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Discussions

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

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We would like to thank the referee for the thorough reviewing of our manuscript. The Comments and criticisms have been taken into account and the questions answered here below :

Q: 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.

R: Rather than the location, the associated land-use (rubber tree plantation, natural forest and pasture) is often assumed to modify deeply the unsaturated soil hydraulic properties. This will be mentioned more precisely in the text of the Abstract.

Q: 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.

R: The sentence has been reworded to smoothen the transition between the introductive part and the description of the aims.

Q: 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?

R: The choice of the diameter of the cylinder for Beerkan method was motivated by the experience from literature (Braud et al. 2005) and the availability of the material. As the soil in this area is sandy and has little or no structure, it was assumed that the diameter of the infiltration cylinder would not impact the representativity of measurement. This point will be developed further

Q: 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.

R: The number of replicates for Beerkan method infiltration tests was modified in the text from "six" to "three to eleven". The size of the samples differed from one location to another because only exploitable data have been shown.

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

R: The experimental procedure for tension disc infiltrometer will be added to the text. The principle of the tension disc infiltrometer is based on maintaining the water in the apparatus under a controlled tension, so that only pores with higher matric potential can be filled. With this technique the biomacopores, cracks and other structures promoting preferential flow can be ignored, to measure hydraulic conductivity strictly in the soil matric. Tension disc infiltrometer consist of a water reservoir, a Mariotte bubling tower, and a contact disc of diameter 20 cm covered with a microporous nylon membrane (with a pore diameter of 20 micrometers). The outlet of water reservoir was connected to the center of the disc with a flexible plastic tube. Quality of the measurement depends largely on the preparation of the device. Therefore, in order to ensure good saturation of the apparatus, avoiding the presence of any air bubble, water was introduced by suction into the device. The disc was immersed into a water filled bucket, and a slight vacuum controlled with a hand pump was used to suck the water into the disc and the reservoir. This procedure also contributed to proper saturation of the nylon membrane. The depth of the immersed inlet tube in the Mariotte tube controls the tension potential h0 applied to water at the disc membrane by adjusting the water height in the air inlet tube. Soil surface has to be cleared of vegetation and leveled to ensure perfect contact with the infiltration disc. Usually the soil surface was slightly covered with clean fine sand to get a smooth horizontal surface and to provide a good contact between the base of the disc and the soil below. As the soil surface is never perfectly horizontal, the relative position of the infiltration disk with the water reservoir is not necessarily constant. Therefore it important to measure it in order to calculate the actual water potential head h0 (cm) controlled with the immersed tube of the Mariotte device. In order to calculate saturated hydraulic conductivity with the multipotential method (Perroux and White, 1988; Smettem and Clothier, 1989), the infiltration measurements have been realised for two different suctions values and interpreted with Wooding's method (Wooding, 1968; Akeny et al., 1991; Angulo-Jaramillo et al., 2000). The tension values used for the experiments were not always exactly the same as they partly controlled by the soil microtopography. But were generally between -15 and -10

hPa for the higher tension and between -7 and -3 hPa for the lower tension.

Q: 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?

R: The diameter of the disc infiltrometer was indeed almost twice of the diameter of the infiltration cylinder. This difference was mainly explained by the availability of the different equipments, especially for the infiltration cylinder. However according to Anderson and Bouma (1973) and Bouma (1980) the representative elementary volume of sandy soil for measuring hydraulic conductivity is usually considered to be 100 cm3. Considering the texture (mainly sandy) and especially the lack of structure of the soil in the different locations (except in the forest) the volume of the soil samples exceed the Representative Elementary Volume. Therefore despite not having been measured on strictly the same volumes or areas but still in the order of magnitude (Beerkan 800 cm3, Disc infiltrometer 2000 cm3, Evaporation 1500 cm3) the results for the different methods can legitimately be considered as adequate.

Q: 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.

