sediment entrainment from the bed and bank of the channel (Iverson et al., 1997; Takahashi, 2009, Papa et Al., 2004, Quan Luna et al. 2012 and many others). This aspect is crucial for a correct debris flow hazard mapping. Nevertheless, in the present study we focused our attention on the link between the rainfall characteristics and the possible triggering of debris flows, and we didn't simulate the flow propagation and deposition. In practical cases, a simulation of the propagation, amplification and deposition of possible debris flow, having different initial volumes, must be done in order to associate the rainfall critical thresholds with the consequent extension of the area at risk. Our study constitute the first step: link between rainfall and initial volume. A second step is needed:

the link between initial volume and hazard assessment. The entrainment process must be taken into account in this second step.

The study case reported in the paper is a flow like landslide occurred in a small catchment (Sambuco catchment) of the Amalfi Coast, Campania Region, in the of south Italy. The deposits covering many of the slopes of Campanian Appennine, and particularly of the studied area, derives from the eruptive activity of the Somma-Vesuvius volcano (eruptions immediately before, and after the 79 A D, until the last one in 1944). These deposits are constituted by ashes, pumice and scoriae. The weathering and the pedogenetic processes of such deposits have produced soils with peculiar characteristics, defined “andic properties”.

The grain size distribution has been investigated and the resulting granulometric curves are reported in the figure below. It may be noted that the content of silt and finer particles is about 20%. Rheological tests (Martino & Papa, 2008, Martino, 2003), were carried out on samples taken in areas affected by debris flow phenomena in May 1998 in the town of Sarno. The mixtures involved in these events have the same main characteristics and genesis of the Amalfi Coast soils. As it is shown in the figure, the granulometry of the samples taken in Sarno is analogous to the ones taken in the Sambuco catchment. The results of the rheological tests performed on the samples collected in the Sarno area may be used in characterizing the debris flow mixtures of the Amalfi Coast basins, such as the Sambuco basin. The rheological tests showed a shear thickening behaviour, described in detail by a Herschel Bulkley equation (Coussot, 1996) with the exponent of strain rate being equal to 1.7. This means that the peculiar composition of the pyroclastic soils of Campania Region is responsible for a rheological behavior that is influenced by the presence of fine particles, giving rise to a threshold-type cohesive effort and a high viscosity, as well as by the presence of coarser sediments that give rise to the collisional behavior. With reference to plasticity properties, the samples taken in Sarno catchment are classified as non-plastic or slightly plastic soils (Bilotta et Al., 2005).

The event occurred in 1954 in Sambuco catchment, as well as many of the events occurred in Campanian Appennine, are initially shallow non-confined slides (or flow) along steep slopes and, downstream, evolve into an in-channel flow.

With regard to the classification proposed by Hungr (2001), the phenomenon under study can be classified as debris flow, considering the fine content and plasticity index of the mixture, even if the rheological tests show a combination of the resistance mechanisms of a debris flow together with the ones of a mud flow.

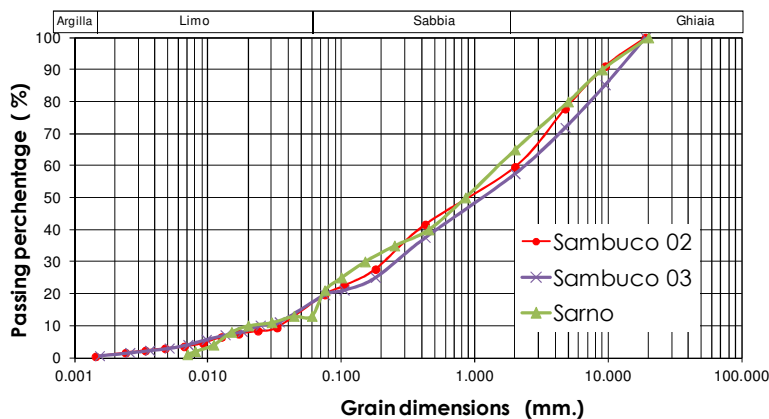


Figure: Granulometric curves

5. Moreover, they are not considering the sediment supply, which is of primary importance for debris-flows. They should specify at the start that this study is dealing with not limited sediment supply debris flows.

