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

Stefan Jaumann and Kurt Roth

stefan.jaumann@iup.uni-heidelberg.de

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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.
Reply: We thank Conrad Jackisch for the constructive comments and suggestions. The manuscript was revised accordingly. Hence, we refer to the revised manuscript.

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 argumentation 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.*
Reply: The manuscript combines two main lines of thoughts: One is concerned with the estimation of hydraulic material properties on the basis of TDR measurement data acquired in a complicated subsurface architecture, which was forced with a fluctuating water table. We agree that this approach is not new and was applied already for many one-dimensional systems in the laboratory and also some in the field. The introduction cannot list all of the available literature. Rather it connects the manuscript to the related literature we deem most relevant for the manuscript. The other line of thought considers a general problem in modeling, namely the investigation, which physical processes and uncertainties have to be represented in order to describe the measurement data adequately. As the true behavior of the system of interest is unknown and since required adequacy depends on the application at hand, we choose to test different hypotheses (realized by increasingly complicated models) and analyze their results. We improved the introduction to better reflect these two lines.
The Richards equation is only valid where water and air phase decouple, i.e. at intermediate saturation. At high saturation, water– and air–flow become coupled and a two–phase formulation is required. Conversely, at low saturation, vapor transport in the air phase is no more negligible and at least a two–components model is required. Richards equation is a single–phase model.
The transfer of laboratory data to field situations is notoriously difficult. Major challenges are (i) bringing an undisturbed sample into the laboratory, (ii) representing structures that are larger than the sample. In our opinion, there thus cannot be such thing as an independent reference for a field site.

We assessed the precision from TDR data close to saturation and the accuracy with error propagation considering uncertainties in porosity and in bulk permittivity (Jaumann, 2012) yielding an uncertainty of 0.007 volumetric water content (Sect. A2.1). This result is of the same order as the evaluation of Roth et al., (1990, 10.1029/WR026i010p02267). A major point of critique of the Complex Refractive Index Model (CRIM) concerns that it is a physically-motivated and not a physically-based model (e.g., Brovelli and Cassiani, 2008, 10.1111/j.1365-2478.2008.00724.x). Additionally, other uncertainties such as the influence of the electrical conductivity on the evaluated water content and on the temperature model for the permittivity of water as well as the spatial distribution of the relative permittivity of the soil bulk are neglected in the manuscript.

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.
Reply: We revised the structure of the manuscript accordingly.

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.
Reply: The solver for the Richards equation (muPhi) is tested, published (Ippisch et

al., 2006, 10.1016/j.advwatres.2005.12.011), and it is, to the best of our knowledge, the numerically most efficient solver. The Levenberg-Marquardt algorithm was implemented according to published literature, because some of the required approaches are not implemented in available toolboxes.
We present only those technical details in the manuscript that are necessary to understand the evaluation procedure, such that the methods are traceable and reconstructible. The position of the methods section depends on the philosophy of the journal.
Due to the discretization of the problem in space and time, numerical errors are always existent, essentially balancing computational effort and numerical accuracy. We chose the grid resolution and meta–parameters given in the manuscript based on a grid convergence analysis.
We adjusted the structure of the manuscript accordingly.

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?
Reply: We clarified this issue in Sect. A1.4.
The support of the TDR sensors depends in particular on the sensor design and can be calculated (Robinson, 2003, 10.2136/vzj2003.4440). For the TDR sensors in ASSESS, the measurement volume contains a cylinder with a radius of approximately half the rod distance around the central rod in homogeneous electrical permittivity distribution.

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.*

Reply: Three single channel time–lapse GPR radargrams were acquired during the experiment and are currently evaluated for a separate publication. The measurement data presented in Klenk et al. (2015) were recorded during a different imbibition and drainage experiment. The main focus of this manuscript is to quantify the effect of unrepresented model errors on the soil hydraulic material properties and to find consistent description of TDR measurement data. These data are characterized by a point–scale measurement volume of the sensors, which is the main reason for the described effect of uncertainties concerning the sensor position and small-scale heterogeneity. Since such point–measurements are rather the rule then the exception in most large–scale studies, the related issues require critical consideration. A rather complementary analysis is required for the GPR data taking into account the larger measurement volume and GPR related representation errors. This would blow the limits of a single paper. Please also note the reply to comment 2 of RC1.

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.

Reply: A transfer of the results to the full retention spectrum can neither be made

nor validated with the available water content data and missing hydraulic potential measurements. We explained in the reply to comment 1, why no laboratory–based reference retention properties are known for ASSESS. We think that Fig. 9 is required for the discussion of the results and is best understood with a direct reference to the application. We improved the description, how to read these figures in Sect. 2.3.3 (Page 10, Line 18). Please also note the reply to the comment of RC1 concerning Fig. 7 (Fig. 9 in the unrevised manuscript).

Please find minor comments highlighted in the attached MS file.

Reply: We revised the manuscript considering these comments.

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