Answer to anonymous Reviewer #2

First of all, even on behalf of my co-authors, I would like to thank the anonymous Reviewer #2 for her/his 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 her/his comments as well as those made by the other Reviewer.
In particular, we are grateful to Reviewer #2 for stimulating us to complete our literature review by indicating some recent papers, about which he was much more aware than we were (Mancarella and Simeone, 2012; Mancarella et al., 2012; Cascini et al., 2013; Sorbino and Nicotera, 2013).
Below are our replies about the points raised by Reviewer #2 (reproduced in italics for clarity).

Sincerely,

Roberto Greco

(…) *in spite of the fact the soils are distinctly layered, as shown in Fig 1, and the hydraulic behaviours of the layers are significantly different (Fig. 5), the model considers the material as a single homogeneous layer. It is a strong inconsistency and the model results too simplified. Furthermore the complexity of the hydraulic behaviour of multilayer media for the activation of landslides in pyroclastic blankets was evidenced by several authors who recently studied landslides activation in pyroclastic soils (cf. scopus data-base).* (…) *It seems appropriate to report a more detailed analysis of the experimental data, analysing the obtained results also considering the nature of the multilayer sequence. All that in comparison to the numerous articles about the area and pyroclastic deposits (ex. cf. Scopus).*

As indicated by the measured suction and water content at the various depths (figure 3 and figure 7), there is no evidence that the presence of layers with different characteristics within the soil profile 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). In fact, during the rainy period (from January to April), the deeper is the tensiometer, the more delayed and smoothened is its response to rainfall events. 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, sometimes observed in layered pyroclastic deposits and related to the abrupt reduction of hydraulic conductivity moving downward along the layered profile (Mancarella and Simeone, 2012; Mancarella et al., 2012), may be ascribed to the peculiar stratigraphy (similar to that of the nearby slope of Monteforte Irpino, thoroughly described in Sorbino and Nicotera, 2013): the soil layers rich with pumices (indicated as A and C in figure 1) are not always found along the slope (e.g., as shown 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, fine pumices are mixed with ashes (Damiano et al., 2012; see also the grain size distribution of layers 5 and 7 given in figure 5d in Sorbino and Nicotera, 2013), and thus the difference in unsaturated hydraulic conductivity compared with the ashy layer is not thought to be so pronounced. Moreover, such layer has not always been found in the holes and trenches dug at various locations along the slope.
However, in order to better clarify the characteristics of the layered profile at the slope of Cervinara, the initial part of section 2 as well as the caption of figure 1 will be modified in the revised manuscript.

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; 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 a significant part of the water infiltrate through the pumices) are not so easy to occur, and we believe that 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, where some Authors (Mancarella and Simeone, 2012) deduce, from the limited depth of the slip surface, that, owing to the "capillary barrier" effect caused by the presence of coarse-textured pumiceous layers, a perched water table formed during the long-lasting, but not extremely intense, rainfall event which triggered the landslides), 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 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 agree with Reviewer #2 (and also with a similar remark made by the other Reviewer), 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.

In particular, it would be necessary to describe the monitored site by defining the characteristics in terms of lithological characteristics and parameterize the rock types in terms of geotechnical, hydrogeological and mineral-petrographic properties.

Also to answer to a remark made by the other Reviewer, the analysis of the equilibrium of the slope in terms of safety factor will be added to the revised manuscript, in order to better discuss the results of the application of the model to the 1999 event. The geotechnical properties of the soils will be thus given in such newly added part.
About lithological and/or mineral-petrographic characteristics of the soil cover, the discussion of such data is beyond the scope of this study, strictly focused on the mathematical modelling of the infiltration process, which is only indirectly related to such information.
About the hydrogeological properties of the rock types, it seems the Reviewer refers to the underlying fractured and karstified calcareous bedrock and to the aquifer which is often present in such formations. The discussion of such properties is also beyond the scope of the study: in fact, there is a conceptual modelling of the fluctuations of the water level in such aquifer, but it is explicitly stated that "the linear reservoir model of the underlying aquifer, introduced as bottom boundary condition at the interface between soil cover and bedrock, does not mean to reproduce the actual water table fluctuations, about which no experimental information is available, but rather their effects upon the water potential at the bottom" of the soil cover (page 14, lines 338-341).

A clear illustration of the position of the sensors compared to the stratigraphic and morphological setting of the test site is very significant, as well as the definition of the possible surface and underground water circulation.

We agree with Reviewer #2: any information about surface water circulation is missing and it will be added to section 2 in the revised manuscript. By the way, there is evidence that nearly no significant surface runoff usually occurs along the investigated slope. About the underground water circulation, as shown by the monitoring data, the soil is usually far from saturation and, as stated at

page 7, lines 177-179, “the values of soil water content and suction observed at the two instrumented locations indicate that in unsaturated conditions there are not significant differences in water potential at the same depth in different points of the slope”: this implies that the main direction of the flow is orthogonal to the slope. However, for the sake of more clarity, in the revised manuscript this information will be anticipated to section 2.
We will also add information about the location of the monitoring station along the slope.

As regards the rainfall data, given that these types of measures require the installation of a rain gouge station in the test area, the authors should demonstrate that records from a station distant more than 20 km, in different meteorological conditions, are representative of the weather conditions of the area.

