Answer to Reviewer #1, prof. T. van Asch

First of all, even on behalf of my co-authors, I would like to thank prof. van Asch for his helpful comments, which will surely lead to a substantial improvement of the quality of the manuscript. Indeed a revised version will be soon submitted, taking into account his comments as well as those made by the other Reviewer.
In particular, we are grateful to prof. van Asch for suggesting us to complete our study with the evaluation of the equilibrium conditions with reference to the attained suction values and to the inclinations at the slope.
Below are our point-wise replies about the various issues raised by prof. van Asch (reproduced in italics for clarity).

Sincerely,

Roberto Greco

I miss here some detailed frames (for different seasons) where I can see more in detail the temporal development of the moisture and pressure profile curves to see whether there is an effect of the contrast between the different layers especially the ash and the pumice layers. That brings me to the first discussion point : a fast increase in moisture content even until nearly saturation can happen in a finer ash layer above a coarse pumice layer, before infiltration in the coarse layer can take place. This may have a great influence on the triggering time of the landslide and the depth of the slip plane. This well known effect cannot be modelled in the single layer model presented in this paper.

The data from the monitoring campaign point out that the presence of layers with different characteristics within the soil profile does not affect so much the infiltration process (and that is why we expect that a simplified single-layer model could be able to satisfactorily reproduce the overall behaviour of the soil cover): there is no evidence of a “capillary barrier” effect, as indicated by the measured suction and water content at the various depths (figure 3 and figure 7). In fact, during the rainy period (from January to April), the deeper the tensiometer, the more delayed and smoothened its response to rainfall events is. Soil suction observed at the uppermost tensiometer, suddenly decreasing during and immediately after the rainfall, always quickly returns to be the highest along the profile (usually it takes 24 to 48 hours, even after the most intense rainfall events), indicating that there is no obstacle to the downward propagation of the infiltration process. Such feeling is confirmed by the measured water content, which never attains high values at the shallower investigated depths.
The absence of an apparent “capillary barrier” effect may be ascribed to the fact that soil layers rich with pumices (indicated as A and C in figure 1) are not always found along the slope (e.g., as indicated in figure 2, the upper layer A with coarse pumices was found only in one of the two locations where the sensors had been installed, which are five meters apart from each other). In layer C, which has not been found in the location of the monitoring station (figure 2), fine pumices are mixed with ashes (Damiano et al., 2012), and thus the difference in unsaturated hydraulic conductivity compared with the ashy layer is not so pronounced. Moreover, such layer has not always been found in the holes and trenches dug at various locations along the slope. In order to make it clearer, the revised manuscript will be modified in the initial part of section 2 as well as in the caption of figure 1.
By the way, owing to the variation of several orders of magnitude of the hydraulic conductivity with the degree of saturation, in the ashes as well as in the pumices, the conditions for the actual formation of a capillary barrier (the ashes and the underlying pumices at saturation degrees such that the latter have an unsaturated conductivity much smaller than the ashes; the presence of a high

infiltration flux from the soil surface such that the upper ashy layer becomes wet, without the establishment, at the interface between ashes and pumices, of a water potential gradient large enough to let part of the water infiltrate through the pumices) are not so easy to occur, and the concept itself of capillary barrier is a very simplified picture of a much more complex phenomenon.
To confirm that the hydrological behaviour of the slope of Cervinara is different from other cases of pyroclastic covers in Campania where debris flows occurred (e.g., the case of Pizzo d'Alvano, near Sarno, in May 1988), it has been reported that the huge flowslide of Cervinara between 15th and 16th of December 1999 (cited at lines 78 to 81 of page 4) affected the entire soil cover, leaving the calcareous bedrock bare or covered only by the deepest layer of altered ashes (Damiano et al., 2012). Furthermore, again differently from the case of Sarno, the rainfall event which triggered the landslides was really extremely rare: more than 320mm fell during 44 hours (roughly twice as much as in the case of Sarno), with around 260mm during the 24 hours before the landslide.
However, we understand from the comment of prof. van Asch that it would be worth to better discuss the observed behaviour considering the layered profile, and to highlight the difference from what has been observed by other Authors in (apparently) similar layered covers. The revised manuscript will thus be changed accordingly.
About the request of adding detailed seasonal frames, they already exist (figure 7a and 7b), and, in our opinion, adding further frames (e.g. with the water content values) would increase the length of the paper without adding significant information.

