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Hydrology and Earth System Sciences Discussions

Interactive comment on "Field scale effective hydraulic parameterisation obtained from TDR time series and inverse modelling" *by* U. Wollschläger et al.

U. Wollschläger

ute.wollschlaeger@iup.uni-heidelberg.de

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Reply to the comments of Thomas Wöhling

We thank Thomas Wöhling for his thoughtful and constructive comments.

1) In essence, this paper uses a rather old approach to inverse analysis of soil moisture data, and does not offer new insights with respect to vadose zone flow modelling. [...]
Our contribution indeed does not add anything to the available family of inversion algorithms and it contributes little to the art of vadose zone modelling. What it does,

however, is to provide a new and practicable approach for obtaining field-scale parameters from in-situ measured data and it demonstrates it for the highly nonlinear regime encountered in most natural settings. This in turn opens the way to more realistic and theoretically sound approaches to vadose zone modelling (please also refer to response to issue 3) raised by G. de Rooij and issue 1) of S. Iden). It furthermore demonstrates that indiscriminate application of evapotranspiration schemes may lead to severe misrepresentations of reality.

2) In summary: The reviewer suggests the application of global algorithms like SCE-UA, Particle Swarm Optimization, Differential Evolution, or all of them together in a self-adaptive multi-method approach such as AMALGAM in order to better find the global minimum in the parameter space.
Please see reply to item 1).

3) In summary: The reviewer recommends to address parameter and boundary uncertainty with Markov Chain Monte Carlo methods such as the Shuffled Complex Evolution Metropolis (SCEM-UA), DiffeRential Evolution Adaptive Metropolis (DREAM) algorithm, recursive implementation (Sequential Monte Carlo), etc.
We agree that various approaches exist to assess the uncertainty of the parameter estimates. We point out, however, that with respect to the description of reality, there is also the modelling uncertainty which cannot be assessed so far. In this situation, we prefer the comparison of modelled and measured phenomena rather than partial analysis of parameter uncertainties.

4) In summary: Using a water table at -4 m depth at the lower model boundary in combination with an additional soil layer with high saturated hydraulic conductivity instead of using a free drainage boundary condition.
Various choices for the lower boundary condition are popular: constant head, free

drainage, and extending the profile to an imposed water table. All of them have specific advantages and disadvantages, none of them is optimal for all situations. Fixed head is the condition of choice if corresponding measurements are available. In all other cases, it typically leads to an artificial sink or source at the lower boundary. Free drainage is best for deep profiles with an even deeper water table. It will never introduce additional water into the flow domain. Artificially extending the profile to a greater depth and imposing a fixed water table there is a heuristic alternative between fixed head and free drainage. It allows to integrate auxiliary information about the architecture of deeper layers and to account for their possible (weak) impact. We chose a deep groundwater table to include the possible effects of the strong textural transition at 1.55 m depth into the model. A capillary barrier could induce both upward and downward fluxes in the deeper section of the soil profile and, since the information is available, we wanted to account for this effect in the model as well. The reviewer is right with the statement that our boundary condition may create artefacts since the saturated conductivity of the lowest model layer influences the flux across the capillary barrier and consequently this indeed also has an effect on the crop factor which is calculated in the inversion. However, a free drainage boundary condition here would not solve the problem since the flux across the lower boundary is determined by the inverted hydraulic conductivities of the different model layers and the flux across the upper model boundary. We will add a discussion on this topic in a revised version of the paper.

5) Different rooting depths are considered in the analysis, which begs the question why the rooting depth was not considered to be a calibration parameter?
We did not include rooting depth as an additional calibration parameter since those model runs with different rooting depths presented in the paper did not show a strong influence on the inversion results (principle of parsimony).

6) Please note, that initial conditions in HYDRUS-1D can be specified using (measured) volumetric water content directly and the transformation in pressure head via the water retention curve is not necessary for model state initialization.
We are aware that water contents can be used directly to set the initial condition in HYDRUS-1D. However, in contrast to pressure head, water content is not continuous across layer boundaries, hence should not be interpolated. For the layered soils at our site, we were thus forced to go to the more complicated iterative procedure.

7) [...] The uniform flow Mualem-van Genuchten model is probably not the best choice to describe such processes. [...]
The choice of MvG was a model decision among a number of different parameterisations available. A dual-porosity model would also be an option, however, depending on details of the formulation, the dimension of the parameter space would increase considerably.

8) The calibrated model should be tested against independent data which was not used in the calibration.
Please refer to our response to issue 2) raised by G. de Rooij.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1489, 2009.