R: The number or replicates for each method depended on the time necessary to perform the measurement, and secondly on the quality of the measurement. Therefore there are much less evaporation measurements, as each experiment lasts two weeks, than Beerkan measurements (around 30 to 60 min for one infiltration experiment) or disc infiltrometer measurements (2 to 6 hours). Finally, as stated previously, it also depended on the number of exploitable measurements, for the results of the infiltration experiment reveal only after processing the data, and some have to be discarded.

Q: 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.

R: The original Beerkan method provides van Genuchten's retention parameters for Burdine condition (m=1-2/n). The retention curve was plotted according to these parameters and the equation with Mualem condition (m=1-1/n) was adjusted with a Marquardt procedure to fit the new parameters. In fact by changing from Burdine to Mualem condition all the parameters were affected.

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

R: The constant evaporation rate was obtained by placing the soil samples in an oven constantly heated at 40°C with air renewal.

Q: 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.

R: Thought the tension was recorded over a wider period than presented here, the measurements were not continuous simultaneously for the three tensiometers. The tension data suffer from several long gaps due to various technical problems (air entry for the upper tensiometer, pressure sensor failures, etc ..) . The modeling was therefore not possible continuously for the entire period.

Q: I am puzzled about the possibility to obtain a statistically (and also physically) plau-

sible 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.

R: As explained previously the number of data depended on the time required to perform the measurements and on the number of measurements that finally were exploitable. The statistics used in the paper namely non parametric tests are aimed to give information on for very small samples (N<6) and therefore seem to be adapted for this study.

Q: 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).

R: The discussion about the interpretation of the different results has been increased in the text : Tension disc infiltrometer measurements are very dependent on the quality of the contact between the disc and the soil surface. Despite all the efforts this requirement can be quite difficult to fulfill. Moreover, in sandy soils local hydrophobicity can occur (especially in rubber tree plantations, where natural rubber is known to have water repellent properties) and therefore affect infiltration dynamics. This phenomenon was affecting particularly the measurements with disc infiltrometer, as soil suction was the driving force. Whereas for Beerkan method the contact is not a problem and due to the slight positive charge hydrophobicity is less problematic. Though

Beerkan and disc infiltrometer method are realized with an imbibition experiment, they don't exactly provide the parameters representing imbibition. In fact disc infiltration method described the saturated hydraulic conductivity and therefore does not provide any information about the wetting part of the retention or hydraulic conductivity curve. For the Beerkan method, parameter α is derived from the infiltration experiment but the procedure to determine it is derived from parameters available in databases but that are usually derived from drainage experiments. Beerkan method can't be considered as a method describing strictly an infiltration process as infiltration and drainage data are used together.

Q: 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.

R: The comparison was made in terms of shape parameters for all the methods. The purpose of scaling the results was to show the influence of the shape parameters between the different land-uses and especially the different methods. Moreover it is mathematically more complicated to perform the same scaling procedure with the shape parameters. However the main reason is that shape parameters depend mainly on the soil texture, that varies much less at a small scale (field, toposequence), than the scale parameters. The reduced hydraulic conductivity curves are presented in attached pdf file.

Q: 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?

R: This point is developed in detail in another paper (Seltacho et al. 2013) where is clearly shown that lateral drainage represents 40% of the annual rainwater according to both (experimental measurements and modeling with hydrus 2D). Moreover the slope of the plan on which the water flowed laterally, corresponding to the interface between the sandy and clayey layer, was notably higher as the depth of the sandy layer increased from upper to lower slope position. This will be explained in more detail in the text.

