Anonymous Referee #1

General comments

I read with interest this research paper investigating the effect of evapotranspiration on physically based models for rainfall-induced landslides. The topic is scientifically significant for the landslide hazard mitigation. I think this paper can be an interesting contribution and is worth to be published but need some major reworking before publication.

Thank you for the effort spent for the Revision and the valuable suggestions.

First, the introduction is not detailed enough: it lacks of significant contributions in the context of: (1) hillslope hydrology and slope stability

The text will be modified in order to address the Referee's suggestions also considering proposed literature works.

and (2) parameters transfer from physical models to real world., methodology and results should be discussed in more detail specifying some possible limits of the assumptions made. This will lead to more convincing conclusions.

Parameters transfer from physical models to real world represents a key issue in geotechnical problems. Model parameters are typically quantified in laboratory at a scale much smaller than field conditions. In this perspective, according the Authors' view, a strength point of the paper is represented by using for calibration and validation of parameters the findings retrieved by a physical model involving 1 m^3 of material forced by realistic boundary conditions provided by actual meteorological evolution, instead of the traditional procedures based on small specimens subject to artificial boundary conditions. In this perspective, a deep comparison among laboratory, lisimeter and field conditions on the same soil involved in the work is reported in Pirone et al. (2016) [doi: 10.1016/j.proeng.2016.08.427] .

In addition, the satisfying performances of the model using so calibrated parameters for interpreting the landslide event is a further indication about the reliability of the whole methodology , worthy to be proposed as a general frame to quantify soil parameters for silty volcanic covers.

Third, some figures need to be modified, some merged, and some are redundant.

The proposed modifications will be addressed in the revised paper; specifically a merging is proposed for Figures 9 and 10 and for Figures 16,17 and 18.

Overall, the paper merges very important aspects of the hillslope hydrology and stability coupling measurements, physical model, and modeling approaches. For this reason I believe it will be suitable for publication and I hope the comments will help the authors to improve the quality and the impact of their manuscript.

Details

To my opinion specific improvements need to cover the following topic:

a)Literature review is limited.

Based on suggestions of Referee, more space will be given to reference literature works.

b) The methodology section should give more emphasis to the novelty presented in this paper.

We will address the suggestions of the Referee identifying in clearer way the novelty elements presented in the paper; in particular, the idea of i) characterizing the hydrological-thermal evolution in site conditions related to safety conditions as an extrapolation of the behaviour of a reconstituted layer, subject to actual meteorological evolution; ii) adopting the same model for early warning purposes.

Subsection 2.1 and 2.2 are long description of Rianna et al., 2014a,b; Pagano et al., 2010. It is not clear if the authors are adding something new to that papers: if yes they should point it out more explicitly to facilitate the reader; if not, although it is clear that the background provided by subsections 2.1 and 2.2 is important, authors should consider to summarize the in the main text and detail them in appendix. New

Same considerations apply to figures 2 to 7: are they showing new data-results compared to Rianna et al., 2014a,b; Pagano et al., 2010?

Works by Rianna et al. 2014 a,b report data related to the description of the physical model and to the first two hydrological years (2010-2012). Then, firstly, the paper displays two further years of experimental data concerning water storage, water content and suction, unpublished on other journals. The paper also reports the unpublished whole time history of soil temperatures. Under such clarifications, the Authors would prefer to maintain the consistency of Subsection 2.1 and 2.2 and Figure 2 to 7. The revised version will better specify what data are going to be published for first time.

c) Authors should include in their Discussion and Conclusion considerations concerning the hypothesis used in the paper:

i) considering an homogeneous soil whereas many other studies in the area deals with stratified soils;

The Authors will address this point in the "Conclusions" section of the revised version, highlighting how the proposed procedure could be valid only for a homogeneous layer; indeed, only under such assumption 1D conditions can be assumed, as numerically demonstrated by comparing results coming from 1D and 2D analysis (Pagano et al., 2010). However, in this geomorphological context, this condition is widespread resulting also applicable to frequent cases of single homogeneous layers resting on pumices, as the presence of pumices may be replaced by suitable boundary conditions (Reder et al., 2017). Further research works could also extend the procedure to more complex inhomogeneous cases; the eventual assumption of 1D water fluxes has however to be proved, for instance comparing suction predicted in two 2D and 1D hypothesis. At present, studies by Damiano et al. 2017 (10.1016/j.enggeo.2017.02.006), show that one-dimensionality of water fluxes could take place also through sloping layered volcanic soils, so that it is likely that one-dimensionality may be extended at some cases involving layered conditions.

