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Interactive comment on "Unrepresented model errors – effect on estimated soil hydraulic material properties" *by* Stefan Jaumann and Kurt Roth

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Dear Editor:
The study is interesting and demonstrates a huge work. However, before it can be transferred to the HESS step of the journal, I suggest the authors should discuss some key points and possibly make some changes in the text. I apologize for having been a bit late with my appraisal, but this also gave me the opportunity to read the comments from another referee and one discussant. I have listed below one general comment and several specific remarks, the most significant of which are starred (*).
Reply: We thank the reviewer for the constructive comments and suggestions. The manuscript was revised accordingly. Hence, we refer to the revised manuscript.

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Discussion paper

General Comments

As a referee, but also as a reader of studies dealing, among various sources of uncertainties, also with those associated with the locations of sensors that monitor a flow process, there is always something causing me some concern. When setting up an experimental test, efforts are made reducing errors (especially the systematic errors) and, among other things, one measures the positions of the various sensors as accurately as possible. I also understand that this task can be a bit more complicated under field conditions, especially when inserting the sensors at the greatest soil depths. Therefore and to the benefit of a wider readership, the authors should justify more why they are interested in this type of uncertainty. Moreover, I have the feeling that the error in sensor location should be viewed more as a systematic error rather than a random error. I think that the method employed by the authors might not be adequate to treat the presence of systematic errors. Some clarifications and a discussion on this point seem deserving.

Reply: We agree, that efforts are made to measure the positions of the various sensors as accurately as possible. Yet, the surface and/or the subsurface structure may change with time and requirements for accuracy and precision may change a posteriori. We clarified this in Sect. A1.4 (Page 23, Line 16).

We agree that the uncertainty in the sensor position is a systematic or structural error. This is the reason why this uncertainty was represented and the parameter estimation algorithm was used to propose more consistent positions of the sensors minimizing this systematic error.

Specific remarks

(*) P.1, L.13. The authors claim that the approximated soil water retention function is reasonable close to the inversion results*. Actually and allowing for the types of water flow processes investigated, it would have been more interesting and effective that the favorable outcome is in terms of the unsaturated hydraulic conductivity function. From the results depicted in the right plots of Fig.10 and Fig.13, this does not seem*

the case.
Reply: Lacking direct measurements of the unsaturated hydraulic conductivity at the position of the TDR sensors, the presented method merely yields an estimate of the initial hydraulic state and an approximation of the soil water characteristic. The remaining parameters for the initial hydraulic conductivity function (K_s and τ) are taken from Carsel and Parrish (1988, 10.1029/WR024i005p00755) and are independent of the presented measurement data. Hence, the presented method is not applicable to approximate the hydraulic conductivity function.

P.1, L.20-23. On the topic of inverse modeling applied to Soil Hydrology, I suggest citing the more recent and comprehensive papers by Hopmans et al. (2002) and/or by Vrugt and Dane (2006). Concerning the lab-scale experiment, the paper by Romano and Santini (1999) also treat types of errors of interest for the present study. As for the in-situ applications, the paper by Romano (1993) can also be in line with some aspects of the present study.
Reply: We revised the introduction accordingly. Please also note the reply to comment 1 of SC1.

P.1, L.22. The paper by Schneider et al. (2006) was published in HESS, not in Hess-D.
Reply: We corrected the reference.

(*) P.2, L.10-13. It is not clear (at least to me) which processes the authors are talking about. For example, the sensor position is definitely not a process. Moreover, as far as I am aware, the previous studies refer to minimum unknown parameters to be estimated mainly because they employed the classic χ^2 penalty criterion coupled with the Levenberg-Marquardt (LM) algorithm. Why do not compare the present results with those ones whether you use, for example, the DREAM tool developed by Vrugt (2016)? By doing that way, the paper would be even more interesting since the authors

claim of having developed a modified LM algorithm.
Reply: Since the applications of the referred studies are diverse, we kept the formulation more general. One exemplary process, which is neglected in most of the studies, is small-scale heterogeneity.
As the major focus of the manuscript, we investigate the effect of neglected structural errors which lead to suboptimal results using the χ^2 penalty criterion. Therefore, we also use the χ^2 penalty criterion coupled with the Levenberg-Marquardt algorithm and quantify the effect of unrepresented model errors by resulting residuals and material properties of the different setups (Sect. 2.3 and Sect. 2.4).
In order to compare the best result of the different setups, we are rather interested in maximum likelihood instead of its distribution in this work. The former is more efficiently found with the Levenberg-Marquardt compared to the DREAM algorithm. Additionally, if the χ^2 is used as likelihood function in DREAM, the discussed problem of neglected processes and uncertainties will remain the same as we use a flat prior in this study. Also, adding additional material would make the already long manuscript even longer.

(*) P.4, L.8-10. Strictly speaking, the θ-based Richards equation describes the variations in space (x, y, and z coordinates) and time (t) of the volumetric soil water content. Then, due to the selected relationship between water content and matric pressure head, one can retrieve the corresponding variations in h.
Reply: We changed the wording in Sect. 2.2.1 (Page 4, Line 9).

(*) P.19, L.25-27. This is a quite common outcome when modeling of data with a maximum likelihood estimator and optimization techniques. I think that this problem should be addressed in another way. Namely, more in terms of the information content of the available input datasets. Does the initial information content increase when adding the additional data? Are the additional data not at all, or weakly, or strongly

correlated among them and with the already available input datasets?
Reply: If the sensors monitor hydraulic dynamics which are not represented perfectly in the model, the residual will increase as the probability to monitor these model errors is increased with the number of sensors. In information theory, the information content of data is often quantified with measures such as the Shannon entropy. In order to apply these measures, the input data have be transferred to random data. This requires knowledge about the general data structure which has to be gained from the data themselves. This implies massive practical issues in heterogeneous media. Since the TDR data monitor the same process at different positions, the Pearson correlation coefficient of the data is mainly positive and depends in particular on the recorded hydraulic dynamics. As the materials A and C which are flipped in case I and III, the characteristics of the monitored hydraulic dynamics changes. Hence, the correlation of these data is weak in general. The hydraulic state of material B is monitored at a similar position in cases II and III. Thus, the correlation of the according data increases.

As general and final comment, I should say that the English usage is very good. Nevertheless, the text is hard to follow. I do not have suggestions on this point, but the authors should make any effort to improve this aspect of the manuscript. Also, sub-section 4.1 might be left out from the manuscript, whereas I do not see the need to have so many small sub-sections in Section 3. Section 6, albeit being a summary, seems pointless and ineffective, chiefly because it also contains many repetitions. A real concluding remark section would be more effective, if necessary. Footnotes are rare or even absent in our scientific literature.
Reply: We revised the general structure of the manuscript. Please note the reply to comment regarding Page 6, Line 19 of RC1. We also revised Sect. 3 and Sect. 4 to make them more concise and generally integrated the footnotes into the text.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-109/hess-2017-109-AC4-

supplement.pdf

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