I like the idea of the simplification to a one layer model, But I would like to have seen more in the discussion about the disadvantages of this simplification: The calibrated effective parameters are based on one single profile with an alternation of ashes and pumice layers and even not a complete profile (see my detailed discussion points) . The large spatial variability in profile building was one of the arguments to construct a one layer model to get a general picture of the temporal water balance. But how representative is the calibrated model for a whole slope or even catchment? I can imagine that due to this variation the calibrated effective parameters will also vary quite intense? from place to place. (…) *Figure 1 and 2 : I do not see a relation between Figure 1 and 2 I see that layer C and D are not instrumented. They are quite different from layer A and B ? Measurements in these layer could have influenced the calibration process? I miss also a table which describe some characteristics of the layers depicted in Figure 1.*

As already pointed out above, the monitoring data seem to confirm the possibility of adopting a simplified single layer model: in fact, looking carefully at figure 5, the water retention data measured at all depths above -1.75m are quite similar to each other (with some dispersion, of course), while only at the greatest depth the behaviour is much more like a fine-grained soil: this was expected, since a layer of altered ashes, with a significant clay fraction, is often found just above the soil-bedrock interface (Damiano et al., 2012). We agree with prof. van Asch: figure 2 is too schematic and wrong, and it will be redrawn: the deepest tensiometer is indeed in the altered ash layer D, which is missing by mistake in figure 2. About layer C, instead, the schematic profiles of figure 2 are right: such layer (which usually presents fine pumices within a matrix of ashes similar to those of layer B) has not been clearly identified at the location of the monitoring station. The new version of figure 2 will include the indication of the layers consistently with the letters used in figure 1. We will also add a table with some physical characteristics of the soil belonging to the different layers.
The main disadvantage coming from the "effective" single layer model, that has not been properly commented in the manuscript (we agree with prof. van Asch), is that such single layer may not reproduce well the behaviour of the soil profile during the extreme event of 1999: prof. van Asch correctly points out that the attained minimum suction values around 1 kPa correspond to a maximum water content slightly larger than 0.4. We believe that different local conditions somewhere else along the slope could explain the triggering of a landslide, but surely the evolution into a debris flow would not have been predictable with our model. The reason for this, besides the intrinsic limits of a single-layer model, could possibly be ascribed also to the calibration carried out with data form a period without extreme rainfall events, thus without information neither about the

behaviour of the bottom fine-grained layer during intense infiltration flux, nor of the seasonal aquifer conceptualized as bottom boundary condition. Following the suggestion of prof, van Asch, we will add more discussion about this limits in section 5 and 6 of the revised manuscript.
About the spatial variability of the soil characteristics when moving from a single location to the entire slope or even to the catchment, we believe that this kind of problem would exist also if a more complex multi-layer model was adopted: the layer sequence would not be everywhere the same and, maybe, the adoption of effective parameters that somehow mediate the hydraulic behaviour of the soils of the various layers could result in a smoother variability of characteristics. However, for future more in-depth analysis of this aspect, we have planned to install another monitoring station at another location along the slope.

