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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.

A. Coppola (Referee)

antonio.coppola@unibas.it

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The authors present the results of a study for inverse estimation of field hydraulic properties based on a time series of water contents measured at different depths. To my opinion, there are some problems which should be resolved before the article can be published. The authors present an inversion procedure coupling the HYDRUS 1D software for solving the direct problem and a MATLAB version of a trust-region solver, which should allow to partially avoid the problem of ending the inversion procedure on local minima. The work is not novel as inverse techniques are today routinely applied. Inverse techniques are attractive as they allow to use all the information one has on

a flow process to hydraulically characterize a soil. Anyway, the authors are certainly aware that this is true only if the data contain sufficient information and the inversion is appropriately designed for avoiding unicity and ill-posedness problems. What's more, the estimates goodness evaluation based only on the interpretation of the calibration data set can be largely misleading. In this sense, it is my opinion that the paper exhibits some conceptual inadequacies in determining hydraulic parameters. The critical points in the paper that I would like to discuss are the following:

1. In the title and in the introduction (pg. 1491 lines 24-26 and pg. 1492 lines 1-3) the authors refer to effective hydraulic properties and state that these can be hardly achieved from lab measurements (see also page 1507, lines 16-18). In my mind the concept of effective properties is more related to soil heterogeneity. Effective properties would be ensemble-averaged properties and should be invoked only for upscaling problems. To the contrary, it is my opinion that what the authors are speaking about is just a problem of transferring lab properties to field properties, as they are only monitoring water contents along one vertical soil profile. To me, this remain a pure physical-deterministic problem and has nothing to do with heterogeneity. These are not philosophical arguments and below I will try to demonstrate that they have direct implications on the results presented in the paper.

Basile et al. (2003; 2006) provided experimental evidences (a comparison between soil water retention and hydraulic conductivity curves as measured in the field and in the laboratory on different soil profiles) that differences in procedures used in laboratory and field experiments may actually cause hydraulic functions to be different, as a consequence of different hysteretic paths being measured. Using a hysteresis schematization, the authors provided a theoretical formalization and quantification of such differences, in a way that a mutually conversion of field and laboratory hydraulic properties could be achieved. For all the horizons the water retention values were always higher for the laboratory curves. The authors ascribed such behavior to the nearly complete saturation (i.e., a higher fraction of air removed during wetting) that could be achieved in the laboratory. The lab and the field parameter vectors differed only in parameter $\theta 0$ and α,both being always higher for the laboratory. Larger α for the laboratory are expected given that the larger pores, nearly all filled with water in the laboratory tests, empty at relatively lower pressure head values, h. For the same reason, lower $\theta 0$ and α in the field were always accompanied by lower K0. Of course, different lab and field curves imply a different evolution of wetting and drying processes in the lab and field. The main consequences of this discussion are the following: - The laboratory hydraulic curves can always be translated in the corresponding curves to be used in the field; - It is not correct to use the porosity as an estimate of the field $\theta 0$ (here I intentionally refer to $\theta 0$ and not to θs just for indicating satiated and not saturated water contents), especially if soil wetting occurs under natural conditions. If this is done, one is using a sort of laboratory curves for describing the field behavior. In order to reproduce the measured data in an inversion procedure like that used by the authors, this constrain the parameters α and K0 to assume anomalous values which are not the field parameters nor the lab ones. The discrepancies between simulated and measured water contents in the April-May and August-September periods could well be explained this way. In this context, invoking the errors in the evapotranspiration for explaining the discrepancies seems only an artifice without any physical basis;

2. It seems from the text that the authors estimated 16 parameters simultaneously, at least in the first stage. In an attempt to improve the fitting, the parameters became 20 (by adding the crop factor ïĄń). With 20 parameters the reference evapotranspiration is reduced by more than one third respect to the calculated one. Apparently, it is loosing any physical meaning (see also point 1). The uncertainties in the hydraulic parameters are unloaded on the evapotranspiration. In this context, I am wondering if the statement (pg. 1506, lines 17-23) “Since the observed water contents are fitted reasonably well . . ..., the estimated average water flow is expected to be correct for the analysed time period” is justified. The fitting improvement seems more a result of the large parameter number and no physical meaning can be attributed to the parameters. In this context, the goodness of parameter estimations cannot be evaluated simply by

the RMSE. It would have been interesting to have a look to the parameter correlation matrix. Besides, the problems arising from the large parameter number are recognized by the same authors when they state (pg. 1504, lines 3-7) Since the simulations . . ..it is obvious that our model is not able to find a global minimum in the parameter space. We attribute this to the high dimensionality of the problem and the occurrence of local minima. . .... Also, it seems that even using a trust-region solver the problem of avoiding the local minima in the inversion procedure is not solved. Abbaspour et al. (1999), gave guidelines to make parameter estimation of multilayered profiles feasible and accurate. The parameters were determined separately for each horizon of the profile. First, the parameters for the topsoil were estimated on the basis of the water contents measured at different depths in the first layer, while the pressure head measurements at the bottom of the same layer were used as its bottom boundary. Then these parameters were treated as known and those for the second layer were estimated in a similar way on the basis of the water contents and pressure heads in the same layer. Coppola et al. (2004) found this approach the only feasible to find reliable hydraulic parameters in multilayered soils.

3. At pg. 1504, lines 14-16, the authors state “The success of the inversion . . ..predominantly depends on the choice of the parameter set taken for initializing the model”. . . .the inferred parameter sets are not equal if different initial parameter guesses are used. At pg. 1505, lines 9-10 they state “this also shows the strong influence of ïĄń on the absolute fluxes entering the root zone of the soil profile”. It is worth noting that the sensitivity to the crop factor is evaluated by fitting the calculated surface water contents to those measured in a different soil profile, thus assuming that the water content dynamic does not change in space. In synthesis: - the authors are optimizing the hydraulic parameters of the different layers; - they are also optimizing the upper boundary condition (the evapotranspiration). - also, by imposing (not measuring) the properties of the fifth layer in the soil profile, they are also choosing a sort of bottom boundary condition, in the sense that its behavior can control the water content evolution in the upper layers. I am wondering how, in this framework, the authors can

establish how the fitting depends on the evapotranspiration, on the estimated hydraulic parameters, on the choice of initial guesses for hydraulic parameters, on the choice of the parameters of the fifth layer, on the spatial variability of the surface water content. Accordingly, I cannot agree with the statement (pg. 1504, lines 26-29) “the calculated cumulative fluxes across the upper boundary and through the root zone of the model are very robust”. Also, the statement (pg. 1506, lines 17-23) “Since the observed water contents are fitted reasonably well . . ..., the estimated average water flow is expected to be correct for the analysed time period” seems speculative because it assumes the reliability of the evapotranspiration and root zone fluxes.

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