**R:** As we are addressing to debris or mud flows triggered by shallow landslides or debris avalanches, the model input variable taking into account the sediment availability is the soil thickness ( $Z_T$ ).

6. Paragraph : : . (and therefore rainfall intensity decreasing with rainfall durations). The sentence is not clear; in fact rainfall intensity is decreasing when the rainfall duration is increasing (for the same rainfall amount).

**R:** Sorry, this is a refuse. The sentence has been changed in “rainfall intensity decreasing with increasing rainfall durations”

7. Study case: Please indicate the minimal and the maximal elevation, which are much more useful than the mean one.

**R:** The catchment elevation are in-between 1000 m and the sea level. We added this information in the text.

8. Sometimes authors classify the phenomenon as a mud-flow, sometime as a debris flow, sometime as a shallow landslide. These is confusing, please clarify this by using the same term for each events, or if authors have clear field evidence (grain size distribution, morphology of deposits, etc.), they should first provide a classification of the flow like phenomena that are concerned by this study.

**R:** We added a classification of the flow like phenomena based on the grain size distribution and the rheological tests, as discussed above.

9 ‘The reconstruction of the areas that were mobilized (Papa et al., 2011a), showed that 2,8% of the total basin area was affected by the detachments’ : : : This is a crucial point. Authors should provide a quick synthesis of this paper (2-3 sentences) which exposes the methods (field investigation, multi-date DEM reconstruction with aerial photographs? Old geomorphologic maps? Other?) used for the definition of the volume of debris flow solid material. The 300’000 m3 is the solid part or the total volume of the debris flows (water+solid)?

**R:** The reconstruction of the tracks of the detachments occurred in 1954 was performed first georeferencing the aerial photographs and then checking and correcting the resulting map with field observations.

A map of soil thickness for the entire catchment (at scale 1 : 2000) has been drawn. This was obtained first by a detailed field investigation that produced a set of punctual observation of the soil thickness. The map was then extrapolated using the observed correspondences between surface geomorphology and the spatial variability of soils and deposits (Guida et al, 2007). The volume involved by the landslides was derived with GIS tools, overlaying the map of the detached area with the map of soil thickness. We added in the paper a description of this work.

10 ‘The event rainfall curve approaches the ID lines corresponding to simulated failure percentages of 0.3 %, for rainfall durations of about 8 h. This is in good agreement with the

*observed failure percentage (0.3 %).” : : : This is not good, this is perfect. In other terms your debris flow is only rainfall-dependant? No sediment supply dependant? They should also provide a statistical analysis of daily rainfall characteristics NOT associated to debris flow event. Did they observe in the data catalog, heavy/strong daily rainfalls where no flow-like phenomenon was observed/recorded?*

**R:** As mentioned above, in our model, the availability of sediment is taken into account by a detailed map of the soil thickness.

In the Fig.5 of the paper we reported the maximum rainfall with durations from 1 hour to 12 hours observed in the period 2002-2011. It is worth noting that the 2005 event didn't reach the downstream village of Minori. This means that for events with this order of magnitude there is not a severe hazard for the people. If, for example, we fix a rainfall threshold line in correspondence of a failure ratio (number of instable elements over the number of stable elements) equal to 0.6% there are 3 rainfall events (12/10/2006, 3/11/2009 and 9/9/2010) over the threshold in the 2002-2011 period. The heavy rainfall event of 2010 caused a devastating flash flood in the adjacent catchment of Atrani (Ciervo et al. 2012) and minor effects in the Sambuco catchment and Minori village. On the whole, in the period 2002-2011 we would have had two false positive (2006 and 2009), one true positive (2010) and one true negative (2005).

