

## ***Interactive comment on “Comparison of three measurement methods of saturated hydraulic conductivity” by C. Fallico et al.***

### **Anonymous Referee #3**

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Peer Review of, “Comparison of Three Measurement Methods of Saturated Hydraulic Conductivity and Remarks on Possible Affecting Factors” (2006-0061), which was submitted for possible publication in Hydrology and Earth System Sciences (HESS)

This manuscript compares the tension infiltrometer, pressure infiltrometer and soil core methods for measuring saturated hydraulic conductivity in a sandy-loam soil. The among-method comparison includes mean value, variability and statistical distribution of the saturated hydraulic conductivity results, plus comments on sample size, flow geometry, boundary conditions, and response to soil heterogeneities. The study is of interest and relevance to the readers of HESS. I have a number of concerns and comments, however, which should be addressed adequately by the authors before the manuscript is considered for publication. These are detailed below.

## A. General Comments

1. The essential conclusions seem to be: i) the three methods are not comparable because of differing sample volumes, flow geometries, boundary conditions, and responses to soil heterogeneities, especially macropores; and ii) the optimum method depends on the advantages/limitations of the method and the problem under investigation. Although these conclusions may be valid, they are by no means new, as many other investigations spanning several decades have reached essentially the same conclusions (see e.g. references cited on P.989, L.15-17). In addition, the conclusions for this particular study are based largely on conjecture and circumstantial evidence, rather than on hard evidence or data obtained in the study. Hence, the authors need to clearly indicate what is new and unique in this study that adds to our knowledge base regarding the measurement of hydraulic conductivity and/or the use of the tension infiltrometer, pressure infiltrometer and soil core technologies. They also need to provide clear evidence or analyses that support the conclusions of the study.

2. The authors state that the tension infiltrometer (TI) method is used extensively for in-situ measurements of saturated hydraulic conductivity,  $K_s$  (P.989, L.27). I do not believe this to be the case, and the usefulness of the TI method for  $K_s$  measurement is somewhat in question. Although the TI method is indeed used widely to measure near-saturated hydraulic conductivity at pressure heads ( $h$ ) ranging from about -15 cm to 0 cm, application of the method for the positive  $h$  values needed to measure  $K_s$  can be problematic, both from a theoretical perspective and from a practical perspective. Theoretically, the Wooding (1968) analysis (P.990, L.24 to P.991, L.3) does not apply for  $h > 0$ , and Booltink and Bouma (2002) showed that  $K_s$  at  $h = 0$  can be several orders of magnitude less than  $K_s$  at  $h > 0$  due to incomplete saturation of macropores (see e.g. their Figure 3.4-6). Furthermore, rapid-flow impedance effects in the TI contact sand can cause the effective  $h$  at the soil surface to be negative, even though a slightly positive  $h$  was set on the TI membrane (Reynolds and Zebchuk, 1996). Hence, the TI method is not well suited for  $K_s$  measurement; and as a consequence of the above,

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it is no surprise that the TI method with near-zero  $h$  and contact sand yields lower  $K_s$  values than the pressure infiltrometer (PI) method which uses large positive  $h$  values and no contact sand. The authors consequently need to justify their use of the TI method for in-situ measurement of  $K_s$ .

3. It is well established that sample dimensions and sample volume have a large impact on the representativeness of  $K_s$  measurements. For example, Bouma (1985) concluded that a soil sample must encompass at least 20 structural units (e.g. soil peds, macropores, etc.) before representative  $K_s$  measures are obtained. Furthermore, Youngs (1987) and Lauren et al. (1988) suggest that ring infiltrometers may need to be at least 20-30 cm in diameter before they consistently yield representative  $K_s$  estimates in structured loamy soils such as the soil in this study. Hence, the 9.8 cm diameter by 6 cm long pressure infiltrometer (PI) rings were potentially too small to yield realistic  $K_s$  estimates, while the 4 cm diameter by 5 cm long soil cores (SC) were almost certainly too small to yield realistic  $K_s$  estimates. In view of this, the authors should justify the PI ring and SC core sizes they used, especially the core sizes.

4. Although the authors are to be commended for writing in English, which is clearly not their first language, substantial editing of the English is required for clarity and conciseness. In addition, there are many paragraphs that consist of only one sentence, and sometimes the sentence is very long and convoluted. Also, curious and ambiguous terminology is occasionally used which needs to be rectified [e.g. ‘slimy’ (P.992, L.17), “trampling plane” (P.992, L.18), “modality” (P.994, L.12), “amplitude” (P.995, L.20), “soil topper” (P.1001, L.11), etc.] (see below).

## B. Specific Comments

1. P.992, L.17-18. Does “slimy” mean “silty”? Does “trampling plane” mean “soil surface”? Please clarify.

2. P.993, L.6-8. Give the water-entry value of the white contact sand. The pressure heads set on the TI membrane must be greater than (i.e. less negative) than the water-

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entry value of the contact sand to ensure that the contact sand does not become an impeding layer due to desaturation.

3. P.993, L.13-14. Give the values of the pressure heads set on the TI membrane. It is important to show how the pressure heads were set to ensure field-saturated flow in the soil, which is required to obtain the soil's Ks.

