

Review of manuscript HESS-2017-109

**Unrepresented model errors –
Effect on estimated soil hydraulic material properties**

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 (*).

General comment

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.

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.
- 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.
- P.1, L.22. The paper by Schneider et al. (2006) was published in HESS, not in Hess-D.
- (*) 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.
- (*) 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 .

- (*) 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?
- 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.

References cited

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