*11. “ After the position of antecedent rain equal to the observed value (212mm.month) the only antecedent rain, without any event rain, causes a failure percentage greater than the observed one (Papa et al., 2011b).” : : : This sentence is not clear or confusing. Moreover authors should precise what they mean with “greater”, no quantitative values?*

**R:** In our model, the water table depth at the beginning of the event rainfall is computed depending on the antecedent rainfall, using the Montgomery and Dietrich (1994) model. As the event of 1954 was in October, after the dry period, in that case the antecedent rainfall was null. In 2005 the antecedent rainfall was 212 mm/month. If we compute the stability condition with the Montgomery and Dietrich (1994) model we obtain a failure percentage of about 1.2%. This value is higher than the observed one (about 0.3 %). We amended the paper giving a more precise description of this point.

*12. Results and discussion. “When the historical database is not wide enough, or in the case of total absence of historical debris flow events, the failure threshold can be fixed by simulating the downstream effect of different debris flow volumes. These simulations can be performed through the mathematical and numerical modeling of debris flows propagation (O'Brien et al., 1993; Medina et al., 2008). By carrying out a large number of simulations with different input volumes (and consequently discharges), it is possible to asses a threshold for the total amount of debris volume that may comport an hazard for the downstream areas.” : : : This work has been performed by many authors with various type of models, for instance: Malet et al., 2005 (Geomorphology). Authors should add this reference which deals perfectly with the topic. This paper discuss also the type fo models associated to the type of flow-like phenomenon. Otherwise, the model proposed by O'Brien in 1993 (Quadratic model) did not include originally the scouring effects. A complete review of debris-flow models including scouring effects is available in Quan Luna et al. 2012 (Engineering Geology).*

**R:** A wider set of bibliographic reference has been added on this topic. Thank you for the suggestions.

*13 Figures: Figure 4: Authors should add some elevation spots (or elevation lines) on the geomorphological map.*

**R:** Elevation lines have been added in the maps.

## **Referee #2**

### *14. Evaluation*

*The objective of this work is to describe the derivation of threshold rainfall for landslide triggering based on an existing model, due to Iverson (2000), which describes shallow landslides occurrence under conditions of transient infiltration into initially unsaturated soils. The methodology is applied and tested over the 6.4 km<sup>2</sup>-wide Sambuco basin in southern Italy, where debris flows were triggered by two different historical events. The paper is well suited for HESS and it may attract the attention of hydro-geomorphologists interested in shallow landslides and debris flows. However, there are problems with both the scientific content of the work and with its presentation. The main science problem is related to the pervasive confusion made between debris flows and shallow landslides. Moreover, it is difficult to see the innovation point in this work, given that the methods are not original and the main conclusions neither. So the authors must (thoroughly) modify the manuscript to focus on what is innovative. Given the time needed to perform such changes, I recommend major revisions. The revised version should be carefully checked by someone for whom English is the native tongue and who is also confident with the topic.*

**R:** Thanks to the comments of the two referees we became aware that in our paper the phenomenon under study was not identified clearly enough. The introduction paragraph of the paper was amended in order to clarify this point. As we already wrote in the replies to referee #1 we think that “the proposed method may be employed to estimate rainfall critical threshold for debris flows and mud flows triggered by debris avalanches or shallow landslides. Our purpose was to reproduce the threshold curves usually obtained by empirical methods (for example by Guzzetti et al. 2008), using physical models. Rainfall threshold curves are often at the base of early warning systems, but when the historical database of past events is not wide enough, the derivation of the curves is problematic. We are not trying to model DF events, we try just to capture the triggering. As shallow landslide and debris avalanche are typical initiation mechanisms of DF, we think that if we identify rainfalls critical for those events we are providing a useful tool for DF early warning systems. Of course other tools must be identified for the warning of DF triggered by different mechanisms”

We think that there are some original and interesting features in our work: one key point is the identification and quantification of the minimum rainfall duration over which the analytical solution proposed by Iverson (2000) can be used (see paragraph 2.2 of the paper); another point is the development of a methodology and a numerical tool for the computation of the rainfall critical thresholds. Even if we employed mathematical models that were well known in literature (Iverson 2000, Montgomery and Dietrich, 1994) the way we use them, the overall methodology and the numerical tool are original and, in author’s opinion, useful for practical applications. We revised the paper underlining the innovative points of the work.

