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Discussions

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 #4

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I was invited to review the paper entitled "A new technique using the aero-infiltrometer to characterize the natural soils based on the measurements of infiltration rate and soil moisture content" by Fulazzaky. I would like to thank the editor team for inviting me and apologize for the delay. At first I read the manuscript before reading the comments from the other reviewers. Then I reread the manuscript after reading the other reviewer comments. My opinion did not change but I still cannot make my choice between rejection and major corrections.

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Interactive Discussion
Discussion Paper

To my point of view, the topic of the paper is in the scope of the journal and the willing to link aero-infiltrometer techniques to water infiltration experiments sounds a great issue. Clearly, this subject could be treated and published in HESS. Yet, I consider that the manuscript must not be published in its present form and that a substantial number of drawbacks must be fixed before. In continuity with the other reviewers comments, my main concerns are related to a) the physics or air and water flows, ii) the presentation of the experimental setup and method that could be improved and c) the layout of the manuscript and its form (research paper ? technical note ?).

Regarding the physics of air and water flow, I agree with the fact that it would be fantastic to link both kinds of flows and establish a clear relationship. Yet, the physics are different, as highlighted by one of the reviewer. Water is the wetting fluid whereas air is not. Water will visit micro-porosity or matrix porosity whereas air may visit mostly macroporosity of even the center of the largest pores. Mechanisms are complex and air and water fluxes are linked to different kinds of friction losses. In addition, mechanisms are strongly dependant on air and water phase connexity. In soils with water contents close to saturation, the air phase connexity may no longer be ensured, the air phase constituting an ensemble of disconnected bubbles. In opposite, in very dry soils, the water phase is no longer connex and water locates at the junctions between particles. In these cases, the flow of the non-connex phase is complex and linking flows of a connex and non-connex phases may be a controversial issue. The fickian (or darcean) approach may be unappropriate. The authors should review theoretical aspects about the relations between air and water flows for the specific problem under study. Let say the authors are interested in air and water infiltrations. The authors should address the problems of the links between initial conditions (initial air pressure versus initial water content accounting for hysteresis) and boundary conditions (imposed air pressure versus imposed water pressure head at surface). The revised version of the manuscript may include more details about the links between the physics of air flow and water flow in soil and as well the restrictions of applications of the studied models. Or, if the authors want to avoid these aspects, they should drastically shorten the manuscript and

HESSD
11, C1236–C1239, 2014

propose a technical note dealing only with simple technical aspects.

Regarding experimental setup and calculations, I was unable to understand certain points regarding the treatment of the experimental data and experimental conditions. Firstly, the authors could detail the reasons for their choices in imposed air pressure and water pressure head at surface and the link between these values. Do air infiltration and water infiltration correspond since respective boundary conditions do not correspond necessarily? Secondly, the effect of lateral flow strongly differ with the apparatus since the aero-infiltrometer has small radius and water infiltration is supposed to be 1D with the double ring infiltrometer. Thirdly, the experimentations differ by their durations and volumes of sounded soil. Air infiltration seems quick and may correspond to a small volume of air whereas the water infiltration experiment is much longer (see table 1). As a result the characteristics derived from these experiments correspond to different volumes of samples.

Regarding the treatment of data, could the authors explain how they combined the two kinds of data. In particular, they gathered the data of air infiltration (e.g. columns 1-2 of Table 1) with the data from water infiltration (columns 3-4, table 1) to get more precise data for both air level of pressure drop and cumulative infiltration. I really don't see how they combined the data. This point is all the more crucial that the procedure of combination may affect the relations between air and water variables proposed in Figure 4 and 6. At last, as quoted by one of the reviewer, equation (4) is false since the initial water content can be non negligible and it assumes the profile is uniform in water content (allowing to use mass balance considerations to derive an averaged water content).

Regarding the layout of the manuscript, the reading gives the feeling that some information was added at specific places in the manuscript in response to reviewers' comments. But this has rendered the manuscript too long. In addition, some parts should be placed elsewhere in the manuscript. For instance, lines 5-20 of page 2525 are too general to be in the material and method section. Please, place these in the

HESSD

11, C1236–C1239, 2014

introduction. Instead of these general statements, the material and method section should present more specific details in particular about the way data are combined or statistical analyses. A specific section should include also a proper description of all the soils under study. Or, the authors can turn the proposed paper into a technical note and to show only some of the data of the aero-infiltrometer and water cumulative infiltration and give more details about how the connection can be done between these two kinds of data to derive the maximum of information on the soil hydraulic properties.

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