ii) effect of the hysteresis which is evident in the physical model data (Fig. 9-a);

The hysteresis has been neglected in the present study by assuming an unique soil-water characteristic curve fitting all the available observations on calibration time span; the constraints associated to such assumption will be added in the section dealing with parameters calibration; the accuracy loss of the prediction due to neglecting hysteresis is at present topic of new researches, and this will be reported in the "Conclusions" section.

iii) transfer in a real world application the same parameters estimated in the physical model (e.g. is there any limit in using the same hydraulic conductivity, how about preferential flow?);

The question raised by the Reviewer about scale problems related to different hydraulic conductivity arising at different scales (from laboratory to field) is challenging and involve all geotechnical problems. Hydraulic conductivity is often measured for small specimens (micro-scale) and then referred to the site (macro-scale) where the scale change may involve different values. It is worth noting however that the "specimen" adopted in the present study is two order of magnitude larger than those typically adopted, and that hydraulic-conductivity is hence measured at a mesoscale, a condition that should make determined values quite close to site ones. Also about such issue, different elements are debated in Pirone et al. (2016) [doi: 10.1016/j.proeng.2016.08.427] that will be properly cited in revised version.

iv) the assessment of hillslope stability by a threshold approach neglecting the soil mechanic parameters such as cohesion and friction angle;

The chain of events resulting in a landslide of a silty volcanic covers consists in rainfalls, suction drops and induced strength reductions, locally triggering instability due to an internal or external cause, and then propagation of local trigger throughout the cover. The approach followed in the work is aimed to detect the suction levels throughout the slope at which a state predisposing to slope failure is attained. In other words, the philosophy of the approach is that of not dealing with what particular triggering cause able to determine the landslide but, rather, what generalized suction drop determined a slope state prone to propagate a local instability. The suction level at which a predisposing state to landslide takes place depends obviously on strength parameters other than apparent cohesion relating to suction. These are very difficult to characterize and quantify, due to the presence of mechanical effects exerted by root plants. These effects are major, perhaps more significant than other strength contribution, as, in these soils, vegetation is abundant over the entire year. In order to overcome the problems related to characterize vegetation effects and, consequently, set a deterministic slope stability analysis modelling root effects, the approach followed was that to set the early warning prediction straightforwardly on suction levels (or variables relating to suction, as water storage). Taking into consideration that mechanical root effects should in turn be related to suction levels strengthening soils and roots and progressively disappear with suction reductions.

v) the assumption of one dimensional flow: is the early warning threshold (estimating neglecting the lateral flow influence) valid for the entire hillslope? Is there any changes in flow behaviour at the toe of the hillslope or in the less steep locations, where lateral flow could be important?

The answer is in part contained in the discussion to the previous points. In general, the comparison between typical depths of quite homogeneous pyroclastic covers and slope length make reliable for this geomorphological context the assumption of 1D conditions. However, on field, actual conditions may depart from those assumed, (lateral flow influence, fracture increasing flow rate etc.). Local features assumed by the slope hydrology should not affect however that average suction levels throughout the slope making it prone to propagate a local triggering. Generally, local hydrological conditions may be responsible for local triggering, but they are supposed to not affect the state predisposing to propagation.

d) The authors should acknowledge explicitly that the analysis presented for the real case application does not use any measured time series of soil suction or soil water content to validate the model.

This point will be clarified in the presentation of the section treating the discussion of analysis results

Specific comments

All specific comments will be taken in great consideration throughout the paper. Among the other ones [item 15], as suggested by the Reviewer, three goodness of fit indices are employed to assess the model's performances (Nash–Sutcliffe, Kling Gupta Efficiency and coefficient of determination); they are discriminated for calibration and validation period. The results reported in revised version provide encouraging performances. [item 16] The value 4.5 mm represents the mean evaporative atmospheric demand estimated through available weather forcing for Summer (JJA) on time span 2010-2014. [item 17] The soil cover usually experiences such values only in the shallower layers during the dry season (see for example Wilson et al., 1994; 1997); nevertheless, the differences between the two approaches are not related in differences in SWCC but, as reported in the text, mainly to reference soil depth from which water is simulated to be extracted according the two interpretative approaches.