

Interactive comment on “In-situ evaluation of internal drainage in layered soils (Tukulu, Sepane and Swartland)” by S. S. W. Mavimbela and L. D. van Rensburg

Anonymous Referee #3

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The Authors present results on “In-situ evaluation of internal drainage in layered soils (Tukulu, Sepane and Swartland)”. In my opinion the paper is not published in the current form and should be deeply rethink and rewritten. I agree with most of the General comments of the others Reviewers. Following, I stress some comments I found relevant:

The paper lack of clearness in terminology, hydrological concepts and methods applied. In this respect the paper is confused. This is not a trivial question because there are many errors and this is especially a problem for HESS target and for the main focus

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of the manuscript.

For example, among others, the Authors many times make confusion between the names of the hydraulic properties (permeability vs hydraulic conductivity in the Abstract, soil water release vs soil water content in the Abstract and Introduction) or between the meaning of “internal drainage” used both for water flow process and method to determine water retention and unsaturated hydraulic conductivity.

Along the paper they many times attribute to the gravity the only driving force acting in the drainage process, not just in the first stage of drainage after saturation but also after weeks. This is not correct, then this assumption hampered their results discussion. This question is strongly connected to the DUL concept. In fact, all the discussion on the DUL is distorted by the assumption that a negligible drainage value exist and is a characteristic and well defined soil properties. I do not want to diminish DUL (and the similar field capacity) concepts, very useful for irrigation management, but their physical meaning is questionable. Among the large literature available on this subject, the contribute of Romano and Santini (2002) can help the Authors in this respect. Another question concerns how the DUL value was assumed. The Authors, at page 9803 lines 17-19, wrote that they assumed as DUL value the measured SWC resulting in a drainage flux of 0.001 mm/h.. How it was determined? Was it a profile-averaged value? or they determine a single value for each horizon?

In the Introduction and Material and Methods sections they very often refer to the Instantaneous Profile Method (IPM). A discrepancy between the results discussion and methods description was that the IPM (e.g. Watson, 1966) requires $\theta(t,z)$ and $h(t,z)$ data. In the data analysis section they correctly report in the eq.3 the need of pressure head values in space and time to get the $k(\theta)$ relationship. But in M&M there aren't any pressure head measurements. So, what I think is that the method they applied and many times referred as IPM was just a simplified version of the IPM, with the strong assumption of unit gradient. This is a powerful method, several time applied, but I have some doubts that this assumption will take in strongly layered (from an hydrological

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point of view) soils. In any case they should correctly refer to the applied method.

I do not like the statistical analysis approach. This is just my opinion and feeling, but for this case study, a statistical comparison in each profile of properties of very few horizons does not have much sense. What I think is that, in the view of enlighten the crucial role of stratification and layering in determining drainage, more attention should be given to the hydro-physical behavior of the soil profile more than the statistical comparisons. In this respect the use of simulation modeling, as already stated by another reviewer, can be helpful.

The Discussion section is very confused. I read it several times and it was very difficult for me to capture the central concepts the Authors want to transmit to the reader.

SOME SPECIFIC COMMENTS

Objectives p9801 lines 9-12. The objectives should better reformulate. In fact:

The first objective. . . “to describe the pedological properties that relate to the presence of layering on the three soil types” is not clear to me. What do the Authors mean? They postulate a soil profile description in three soils as the first objective of the work? If yes, this objective is very poor.

The second objective. . . “to determine the soil water release, unsaturated hydraulic conductivity and drainage-time functions that characterised the internal drainage outcomes of layered soils” is also poor. Measurements of $\theta(h)$ and $k(\theta)$ are not very innovative themselves.

- p9798 lines 2-3. Delete “following deep infiltration”. The properties are, at least in the framework of your paper, independent from the process under study.

- P9798 lines 2/7. SWC should not be used for both soil water release and soil water content.

- P9798 line 25. Change “factor” with “process”.

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- P9798 line 26 to p9799 lines 1-2. What this sentence “Since the large pores. . . diagnostic horizons” means?

- P9799 lines 4-6. This sentence “While the former. . . (Eching and Hopmans, 1993).” Is somewhat confused. The soil water retention curve is the relationship between soil water content (θ) and pressure head (h). It is not a function of h and θ .

- P9805 section 2.4.1. Estimation of unsaturated hydraulic conductivity. This data analysis is not applicable without have pressure head data (e.g. by tensiometer readings), otherwise it should be assumed a unit gradient at bottom of the soil profile.

Romano, N. and Santini, A.: Water retention and storage: Field, in: Methods of Soil Analysis, Part 4, Physical Methods, edited by: Dane, J. H. and Topp, G. C., 721–738, SSSA Book Series N.5, Madison, WI, USA, 2002.

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