

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

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Reply to the comments of Antonio Coppola

We thank Antonio Coppola for his thoughtful and constructive comments.

1a) In the title and in the introduction the authors refer to effective hydraulic properties and state that these can be hardly achieved from lab measurements. [...]

The phenomenology of hierarchical heterogeneous media may be studied at different spatial and temporal scales with the same conceptual frameworks often invoked at the

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different scales. A prominent example for terrestrial systems, which as far as we know also introduced the notion “effective” vs “apparent”, is solute transport in groundwater where the central limit theorem ascertains that for sufficiently long transport distances through sufficiently uniform media the transition probability density is a Gaussian. This leads to the convection-dispersion model with the coefficient, or tensor, of dispersion as a key parameter. While the actual processes that lead to dispersion are different at different scales, the formal descriptions are identical and the names of the parameters are chosen identical. However, their values are different at different scales and the relations between them may be quite complicated, certainly not linear. Such parameters are called “effective” if it has been ascertained that the underlying process is what it is supposed to be, convection-dispersion in this case. It is called “apparent” when this is not the case, as for instance for near-field transport. Following this terminology, we indeed should have called our parameters “apparent” since they are used in the Richards equation with both, the equation and its Mualem-van Genuchten parameterisation presumed to be correct without actually ascertaining it. The revised manuscript will be modified accordingly.

1b) In summary: Using the hysteresis approach of Basile et al. (2003, 2006) to transfer laboratory measurements to the field. “The laboratory hydraulic curves can always be translated in the corresponding curves to be used in the field.”

For a uniform medium, we agree with the reviewer that including hysteresis would probably improve the transfer of laboratory measurements to the field. However, we identify the multi-scale architecture of natural soils as a much more severe problem for transferring parameters from the lab to the field. Such a transfer is only feasible if an REV exists – a lack of structures with length scales in a sufficiently large interval – and if the sample taken is of the size of this REV. To our best knowledge, assertion of this situation lacks in all corresponding attempts reported in the literature so far.

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1c) *It is not correct to use the porosity as an estimate of the field θ_0 [...]*

Hydraulic processes described by Richards' equation operate between a minimum water content, often called θ_r , and an upper water content, often called θ_s . They are blind with respect to absolute values of θ_r and θ_s , only the difference $\theta_s - \theta_r$ matters. In this respect, it would appear to be appropriate to use $\Delta\theta$ as a parameter rather than the two individual values. Tradition has it the other way round, presumably due to the believe that θ_r and θ_s have some physical meaning. That indeed becomes relevant once solute transport is considered, which opens a wholly new bag of issues, however. The choice of θ_r or θ_s as a fixed parameter, and estimating the other one, is a matter of taste. We chose the upper limit as fixed.

2a) *It seems from the text that the authors estimated 16 parameters simultaneously [...]*
The uncertainties in the hydraulic parameters are unloaded in the evapotranspiration.
[...] The fitting improvement seems more a result of the large parameter number and no physical meaning can be attributed to the parameters.

We agree with the reviewer that optimisation of a large number of parameters simultaneously always implies the problem of non-uniqueness and parameter identifiability of the model. However, we do not think that our model loses its physical meaning by enhancing the parameter space by one additional parameter when invoking the crop factor. As described in section 2.2.3 of our manuscript, the reference evapotranspiration, as it is calculated by the FAO Penman-Monteith formula, is basically a climatic parameter that is calculated for a specific reference crop and which needs not necessarily represent potential evapotranspiration which is needed by HYDRUS in order to calculate the evapotranspiration fluxes. To account for differences between the specific crop, the FAO recommends to scale these fluxes by using a crop factor. In our case, we determined this factor during the inversion. In fact, the application of the crop factor is indeed necessary when using the FAO Penman-Monteith formula for determination of ET. If this is neglected, the evapotranspiration fluxes – except the amount which is removed by the root water uptake function at low matric potentials –

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will always be the one for the reference crop with its own specific properties and these may be very different in case of other specific crops. If the boundary fluxes are biased, this, of course, may also have a huge influence of the soil water balance. Since the FAO approach is currently applied in many model studies we want to emphasise that accounting for the crop factor in the calculation of the crop ET may lead to a strong improvement of the inversion results as it was shown in our study.

At first sight, the total number of estimated parameters appears rather large. However, for each layer only a set of 4 values is used and there is a long time-series of data for – at least in the upper layers – strongly fluctuating states available for each of these sets. Hence, the situation is more akin to parameter estimation from multi-step lab experiments with their solid data base than to estimating highly parametrised hydrologic models from rather scarce data.

2b) 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. [...] We did inspect the correlation matrices of the various inversion runs. Very few of these contained correlation coefficients > 0.9 : In those seven runs leading to an RMSE < 0.013 , five runs showed just one such parameter pair, none of them including the crop coefficient. In three cases the values of θ_r and α of model layer 4 were correlated (correlation coefficients: 0.96, 0.997, 0.97), one simulation showed a correlation coefficient of 0.91 between θ_r and n of model layer 2, and in the fifth case the values of n from model layers 1 and 4 were correlated (correlation coefficient: -0.99).

3) [...] 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. [...] I am wondering how, in this framework, the authors can establish how the fitting depends on

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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. [...]

Concerning the application of water contents measured in a nearby soil profile for the upper model layer the reviewer is right that this could be a problem due to the spatial heterogeneity in the hydraulic properties of the soil. However, recent ground-penetrating radar groundwave measurements from the Grenzhof Test Site (A. Lodde (2009), unpublished data) show almost constant near-surface water contents along several tens of meters. Hence, for our site we are very confident that the water contents from the nearby profile are applicable to be used in our model. We will add a comment on this in a revised version of the paper.

Regarding the application of the crop factor and the choice for the lower boundary condition please refer to our discussions in issue 2a) above and issue 4) raised by Th. Wöhling. In addition, variation of the initial condition and inverting the hydraulic properties alone without crop factor did not lead to an improvement of the general evolution of the measured water contents. Please, refer to Fig. 5 of the article and the related discussion. Introducing the crop factor led to a significant improvement of the fit. In addition, this appears justified when the FAO Penman-Monteith equation is applied. The reviewer is right that the estimated fluxes include of course the uncertainties resulting from the applied boundary conditions and we will indicate this in our discussion accordingly.

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