4. P.994, L.1. Do you actually mean “blunted upper” rim, or “sharpened lower” rim. Please clarify.

5. P.994, L.12. By “the modality” do you actually mean “normally”. Please clarify.

6. P.995, L.4-8. It would help the reader substantially at this point if a schematic diagram of the field site was given indicating the positions of the two sampling areas, how the three methods were arranged within the sampling areas, and the number of samples/measurements collected using each method. Were the measurement locations randomized? Were all three methods applied at each measurement location? Please clarify.

7. P.995, L.17. Define “u”. Presumably it is the “probit” variable.

8. P.996, L.10-11 (Table 2). Give more detail on how the R-square criterion is used as a test of the appropriate statistical distribution. Presumably, the R-square value indicates how well the data approximates a straight line when plotted on normal and log-normal fractal plots (Fig. 2, 3). Are the R-square values for the log-normal distribution sufficiently greater than those for the normal distribution to conclusively show that the data are better described by the log-normal distribution? Also, artificially large R-square values can be caused by a few “outlier” or extreme data points. Perhaps better and/or additional descriptors of how well the data fit normal and log-normal distributions might include calculations of skewness, kurtosis and coefficient of variation. You might also consider conducting formal normality tests, such as Anderson-Darling, Shapiro-Wilk, etc.

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9. P.996, L.17 (Table 3). I do not understand your definition of “range” (A). Do you actually mean the subtraction of the maximum Ks (Kmax) from the minimum Ks (Kmin), as implied in the table heading? If so, this is not a good expression of range. One option would be to calculate  $\log_{10}(K_{\max}/K_{\min})$  which gives the range in orders of magnitude. Another option is to simply add a column of maximum values and a column minimum values to the table, which may be preferred as it provides more information to the reader. It is confusing and inaccurate to refer to ‘range’ as “amplitude”, since amplitude implies periodicity.

The coefficient of variation (CV) and standard deviation (SD) statistics are equivalent [i.e.  $CV = (SD/\text{mean})100$ ], and hence including both does not give any additional information. I suggest deleting SD and retaining CV, as CV gives a better and more intuitive feeling for data variability than SD.

It is not clear if the Ks mean values are significantly different or not. Would it not be possible to conduct a comparison-of-means test (e.g. LSD, Tukey, Duncan, etc.) to determine if the means are different, and then include the usual indicator letters after the mean values to show whether or not the means are significantly different at  $P < 0.05$ ?

10. P.997, L.10-13 (Table 4). The headings, “normal series” and “ln-transformed series” are ambiguous, and should be renamed something like, “raw data” and “log-transformed data”, respectively. Are the correlation coefficients significantly different ( $P < 0.05$ ) from zero or unity?

11. P.997, L.14-16 (Tables 3 and 5). This statement is not clear. Are you saying that there are no significant differences ( $P < 0.05$ ) between the Ks mean values in Table 3? If so, indicate this via different letters after the mean values in Table 3 (see also comment 9 above). Replace the “division” signs in Table 5 with “dashes”.

12. P.997, L.23-29. This discussion regarding the statistical comparison of the data sets are not clear. Please clarify. Comparison-of-means tests, such as Tukey, Duncan,

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etc., do not require equal sample sizes. The comments for Table 2 (above) also apply for Table 6.

13. P.998, L.8-11. The comments for Table 3 (above) also apply for Table 7.

14. P.998, L.13-17. This comment is not clear. Please clarify.

15. P.999, L.8. Does “modality of providing some water” actually mean “method of applying water”? Please clarify.

16. P.1000, L.1-6. Additional, and perhaps more important, reasons for the TI discrepancy include an inappropriate flow analysis for positive pore water pressure heads (i.e. the Wooding, 1968, analysis), and lack of complete macropore saturation at the near-zero pressure heads applied to the soil surface under the contact sand (see General Comments, item 2).

17. P.1001, L.11. By “soil topper” do you actually mean “soil surface”?

18. P.1001, L.13-16. Pulling grass out by the roots would most certainly create additional soil disturbance and additional macropores. This procedural artefact would in turn increase the variability of flow both within and between measurement sites, thus further increasing the variability in Ks within and among the measurement methods. It would have been better to have cut the grass off at the soil surface, leaving the roots in place.

19. P.1001, L.24 to P.1003, L.7. Given that the SC cores were apparently collected from within the PI rings shortly after the PI measurements (P.994, L.20-21), it is possible (perhaps likely) that the lower mean Ks value for SC was caused primarily by compaction and/or macropore collapse in the wet soil during core insertion, as wet soil has much lower strength than dry soil. This possibility is supported by the fact that the SC statistical distribution has a lower slope than the PI distribution (Fig. 2), which is in turn consistent with the fact that finer-textured wet soil is more susceptible to compaction and macropore collapse than coarser-textured wet soil. Perhaps a

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plot of SC-Ks versus PI-Ks would be useful for characterizing how the SC method behaved relative to the PI method, especially with respect to the 1:1 line and site-by-site estimates of soil texture and macrostructure.

20. P.1002, L.24 to P.1003, L.1. Are “splits” the same as “cracks”? By “swilling” do you actually mean “swelling”? Please clarify.

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