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Interactive Comment

Interactive comment on "Use of field and laboratory methods for estimating unsaturated hydraulic properties under different land-use" *by* S. Siltecho et al.

Anonymous Referee #1

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USE OF FIELD AND LABORATORY METHODS FOR ESTIMATING UNSATURATED HYDRAULIC PROPERTIES UNDER DIFFERENT LAND-USE

S. Siltecho, C. Hammecker, V. Sriboonlue, C. Clermont-Dauphin, V. Trelo-ges, A.C.D. Antonino, R. Angulo-Jaramillo

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This paper deals with the usability of the soil properties measured or estimated by different experimental methods for modeling soil water flow. In my opinion, the topic treated by the Authors is important both scientifically and practically. The scientific

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interest is related to the development of a fully objective approach for simulating dynamics of soil water in the vadose zone, with particular emphasis to the choice of the procedures that have to be applied to obtain the necessary input data to characterize the porous medium. The practical interest is related to the possibility of effectively using hydrological modeling as a tool to improve water management in a zone of interest. In general, the manuscript represents a contribution to soil water flow modeling, although the results have only a local validity (but they have, in my opinion, a general interest). However, I believe that there are points throughout the manuscript that should be better discussed. Some improvements and developments are also necessary.

Abstract The Authors state that "location was not a significant discriminating factor when all methods were considered together". I have not clear what is the reason to check a location effect in this particular case. Moreover, this result could be more or less expected in the sense that the variability between methods masks the variability between locations.

Introduction "... to a complete characterization of hydraulic characteristic curves. Finally we compared...". The passage from an introductive reasoning to the description of the aims of the investigation is too abrupt.

Materials and methods The cylinder used for the beerkan experiment was small (10.2 cm in diameter), implying that there was the risk not to sample a representative surface area. Why did the Authors make this choice?

It was not clear how many replicate beerkan infiltration runs were carried out at each site. Six runs were reported in the text (page 6107) but three to 11 runs were reported in Table 3. Moreover, for the beerkan method, this table lists a sample size for each site differing with the considered parameter.

A description of the disc infiltrometer experiment and the applied procedure to analyze the data was not provided.

An appreciably larger disc (20 cm) was used for the disc infiltrometer experiment than the beerkan run. I think that an effect of the source size on the measured soil parameters should be expected. Why did the Authors use sources with a so different size?

The number of soil samples used to characterize each site with the evaporation and the inverse methods was two (text, page 6108) or two or three (Table 3). On what basis did the Authors believe that this was an appropriate sample size for soil characterization at a site?

More in general, the Authors should explain the reasons why, for a given method, the applied experimental procedure, including sampled soil volume and number of replicated measurements, was initially thought to be adequate for soil hydraulic characterization.

Including a short description of the approach used to adjust the curves obtained with BEST to van Genuchten with Mualem conditions (page 6107) would allow an easier reading of the paper.

The Authors should also explain how they established the constant evaporation rate of 8$\times$10-9 m s-1 (page 6108).

As reported on page 6105, soil water content and pressure head was monitored continuously at each site for three years (2007-2009) at 7 and 5 depths, respectively. It is not clear the reason why only a part of these data were used (2.3 Evaluation of the methods). In my opinion, it would be more logical to use all the experimental information to make the modeling check.

I am puzzled about the possibility to obtain a statistically (and also physically) plausible information with only three data points (page 6111). Again, the Authors should convince a reader that their experimental methods and procedures were sound and that applying statistical testing was meaningful, despite the very small sample sizes.

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Results and discussion I found this section too much poor in terms of interpretation of the results since I did not find any attempt to suggest possible reasons of the detected differences. I only show a few examples to be clearer, but the problem is general. In comparison with the beerkan method, the disc infiltrometer yielded higher Ks values at some sites and lower Ks values at other sites. Why? In my opinion, a possibility could be that sample sizes were too small to yield reliable results. But this should not be the interpretation by the Authors because it would imply that the experiment was not properly realized. Another example is the lack of any attempt to explain the results of the statistical analysis (pages 6114-6115). Still another example is the fact that hysteresis was expected, due to the soil textural characteristics, but it was not detected in the investigation (page 6117).

I also see points not clear from a methodological point of view. For example, the Authors do not explain the reason why, with reference to the beerkan method, a comparison was established in terms of shape parameters but not with reference to scale parameters. In any case, figure 3 is not easy to read. Moreover, it is not clear why the discussion in the text starts from figure 3d and it goes back to figure 3a.

According to the Authors, the data were collected on a generally gentle slope (3%, page 6105) and the field slope had a very noticeable effect on modeling validation since Hydrus 1D performed well only when infiltration was strictly 1D. In my opinion, it is necessary to better develop this part of the manuscript showing more results, also from Hydrus 2D, and also establishing comparisons between the two modeling approaches (Hydrus 1D and 2D). In addition, the Authors have the task to physically convince a reader that a slope of 3% is enough to induce substantial lateral flow. Are we sure that using the Hydrus code was in general an appropriate choice? Why?

Final comments I found some typing errors throughout the manuscript (for example, Univserité on page 6099, Simuneket on page 6109, Table 2 instead of Table 3 and infiltration instead of retention on page 6113). A check of the entire manuscript is advisable to remove these errors from the final version of the paper.

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Appendices A, B and C describe in short methods that have been reported in great detail in many other articles and textbooks. Therefore, I do not see the need to include these appendices in the paper. Maybe, the saved space could be used to improve the manuscript in terms of physical explanation of the results.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6099, 2014.