

## ***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 #3**

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The manuscript is a resubmission of a former one, published on HESSD 2013 10:12717-12751, that I commented as Anonymous Referee on 5 December 2013. In that occasion, I highlighted several limitations of the manuscript including vagueness of the underlying theory, limited number of field tests, lack of statistical support for the results. Most (if not all) of those weak points were not addressed by the Authors' reply (posted on 13 December 2013) and, unfortunately, the resubmitted version has not been substantially improved. As matter of fact, the present version is (practically) identical to the previous one. Therefore, I don't repeat in detail the criticisms that I raised

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to the previous version of the manuscript but I try to summarize why, in my opinion, its publication is not recommended.

Apart from being unnecessary long and wandering, the manuscript basically tries to prove that the pressure drop rate,  $P$ , measured by the aero-infiltrometer can be used to estimate the water infiltration rate,  $f$ , and the soil moisture content,  $\theta$ , at a given site. At this aim, two empirical relationships are presented that were calibrated using infiltration rates collected by the classical double-ring infiltrometer.

The relationship between  $f$  and  $P$  (eq. 3) is potentially sound but its parameters were calibrated with only three field tests and no independent validation is performed. As I stated in my former review, parameters of the suggested relationship are prone to the influence of several factors (soil heterogeneities, spatial variability, initial conditions etc.) that are not specifically assessed by this study. Even if limited to the three considered soils, a much greater number of field tests should have been conducted to assess statistically the reliability of the proposed parameters, i.e., the reliability of the proposed technique. Considering that a site specific calibration is necessary in any case, I think that this analysis is critically important to decide whether or not the technique is suitable as a surrogate of more tedious and time-consuming infiltration experiments.

The relationship between  $\theta$  and  $P$  (eq. 5) is even more questionable given that it lacks of theoretical support. As matter of fact, the soil water content is estimated by a relationship (eq. 4) that supposes a proportionality to cumulative infiltration. However, it should be considered that, during the infiltration process, the soil water content under the double ring infiltrometer is rapidly changing both in time and space. The soil saturated bulb underneath the infiltrometer ring extends and the wetting front deepens and enlarges. Therefore, the attempt to estimate  $\theta$  from the cumulative water infiltration is misleading, at least because the measurement volume is not defined. I also note that the Authors endure in confusing volumetric water content (that is the volume of water per unit volume of bulk soil) with degree of saturation (that is the percentage of soil porosity occupied by water). Probably, a detailed monitoring of soil moisture conducted

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during the water displacement by air from the aero-infiltrometer could be suggested to usefully investigate, at least empirically, the relationship between the two variables.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2515, 2014.

**HESD**

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