Here we beg to disagree with Reviewer #2: at page 4, lines 99-101, it is clearly specified that a rain gauge has been installed at the monitoring station: such data have been used for the calibration of the model. About the application to the event of 1999 (when the monitoring station had not yet been installed), it is clearly specified (page 15, lines 348-349) that rainfall data from the rain gauge of San Martino Valle Caudina, less than 3km far from the landslide location, have been used.

The simplification as homogeneous material in front of the multilayer nature of the sequence of the site does not look congruent when adopting a model so complex.

It is clear that Reviewer #2 does not like the idea of trying to adopt a simplified single-layer model to attempt to describe the behaviour of the layered soil profile. As already pointed out above, the monitoring data seem to confirm such possibility: 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 that of a fine-grained soil: this was expected, since a layer of altered ashes, with a significant clay fraction, is often found just above the bedrock (Damiano et al., 2012). Such layer is indicated as D in figure 1, but by mistake it is missing in the description of the typical soil profile given at page 4, lines 83-85: it will be mentioned in the revised manuscript.
However, we do not fully understand the Reviewer’s point: on one hand, she/he considers the proposed model too complex, on the other, she/he implicitly would prefer an even more complex model, in which the layered nature of the soil cover was retained. Here we would like to stress that, considered the complexity of the studied phenomenon, our model is far from being complex: it consists of the two boundary conditions and of a single homogeneous (fictitious) soil layer in presence of root water uptake. As explicitly stated and thoroughly described in section 2 (at least, we hope so), the introduction of the various considered hydrological processes into the model has been suggested by the data coming from the monitoring activities. We will carefully read section 2 and introduce changes wherever possible to make the whole description more clear.

Authors should clarify whether all data were used for calibration.

As stated at page 11, lines 261-262, all the data from 01.01.2011 to 20.07.2011 were used for model calibration. Rather than a validation test, the calibrated model has been then applied to the 1999 rainfall data set. We will modify section 5 (at page 15, lines 346-349) to better clarify this point.

They stated that a genetic algorithm was used without explain(ing) *the calibration procedure* (…) *the use of a genetic algorithm is unusual for the solution of a least squares problem, of continuous type, which can be solved through a traditional procedure (eg Gauss-Newton)* (…) *An objective numerical assessment (not only empirical) of the congruence between measured data and simulated data by the model should be introduced.*

We agree with Reviewer #2, and in the revised manuscript we will add more information about the calibration procedure, especially about the value assumed after calibration by the objective function defined at page 11, lines 263-264.

About the choice of using a genetic algorithm for the search of the minimum of the objective function, we thought we had already explained (page 12, lines 266-269; and page 17, lines 404-409) that it has been taken to avoid possible equifinality problems (in other words, the existence of more local minima within a region in the parameters space where the objective function is flat, which could make the use of a gradient-based algorithm ineffective): such problems can always occur when the parameters of the van Genuchten's retention curve are searched (in fact, to circumvent this problem, which is strictly dependent upon the functional form of the retention curve, many Authors suggest to eliminate one of its parameters by assuming m=1-1/n). To such aim, the genetic algorithm is quite handy, because it allows constraining within predefined regions the unknown parameters, and also because, for its intrinsic randomness, usually performs well in presence of local minima. We will further clarify this points in the revised manuscript.

To be more useful, the proposed model is probably more suitable for a sensitivity analysis in order to understand what are the most important parameters in determining the variations of suction in the soil.

We agree with Reviewer #2: carrying out a sensitivity analysis could be interesting. Anyway, such analysis is beyond the scope of this study for several reasons. First of all, as already pointed out in the previous answers, we are interested in setting up an "effective" model, which does not claim to reproduce exactly the physics of the considered hydrological processes: the bottom boundary condition, as well as the fictitious soil layer and its hydraulic properties do not allow to carry out a sensitivity analysis with clear physical meaning. Besides, even if a sensitivity to the "effective" parameters of the model could be interesting as well, it is quite difficult to carry it out properly, because some of the considered processes may play a significant role only in some particular conditions (e.g., the seasonal vegetation-related parameters; the root water uptake model, which is effective only within a given interval of soil water potential) and we do not have enough data covering all the possible situations.

A large number of parameters listed in the model are assigned based on literature data or on expert assessments; 10 are calibrated based on suction measurements. It would seem that authors use indistinguishably all data related materials with a strongly different hydraulic behaviour, as shown in Fig 5 (…) *Advantages obtained from the model used are not clear with respect to a simplified model. This would help the reader to understand the real contribution of the model in the study, and especially if one can make mathematical simplifications of some components of the model.*

It is not clear what Reviewer #2 refers to: about soil hydraulic parameters, experimental data are used only as a comparison with the parameters obtained by calibration; about the parameters of the conceptual linear reservoir model, it is explicitly stated (page 14, lines 338-341) the limited meaning they assume; all the other parameters come from literature and have nothing to do with the properties of the involved soils.
About the advantages of the proposed model, we thought we had already explained them at page 7, lines 171-175. We will modify this part in the revised manuscript, to underline that there is another advantage: carrying out the hydraulic characterization of layers rich with coarse pumices is very difficult (e.g., it is nearly impossible to collect undisturbed samples), so a multilayered model would rely upon uncertain parameters, thus losing part of its intrinsically more sound physical basis.

References

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