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Hydrology and Earth System Sciences Discussions

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

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Mavimbela and van Rensburg present results of internal drainage experiments conducted on three layered soils. In addition to the drainage experiments, the water retention characteristics of the various soil horizons were measured in the laboratory using the hanging water column method, and the saturated hydraulic conductivity was measured in situ using a double ring infiltrometer. The hydraulic conductivity functions were inferred from internal drainage data using the instantaneous profile method. The aim of their study was to evaluate the suitability of these soils for infield rainwater harvesting

techniques.

The language of the discussion paper should be revised for grammar, word use and style. It contains several sentences that are grammatically incorrect or very difficult to understand, or both (e.g., p. 9799, l. 23-25; p. 9808, l. 16-18; p. 9812-9813, l. 25-1). The use of abbreviations is sometimes confusing and inconsistent. The cited literature contains quite a large amount of "gray literature" (e.g., Bennie, 1994; Hensley et al., 2000, Slawinsky et al. 2004). The review and description of the instantaneous profile method seems to be incomplete and should be improved.

In addition to these shortcomings I have some major objections concerning the validity of the conclusions and the plausibility of the results presented in this discussion paper:

1) The internal drainage method was applied to derive the hydraulic conductivity functions of the various soil horizons. However, from knowledge of the soil hydraulic properties alone, we can not draw any conclusions about the suitability and potential of these soils for infield rainwater harvesting techniques. An elegant way to achieve this goal would be to simulate in situ soil water dynamics under actual boundary conditions using a numerical model that takes into account all relevant processes governing the water balance under atmospheric forcing such as precipitation, infiltration, ponding, surface runoff, drainage, and evaporation.

2) The drainage experiments allow to infer the water holding capacity of the soil profiles, which is a well established concept and a quantity of interest when characterizing soils from a hydrological point of view, especially in the light of their potential for infield rainwater harvesting. Unfortunately, the authors do not provide this information but use the concept of the "drainage upper limit" instead, which is far less common in soil hydrology and more difficult to interpret.

3) The "classical" exponential equation used to fit the hydraulic conductivity curves is rather outdated. What was the reason for fitting this equation? To enable

statistical testing on the regression coefficients? The outcome of such statistical tests is highly questionable anyway, mainly because the exponential model does not provide an acceptable description of all experimental data (Fig. 5). It shows systematic bias. In my opinion, any statistical test is rather meaningless in this case.

4) The soil hydraulic properties derived from lab and field experiments are inconsistent and physically unrealistic. Most of the hydraulic conductivity curves (Fig. 5) show a very steep drop by more than four orders of magnitude within a water content range of only two to three volume percent close to saturation. The authors provide an explanation for this in the Conclusions section (p. 9817, l. 2-7), which is, however, not conclusive in my view. In theory, the large drop in hydraulic conductivity could be explained with the presence of macropores (e.g., Durner, Water Resources Research, 30, 211-223, 1994). To be a plausible explanation, however, this requires that the corresponding water retention curves show the same decrease in water content in the matric potential range close to saturation. From inspection of Fig. 4 it appears that this is obviously not the case here. Consequently, the steep drop in hydraulic conductivity can not be attributed to macropores. What is the explanation for this discrepancy?

5) For obvious reasons, and as pointed out by Vachaud (Methods of Soil Analysis, Part 4-Physical Methods, ch. 3.6.1.2.a, p. 937-945), the instantaneous profile method is not applicable to nondrainable soils. But exactly this seems to have happened in the present study. Both the Tukulu and Sepane soil have a clay rich C-horizon (soft rock) underlying the profile at 85 and 80 cm, respectively (Tables A1 and A2). The Swartland soil has fine textured A- and B-horizons overlying a coarse textured C-horizon (saprolite) at 40 (Table A3). Consequently, the three soil profiles are not expected to drain freely, although for different reasons. This is also reflected in Fig. 6, which indicates that the drainage process effectively

ceases after only about one or two days (with the exception of the C-horizon of the Swartland soil). The minor decrease in soil water content (in the order of 0.2 percent by volume) that occurs in the remaining part of the drainage experiment may not necessarily be due to drainage, but may as well be explained with water loss due to evaporation either through the surface or the side walls of the monoliths.

6) The authors conclude that drainage losses from soil horizons with high clay content should be around 1 percent of total annual rainfall (p. 9816, l. 20-22). I can not follow this conclusion. Based on what kind of information was it drawn? What are the underlying assumptions?

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