*15 General comments. I found the title misleading: it deals with “debris flow critical rainfall thresholds” and it turns in the paper that the main physical process considered is shallow landsliding. The authors should make clear that, even when focusing on debris flows mobilized by*

*landslides, not all failing hillslopes mobilize to form debris flows. I think the title should reflect more accurately the content of the paper.*

**R:** With reference to the classification proposed by Hungr et al. (2001) our approach is suited for debris flows and mud flows triggered by debris avalanches or shallow landslides. We simulate the possible instability of each land pixel (resolution 5 x 5 m) and then we sum up all the instable elements in the catchment drained by the river in which debris flows are expected to occur. To make predictions of locations and dimensions of specific landslides is not the aim of the work. We know this is challenging. For example, the comparison of the unstable elements predicted by TRIGRS with observed landslides resulted in a great number of false positives and false negatives (Baum et al. 2009). Our task is a bit easier. As we wrote in the paper, we move from the consideration that, “with the aim of giving debris flow warnings, it is not necessary to know the distribution of instable elements along the basin but only if a debris flow may affect the vulnerable areas in the valley. The capability to reach the downstream areas depends on many factors linked with the topography, the solid concentration, the rheological properties of the debris mixture, the flow discharge and the occurrence of liquefaction of the sliding mass. Many of these factors do not depend on rainfall. The most rainfall dependent factors are flow discharge and correlated total debris volume. In our study, the total volume that is instable, and therefore available for the flow, is considered to be the governing factor from which it is possible to assess whether a debris flow will affect the downstream areas or not.” These are the reasons why we referred to debris flows, and not landslides, in the title.

*16. The confounding overlapping between shallow landsliding and debris flows is not limited to the title and is widespread in the work. Perhaps the section which suffers most from this confusion is the one dedicated to the validation of the methodology. The map of ‘traces of the landslides’ (whatever this means) (Fig 3), which is used as a reference for the modeled landslide map (not reported, the authors examined only a percentage of the topographic elements in the catchment), does not show typical shallow landslides that are a result of slope water table. Fig. 3 reports the runout paths of the debris flows triggered by two storm events. The debris flow runout and deposition are clearly different processes than the one described by the Iverson’s model used in this work. Of course, this leads to significant differences between the ‘observed’ and ‘modeled’ failing hillslope area and undermines the whole verification of the model.*

**R:** The Fig.3 does not report the runout paths of the debris flow but only the formation areas. Please refer to the Fig.3/mod reported below. In 1954 the flow propagated in the main valley (Sambuco River) and flooded the village of Minori and the beach. For a complete reconstruction of the event please refer to Papa et al (2011, DFHM Padua). The reconstruction of the tracks of the detachments occurred in 1954 was performed first geo-referencing the aerial photographs and then checking and correcting the resulting map with field observations. The detachment areas mapped may include also a portion of the propagation path in which entrainment of sediment material occurred. This may happen because sometimes it is difficult to precisely identify where the propagation phase begins.