For me the role of vegetation in triggering these landslides on these steep slopes is still not clear. In the simulation of 1999 it was the amount of rain in spring and early summer which resulted in a relative moist profile after the summer and probably had a positive effect on the wetting of the profile after the dramatic rainfall of December 14 and 15. But how large was this effect given the huge amount of rain which felt during these two days. And what could have been the role of evapotranspiration (vegetation) during the summer. I tend to minimize the role of the initial moisture content of the triggering of these type of shallow landslides (turning into debris flows) on very steep slopes and to maximise the role of the amount and intensity of the triggering rain event. The 1999 scenario did not change my vision. The role of vegetation especially for these shallow landslides triggered by intensive rainstorms on steep slopes remains to be debated. How important is this initial moisture content.

We agree with prof. van Asch: the role of the vegetation in the triggering of the event of December 1999 is probably negligible (indeed, our model assumes via the relevant seasonal parameters that such effect was minimum during that season) As described in section 2, the introduction of the vegetation effect into the model has been suggested by the monitoring data observed during the few rainfall events occurring in late spring/early summer.
Similarly, also the relatively high initial water content could have not been so important in the triggering.
In fact, the point we would like to stress (but now we understand that it has not been clearly presented in the discussion) is that we believe that in some cases, more than soil moisture conditions, the bottom boundary condition may constitute the main obstacle to the leakage of large infiltration rates towards the fractured bedrock.
Indeed, everything seems to indicate that, during most of the observed rainfall events, the hydraulic properties of the soil cover are such that even very large infiltration rates can pass through the soil cover without increasing that much its water content, nor causing a significant decrease of soil suction.
Figure 3 and 7a clearly indicate that during winter the fluctuations of suction caused by rainfall infiltration tend to smoothen with depth: at the depth of -1.75m soil suction is always between 4 and 6 kPa, and we expect that the fluctuations would be even smaller at greater depth. This behaviour means that, whatever the amount of the infiltration rate is, it is able to cross the interface with the bedrock and leak towards the underlying aquifer (a sort of free outflow condition). This is confirmed by the high value of “effective” saturated hydraulic conductivity (nearly 300mm/h) suggested by calibration.
The completely different situation observed during summer (figure 7b: the deepest tensiometers show the highest suction increments) suggested us that at the soil-bedrock interface a more complex hydraulic constrain than a free outflow should have been introduced. And this led us to introduce the conceptual model of a seasonal aquifer in hydraulic contact with the overlying soil cover.
The remark about the “capillarity gradient” which “tended to impede the downward-directed gravity driven flow” (page 6, line 138) aimed to stress that not only the hydraulic conductivity of the soil, but also the hydraulic condition at the bottom, determines how difficult is the passage of a given infiltration rate, by affecting the establishment of the needed water potential gradient.

An unusually rainy summer followed by the usually rainy autumn (as it happened in 1999) may have been created the conditions (an unusually high water level in the aquifer contained in the fractured bedrock and, thus, an unusually small suction at the bottom of the soil cover) which, given the extremely high rainfall intensity of 15th and 16th December 1999, could have favoured an unusual increase of soil water content just above the soil-bedrock interface.
Since no data are available about the actual water level in the aquifer, there is no chance of validating the above mentioned hypothesis. Moreover, the adopted linear reservoir model is only a conceptual scheme to account for the fluctuation of water potential at the bottom of the soil cover and, as clearly stated at page 14 lines 338-341, such model "does not mean to reproduce the actual water table fluctuations".
We will modify the comment to figure 4 in section 2 to be more clear about the importance we attribute to the moisture conditions at the bottom, which we suppose related to water table fluctuations inside the underlying fractured bedrock. Also the relevant parts of section 5 (the end of page 14 and the first lines of page 15) and of section 6 (lines 397-400 at page 17) will be modified to stress the role of the bottom boundary condition.

The coupling of the hydrological conditions with the conditions of failure and the development of shallow slides which might develop into flows is vague. It would have been nice to explore with a simple equilibrium model at which slope gradient or slope configuration and until which possible depth instability can occur given the outcome of the hydrological model and to compare these results with the topographical characteristics of the slopes which failed nearby the measuring plot. (…) *It is also nice to give an idea of the water content here. According to Figure 5, it must be around 0.4 which is far from saturated? I wander if this conditions can create mudflows with long run-out distances*

We wish to thank prof. van Asch for the suggestion: in the revised manuscript we will add the evaluation of the safety factors at various depths with the limit equilibrium equation under the hypothesis of infinite slope, and we will discuss the obtained results, also in terms of aoil water content, with reference to the morphological characteristics of the failed slope.

