

Interactive comment on “Hydrological modelling of a slope covered with shallow pyroclastic deposits from field monitoring data” by R. Greco et al.

Anonymous Referee #2

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The paper presents a one-dimensional hydrological model to evaluate the influence of rainfall in the activation of shallow landslides in fine-grained deposits. The presented model is quite complex, being referred to soils characterized by 27 parameters including hydraulic characteristics of the soil, evapotranspiration, suction, roots, effect of interception of rain due to vegetation and hydraulic characteristics of the aquifer that is created in the layer of topsoil.

The authors present the result of a testing in a site in which they have carried out measurements of water content and volume of suction at different depth for about 6 months. Synthetic considerations have been presented about the dynamic of the infiltration on

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the basis of the measured data. These considerations should be effectively improved with a more detailed and accurate analysis of results obtained. In this regard it is noted that the authors attempt to reconstruct the retention curve of the ground (Fig. 5) coupling suction and volumetric water content measurements. The different behaviours are imputed to layers with different hydraulic characteristics.

However, 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).

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. They stated that a genetic algorithm was used without explain the calibration procedure, and whether they have used all available data to calibrate the model or the same data were divided into a test and a training set to evaluate the reliability of the model. Furthermore, 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).

The authors state that the model, after calibration, well reproduces the measured data. However is not presented any quantitative assessment of the efficiency of the model to reproduce the experimental data. In addition, from visual comparison between measured data and simulated data, it does not seem that the model reproduces reliably measured values, especially at higher soil depth. In all cases it would be interesting to define the increase of reliability and information that can be obtained with the model presented compared to other more simple models.

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The obtained model is then used to simulate the soil behaviour in the year before the activation of the Cervinara landslide in 1999. They show that the model defines the strong decrease of suction during the rain that triggered the landslide.

Overall it is believed that the article needs a thorough revision:

1) 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. 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. All that in comparison to the numerous articles about the area and pyroclastic deposits (ex. cf. Scopus).

2) 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.

3) As regards the rainfall data, given that these types of measures require the installation of a rain gauge 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.

4) 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;

5) Authors should clarify whether all data were used for calibration.

6) An objective numerical assessment (not only empirical) of the congruence between measured data and simulated data by the model should be introduced.

7) 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.

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8) 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.

Overall, it is believed the work is not completely clear, especially in the aspects of modelling type.

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