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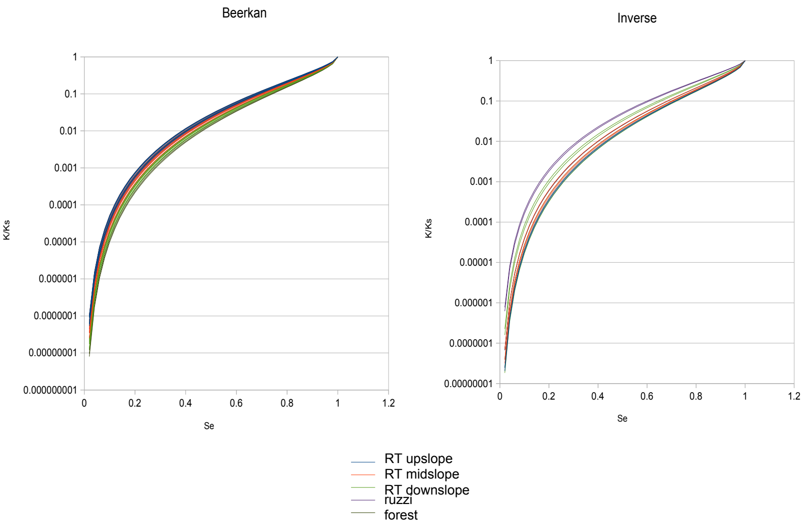

Fig. 1. reduced hydraulic conductivity versus effective saturation

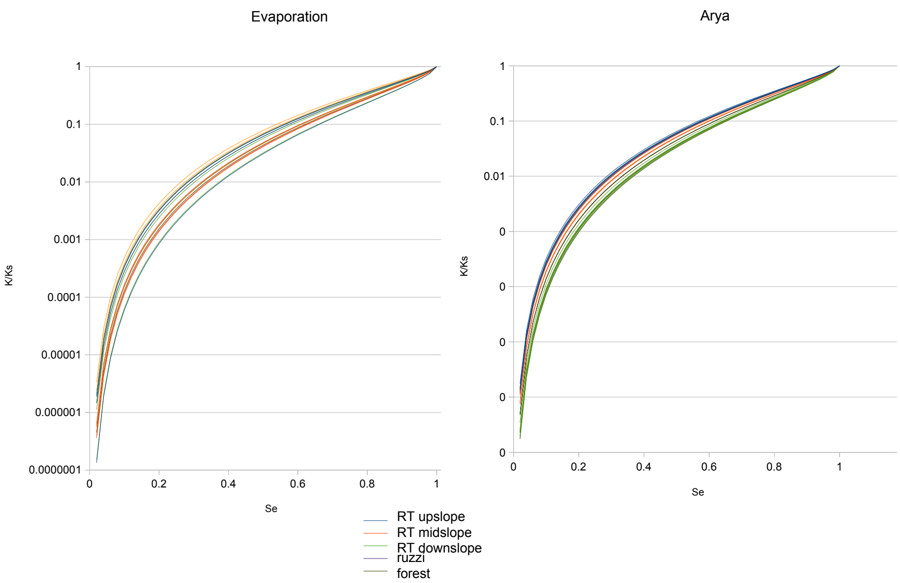

Fig. 2. reduced hydraulic conductivity versus effective saturation

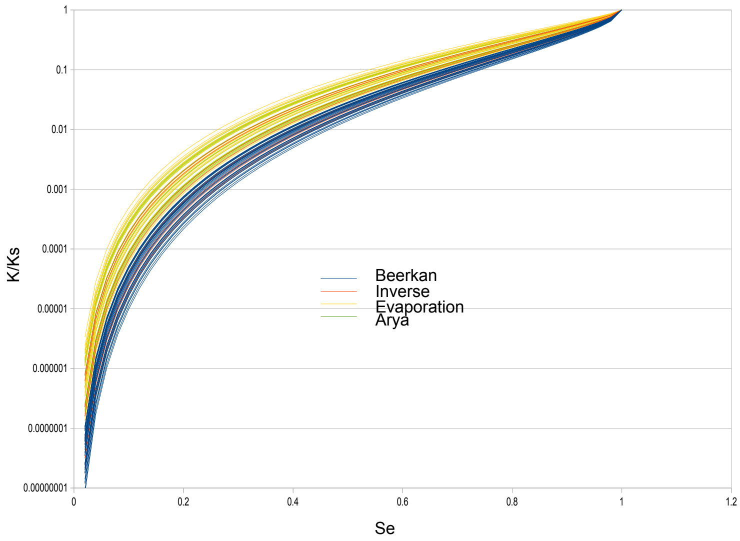

Fig. 3. reduced hydraulic conductivity versus effective saturation