5803/07 Must we not know the root density for the model?

The adopted model of root water uptake does not need to specify a root density. Anyway, at page 4, lines 89-90, some qualitative information about root density is provided. So far, no quantitative evaluation of this parameter is available.

5804/1-5 A bit more information about this calibration. How good was the correlation?

As solicited also by Reviewer #2, in the revised manuscript we will add more information about the calibration procedure, namely about the value assumed by the objective function defined at page 11, lines 263-264.

5804/19-23 According to me the higher losses of water in the top soil points to the influence of evapotranspiration on the water balance in the topsoil?

We agree with prof. van Asch. The comment to figure 3 is misleading and we will modify it in the revised manuscript. It will read more or less (in **bold** the envisaged changes): "the soil suction increment does not seem to be caused **only** by upward evaporation fluxes (…) the two deepest tensiometers (…) **while** the two upper TDR probes (…)

5805/5-12 I see: during the july month : -gradient upper soil going up and then down (effect of dropping groundwater and then effect of evapotranspiration) -gradient middle soil strongly going up >1 (effect of dropping groundwater) -gradient lower soil going up and down but not above 1 so not so clear effect of a dropping groundwater ??

We will better clarify the interpretation of the estimated water potential gradients in the revised manuscript. The point here is that at the bottom of the cover the soil (layer D) has different hydraulic behaviour, more like a fine-textured soil (see figure 5). Consequently, during summer, when the suction is between 20 and 35 kPa (figure 3), the water content is still above 0.3, while in the upper layer it dropped below 0.2. This large difference in water content can cause the unsaturated hydraulic conductivity to be much larger at the bottom than above -1.40m depth. So, the infiltrating flux is associated to a smaller vertical potential gradient.

5805/14. I would say already during May

It is true: we will modify it in the revised manuscript.

5805/20 What is understory? Did the rainfall gauge measure the effect of canopy interception . The rainfall graph did not show high daily rainfall in May and the months after.

Thank you for catching this typo. We will change it to undergrowth or underbrush.
The rain gauge is located in a clearing within the woods, so it is not affected by canopy interception. The few rainfall from May on is typical of the precipitation regime in Southern Italy.

5806/1 Indicate behind the soil depths in Figure 5 the soil types. Also in Figure 7

It will be made in the revised manuscript.

5813/16 What are the soil types in Figure 6 ? Are there no large differences between ashes and pumice even under unsaturated conditions?

See our answer to the first point. We will add in the caption of figure 6 that it refers to ashes (layer B of figure 1).

5814/9 Flow slide? What do you mean a shallow slide which developed into a flow with a long run-out distance

Yes, it is exactly so. We will clarify it in the modified manuscript.

5814/27 More elegant to use the same units in figure 9 and in the text.

Thank you for catching this. We will use consistent units throughout the entire revised manuscript.

Technical 5804/13-16 Complex sentence Rewrite in pieces Some graphs are in colour, (Fig 3, 7,8,9,) which must be adapted for a black and white printing process using different signatures in stead of colours. I hope you can find a way to keep the figures readable given the huge amount of point signatures.

We will break the sentence into smaller ones.
Unfortunately we don’t think it is possible to obtain clear graphs without using colours to distinguish different data series. However, as HESSD is an open-access on line journal, everybody can display the graphs on a colour monitor while reading a black and white printed hardcopy of the paper.

References

E. Damiano, L. Olivares, L. Picarelli 2012. Steep-slope monitoring in unsaturated pyroclastic soils. Engineering Geology, 137-138: 1-12.