

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

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I congratulate the authors to an interesting study at the ASSESS experimental site. I consider the topic and the discussion manuscript highly relevant and worth to be published in HESS. Because of this, I would like to contribute some comments for a revision.

1. If I understand correctly, the authors argue for a retention-dynamics-based identification of soil hydraulic material properties based on inverse modelling of an imbibition and outflow experiment. There have been many studies on the issue of inverse parameter estimation, which I consider relevant for the MS. This also holds for the discussion of heterogeneity and “unrepresented model errors”. I.e. the authors name the “validity limits of the Richards equation” but I do not see the conceptual basis of the argumen-

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tation for their approach. Moreover, I suggest to present an independent reference for the found parameters (e.g. from laboratory analysis) and to include a critical view on the TDR inferred soil moisture values.

2. Despite my appreciation of the logical intention of the structure of the MS, I find it very difficult to follow. Especially, I could not trace answers to my expectations from the title and abstract – probably because they became obscured by many detailed side-tracks and because some promised elements (like GPR data or elaboration on what are model errors) are not really followed. Maybe a fundamental revision and exhibition of the main story line could clarify most of the forthcoming points.

3. What is the reason to use own models, solvers and the LM least squares optimizer instead of established and tested toolboxes? Is it really matter of the MS to present the technical details and equations although they are not developed further, taken up or discussed later on? How can be assured that numerical errors in the code do not bias the results (see also Clark and Kavetski 2010, 10.1029/2009WR008896)? I can imagine that the details suit well as appendix and that an explanation of the concept and intention to use these tools can clarify much of my second concern.

4. Since heterogeneity is also an issue of scale and conceptual deficiency, I find the arguments not yet well drawn. What support of the TDR sensors is integrated by the measurements? How exactly are the estimated positions of the TDR sensors calculated and how precisely are the real positions known?

5. Since GPR data of the experiment appears to be existing (Klenk et al. 2015 under review in HESSD doi:10.5194/hessd-12-12215-2015) I do not understand, why it is not used for the study (although mentioned in the abstract and introduction)? I suppose that the TDR and GPR data could be a very valuable pair of observations to be compared directly (as both rely on the rel. electrical permittivity). The strong advantage of GPR as spatially continuous technique could be related to the local measurement with higher absolute precision of the TDRs.

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6. Figures 10 and 13 suggest to me, that the observations relate to the portion of the (sandy) retention curve which is rather linear (and that the strongly non-linear part is actually only of importance at low matric potential). How is a transfer of the found parameters to the full retention spectrum validated? Since the ASSESS site is an artificial, well-defined test bed I would assume that the actual retention properties are known and that local deviations are mainly due to differences in bulk density. Hence I could imagine that the authors could use fig. 11 in the methods section to explain their approach in much more detail and related to specific research hypotheses referring to the retention properties. At the moment, I find it very difficult to read figure 9 and 12 and to compare the 1D and 2D case.

Please find minor comments highlighted in the attached MS file. All the best, Conrad

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-109/hess-2017-109-SC1-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-109, 2017.

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