

Interactive comment on “A new technique using the aero-infiltrometer to characterise the natural soils based on the measurements of infiltration rate and soil moisture content” by M. A. Fulazzaky et al.

Anonymous Referee #1

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General comments

The declared aim of the manuscript is to propose a new infiltrometric technique to be used for determination of both the rate of water infiltration and the soil water content. Basically, the field test consists of measuring the pressure in an air tank equipped with a nozzle that injects air into the subsoil. The hypothesis is that the pressure drop rate in the air reservoir could be related to the water infiltration rate collected at the same site by a classical double-ring infiltrometer. A power model is then proposed

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to derive water infiltration rate from air pressure drop rate. The relationship contains two empirical parameters that were specifically calibrated for three natural soils and an artificial soil. A similar empirical relationship is proposed for obtaining the soil water content from the air pressure drop rate.

The manuscript is unnecessary long and many sentences are redundant and/or not relevant to the aims of the study. Also as a consequence of the poor organization, the manuscript is difficult to read and the English style is frequently incorrect. However, the major limitations of the manuscript are associated to both the underlying theory and experimental setup. For these reasons I did not go through a detailed list of the wandering sentences and/or inappropriate terms, but I specifically focused on the main drawbacks that need to be firstly overcome.

The main criticism is about the relationships proposed. They are merely empirical and not theoretically supported and their parameters are prone to the influence of several factors (soil heterogeneities, spatial variability, initial conditions etc.) that are not specifically assessed by this study. The overall conclusions of the manuscript are drawn from only three field tests and few other laboratory experiments that are inadequate to give any statistical support to the results. There are several other weak points, listed below, that convinced me that, in the present form, the manuscript is scientifically poor.

Specific comments

1) The theory and the applicability of the aero-infiltrometer is too generically assessed. Introduction fails to give an updated state of the art and methodology does not give a meaningful illustration of the underlying theory for air diffusion into the soil or the practical information for field use of the device. For example, effect of nozzle insertion and fitting into the soil as well as influence of initial soil water content on air diffusion is not analysed (P. 12721, L. 11-20).

2) It is generically stated that water infiltrometers (single or double, pressure or tension) are cumbersome and unpractical (P. 12719, L.15 to P. 12720, L.2), but some

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of the most recent developments aimed at reducing the field experimental effort and extending applicability of infiltrometric techniques (for example the BEST procedure by Lassabatere et al., 2006) are neglected. The conclusion that the aero-infiltrometer could permit a rapid field characterization is questionable given that a soil specific calibration is necessary in any case (P. 12735, L. 12-14).

3) The theoretical support for eq. (4) is lacking. Water infiltration from a double ring infiltrometer is a dynamic process that includes a transient phase, governed by soil sorptivity, that is followed by a steady state condition in which, under the assumption of unit hydraulic gradient, the infiltration rate is equal to the field-saturated hydraulic conductivity (Reynolds et al., 2002). During infiltration, the wetting front deepens and the field-saturated zone underneath the infiltrometer ring extends. In my knowledge, derivation of a static soil variable, like θ , from measurement of only the cumulative water infiltration is not possible. Also questionable is the definition of the soil volumetric water content, that is the volume of water per unit volume of bulk soil (Hillel, 1998). It is known that θ ranges from 0 (oven dry soil) to the soil porosity that, in natural soils, is at most equal to 0.50-0.60. Physically, it cannot be equal to 1 (or 100%) as incorrectly stated by the Authors (P. 12725, L. 29 to P. 12726, L. 2).

4) Despite being totally empirical, eq. (3) relating air pressure drop rate, P , and water infiltration rate, f , can be considered scientifically sound. Conversely, I cannot understand the theoretical basis for eq. (5) between P and θ . As matter of fact, it is based on the relationship between infiltration rate and water content that, as discussed above (see point 3), is questionable. Moreover, Authors do not give any experimental support for its validity. I think that independent measurements of water content during air diffusion from the aero-infiltrometer can be helpful at this aim. In the present form, the validation of eq. (5) cannot be considered valid given that it is conducted by non-independent measurements.

5) The methodology is validated by only three tests conducted in natural soils but detailed information on the soil characteristics (i.e. texture, bulk density, water content at

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the time of measurements) is not given. The experimental setup for double-ring infiltrometer is not explained (ring diameters, depth of insertion, height of ponded water) and it is not clear how water infiltration is measured. Frankly speaking, I am also puzzled about the type of infiltration tests conducted, whether they were constant head or falling head experiments. In fact, it seems from P. 12721, L. 23-24 that "the decreasing of water level" is measured thus supposing the use of a falling head experiment. In this case, the Authors should be aware that the results are more influenced by initial soil conditions and depth of water poured into the infiltrometer ring (Philip, 1992).

6) Soil classification was conducted from infiltration data gathered from a single infiltration test (P. 12727, L. 23 to P. 12728, L. 4) thus neglecting that soil infiltration is strongly affected by spatial variability. Actually, the Authors recognize that dynamic soil properties are high variable and can influence the results (P. 12721, L.21-23), but this issue is not accurately assessed. Moreover, the reported infiltration rate ranges (P. 12727, L. 25 to P. 12728, L. 4) are misleading given that they include the transient stage of infiltration that, as indicated above, is particularly affected by initial soil water content. This could also justify some inconsistencies between infiltration rates and pressure drop rates for the three soils. In fact, the sandy loam soils, that shows expected low f values (2.0 – 6.3 cm/h) is surprisingly characterized by the highest P values (66.9 – 251.4 psi/h). Explanation given for that behaviour (P. 12730, L. 3-15) is in my opinion not exhaustive.

7) Physical interpretation of parameters α and β of eq. (3) is confused (P. 12729, L.24 to P. 12730, L. 3). According to the power relationship (eq. 3), α is the water infiltration rate corresponding to $P = 1$ psi/h and I don't understand the meaning of the sentence on P. 12730, L. 15-17. Evaluation of the role on the initial soil water content on the parameter α , that was conducted on laboratory test data, is interesting and, in my opinion, should be extended to other textures and also to natural soils. In the present version of the manuscript, conclusions regarding the dependence of α from θ are drawn from only four tests (no repetitions) at different initial soil moisture that are

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statistically insufficient to support the conclusions.

8) Discussion is excessively long. The pedant repetition of results already reported in tables and figures is not necessary.

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