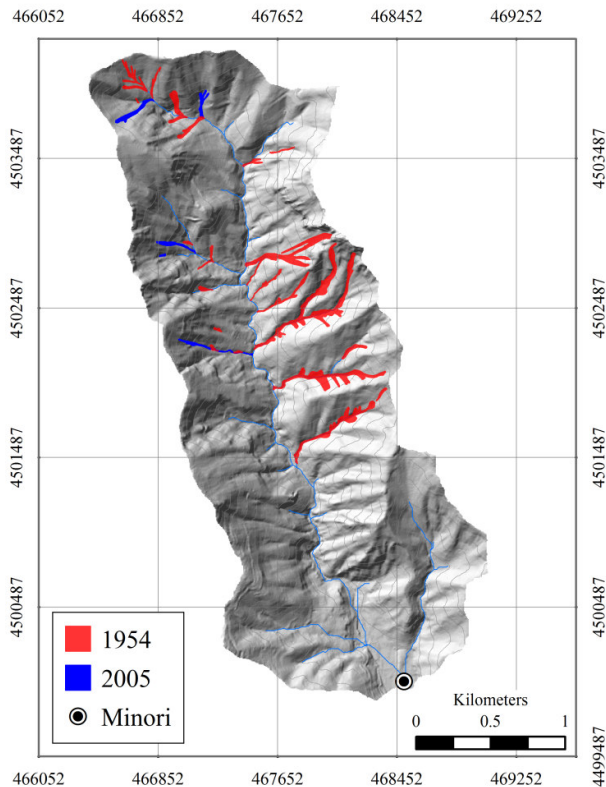


Fig.3 mod: Map of the studied basin, with the traces of the landslides occurred in 1954 and 2005 events.

*17 The paper fails to give proper credit to related work. Among the several papers which are highly relevant to this work and which are not cited are: Frattini et al., 2009, which develops probabilistic thresholds for triggering shallow landslides by rainfall using the Iverson's model (the work reports an application to a 180 km<sup>2</sup> area in northern Italy); Baum et al. (2010) which develops a model of the infiltration process using a two- $\alpha$ - $\beta$  layer system that consists of an unsaturated zone above a saturated zone and implemented this model in a geographic information system (GIS) framework; Lanni et al. (2012) which combines a model for the initial unsaturated soil conditions through the whole soil profile and a groundwater model.*

**R:** The suggested references have been added into the paper.

*18. Model implementation. To save computation time, the hillslope instability model is applied to a number  $n$  of the topographic elements of the basin equal to 1%,. The authors reports that for  $n$  exceeding 1% of the total basin cells, the simulation results converge to the one obtained simulating all the basin cells. However, the authors fail to provide any indication on how to select the 1% of the basin, given that all the various classes of both the soil/geotechnical parameters and of the morphological (local slope, upslope area, convergence/divergence) parameters should be equally represented.*



**R:** Probably the detailed explanation of the model implementation requires an independent paper, so a brief description was included in the original text. The next lines are provided in the attempt of clarifying the reviewer doubts. As it is explained along the text, there are 12 different variables involved in the model, namely: "...morphological features ( $A/b, Z_T, \alpha$ ) and the soil parameters ( $c, \varphi, \gamma_s, K_x, K_Z, D_0$ ). These are considered as stationary at the process scale. The dynamic variables are the rainfall related variables ( $(I_z)_{steady}, I_Z, T$ )...". Most of the properties ( $c, \varphi, \gamma_s, K_x, K_Z, D_0, Z_T$ ) are considered to be "stochastic", it means that instead of defining a value, a range and a pdf distribution is provided. This hypothesis is based on the uncertainty in the evaluation of these parameters. For every cell the model selects randomly a properties sample, fitting the user provided pdf. The size of the sample is 10 for every parameter. The geomorphologic properties ( $A/b, \alpha$ ) are considered "deterministic" because they depend on the topographical LIDAR, and a low error is assumed on this data. The testing mesh size in the dynamic variables space ( $(I_z)_{steady}, I_Z, T$ ) is  $10 \times 50 \times 50$ . The basin DTM (Digital Terrain Model) contains more than 6.000.000 cells. To evaluate the stability of the soil 10 points are selected along the soil column. As a consequence of the previous values, the total amount of cases to evaluate for this basin was:  
 $10 \times 6.000.000 \times 10 \times 50 \times 50 \times 10^7 = 10^{18}$

Therefore the complexity of the model makes impossible to use it as a "deterministic model", so also the application of the model to the DTM cells was also performed in a "stochastic" mode, and a sample of the topography is selected randomly.

Previously, in order to check the performance of the model, a small basin was selected and a sensibility analysis was carried out. One conclusion of the analysis was that beyond the inclusion of 1% of the cells in the sample the results converge faster to the "100% of the basin case".

Another important computational cost reduction comes from the fact that once a cell becomes unstable is not necessary to compute the rest of the rainfall event, more duration will trigger instability and more intensity as well.

*19 Epistemic uncertainty. The authors report that the uncertainties in the evaluation of the soil variables are taken into account assigning to each variable an average value along with a confidence interval. However, the results are reported without any reference to the said uncertainties.*

**R:** The possibility of assigning a PDF to the soil variables is included in the numerical code but is not used in the applications presented in the paper. We amended the paper in order to asses clearly this point.

*20 Details Abstract: I don't understand what the 'virtual' basin mentioned here does mean. Which is the difference between the Sambuco basin and the virtual one? Moreover, it appears here a 'studied basin' which was not mentioned earlier.*

**R:** the virtual basin is the one made up of  $n$  computational elements, being  $n$  a number lower than the total basin elements but high enough to provide similar results. The abstract has been corrected and the "virtual basin" and the "studied basin" defined properly.

21. P7 and P9: Both equations at P7 and P9 are numbered as (9). P9L21 Pyroclastic P10L8-10:

**R:** The numbers of equations have been checked

22. P10L9-11: A rather crude method is introduced to account for root cohesion: the value of the soil cohesion is multiplied by 1,25. The authors should recognize the limitations and the implications of this crude method.

**R:** The 25% of increase in soil cohesion was set after calibration. This is the only calibration parameter of the model. This is not properly a method to take into account the root cohesion. We only say that there are some possible explanations for the fact that a higher value of the soil cohesion is needed in order to reproduce the observed failure percentage. One of these explanations is the presence of root cohesion.

23. P11L8-11: 'In the studied example, as the 2005 event (failure percentage = 0.3%) did not reach Minori the critical failure percentage should be fixed between 0.3% and 3%.' This is an example of the effects triggered by confounding debris flows and landslides. The observation that the debris flows didn't impacted the downstream town should not have implications on the extension of the failing area (which should be observed per se). This may well depend on the characteristics of the triggering storm, or on processes (such as liquefaction) which are not included in the model.

**R:** Of course the observation that the debris flows didn't impact the downstream town does not have implications on the extension of the failing area. The failing area has been observed per se and was estimated to be 0.3% of the total basin area. What we are discussing here is whether this small instable area is able to trigger a debris flow that may reach the downstream village. As a matter of fact in that event the observed landslide didn't give place to a debris flow and the downstream village was not affected. We assume that there is a critical instable volume; instabilities involving volumes below this critical value are expected to not give place to flows and therefore are not hazardous for the vulnerable area that are a few kilometers downstream.

24. P12L25-27: the text here should be reworded since it is very hard to understand

**R:** ok

25. Figures A figure should be added reporting the main morphological characteristics of the basin, the observed landslides (not debris flows) and the town of Minori (mentioned twice in the paper and unrecognized in the maps).

**R:** The Fig. 3 has been changed, please refer to Fig.3/mod in the above section

26. Figure 1: Please check the variable reported on the y axis. It is wrong.

**R:** On the y axis of this graph we report the computed pressure heads at the bottom of the soil layer. We corrected the figure caption and the y axis label.

27. Figure 2: Which is the meaning of the other basins identified in red?

**R:** The basins identified in red are the basins neighboring the studied one. As this result to generate confusion we canceled the boundaries of the other basins leaving only the watershed of Sambuco.

28. *Figure 3: A graphical scale should be added.*

**R:** As the scale can be derived from the geographical coordinates, that are in the map, at first we decided to don't add the scale. But we recognize that reading the scale is perhaps more immediate and therefore we added it in all the maps.