**Hydrology and
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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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Review of 'Field scale effective hydraulic parameterization obtained from TDR tme series and inverse modelling' by U. Wollschläger et al.

This is a well-written contribution that attempts to model field-scale water fluxes between the atmosphere, the root zone, and the deeper soil directly, i.e. without resorting to integrating or averaging fluxes obtained at smaller scales.

Major comments

The fact that your vegetation cover closely resembled the reference crop helped to re-

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duce the parameter space. Can you discuss (by informed speculation if necessary) the possibilities and limitations you envision when you want to apply your parameterization method to soils with another crop, or with a mixed (semi-)natural vegetation? Specifically, can you say anything about the relative importance of the soil and the vegetation parameters for the response (the actual ET) of the soil-vegetation system to the atmospheric forcing at the field scale?

The absence of a validation on independent data (not used for calibration) is regretful. Is there any way to remedy that?

The discussion can delve deeper into the sensitivity of various field-scale fluxes to the various parameters under different circumstances. Also, you are intending to use Richard's equation outside its range of validity. I therefore believe that the parameters are indeed effective, yet you calibrate on very real local-scale water contents. You may want to look into Feddes et al. (1993), which is a numerical study with a set-up and an objective that is very similar to those of this paper. The numerical nature of that study allowed a more elaborate discussion of the value of individual observations. (Feddes, R.A. et al., 1993. Estimation of regional effective soil hydraulic parameters by inverse modeling. In: D. Russo and G. Dagan (eds.) Water flow and solute transport in soils. Developments and applications. In memoriam Eshel Bresler (1930-1991), pp. 211-231. Springer, Berlin).

You focus on field scale flows, and yet you work with only two profiles with TDR sensors that are located close to one another. This needs some justification, particularly since you devote so much attention to the observed and simulated moisture contents.

Minor comments

p. 1496, l. 5 and other occurrences: parameter alpha is not the reciprocal of the air-entry value, although it is related to it. The vG retention curve has no well-defined air-entry value. Perhaps not directly relevant to the paper but useful to support this comment: de Rooij, G.H., and H. Cho (1999; Hydrological Sciences Journal 44:447-

465) developed an approximate expression for the air-entry value based on the vG parameters that contains both n and alpha. Furthermore, Liu, H.H. and J.H. Dane, 1995 (Soil Sci. Soc. Am. J. 59:1520-1524) argued that a Brooks-Corey soil (with a well-defined air-entry value) can lead to an apparent vG-type water release curve through the effect of the sample height on the range of matric potentials in the soil sample.

p. 1498, l. 8-10. Why did you not impose a matric potential of – 6 m H2O at 4 m depth?

p. 1501, l. 4-11: Is parameter optimization on one TDR probe at each depth consistent with your desire to determine field-scale soil hydraulic parameters? (Relates to one of the major comments.)

p. 1502. l.2: I noted some details in Fig. 2 that may be of interest to discuss here: Days with low reference ET were often, but not always rainy. The envelope over the peak ET values is a smooth curve representing the energy-limited (potential) ET that can be achieved on cloud-free days. The reference ET can be much lower than that value under less favorable circumstances (as in early June and late September).

p. 1502, l. 6-9: Probably the most sensible thing to do.

p. 1503, l. 16-18: The UN has good guidelines and crop factors (reports by WMO and FAO). Not necessary for this work, but may be useful for future work along these lines.

p. 1504: l. 14-16: Is there potential relief by using more powerful optimization algorithms?

l. 20-28: This suggests alpha does not matter too much. Correct?

p. 1505, l. 1: What about the effect on the various fluxes? That's more relevant. This also ties in with the emphasis on point scale data while you are more interested in the field scale.

p. 1505, l. 17 to the end of the paragraph. It is sometimes hard to follow the line

of reasoning. The behavior of the simulated shallow water content and the simulated evapotranspiration are consistent; this seems to preclude a preferential flow effect that could limit the transfer of water from wet areas to dry areas in the field (e.g., because the dry parts are water-repellent). Yet, later on you mention preferential flow. Further down, you discuss the trends in the water contents at larger depths. The soil there dried more than the model simulated. I noted that the drying already starts in May at both depths and continues through the summer, without much effects from rain. Could this be caused by a water uptake by roots that had penetrated to that depth during spring and summer growth? It would be interesting to see if the matric potential at 0.63 and 0.92 m indicated (near-) hydrostatic equilibrium; than the water could not have moved up through the matrix, and roots end up as the main suspects. Finally, an argument against preferential flow is the absence of a response in the deeper water contents to rainfall. But since you have only a few TDR probes, this is not conclusive.

p. 1506, section 3.3: Can you reflect on the relative importance of the soil and the vegetation (in more general terms: soil properties versus land use) for the various fluxes during wet and dry conditions?

p. 1506, l. 15-17: adding a validation on independent data would provide additional justification.

Figure 3. Given the trend between -1 and -1.2 m depth, it seems more realistic to extrapolate the pressure head profile towards h = approx. -2.3 m at 1.5 m depth; this would also eliminate the non-physical discontinuity at that depth. But if it takes too much effort to change that, never mind; the spin-up period presumably took care of it.

Comments regarding the presentation

If you refer to a book, please indicate which pages you refer to.

I would like to have the figures a bit larger.

Write out 'laboratory' in full, instead of 'lab'. (p. 1492, l. 2 and elsewhere)

p. 1490, l. 23: parameterization -> values

p. 1492, l. 23: content -> contents

l. 26: ...referred this result to the large amount... -> attributed this to the large number...

l. 28-29: In a recent article... -> Recently, ...

p. 1493, l. 4: which -> that

l. 9: Should 'quasi-linear' be 'quasi-steady state'?

p. 1494, l. 9: At which height were the wind speed and other height-dependent variables measured?

p. 1498, l. 4-5: I do not follow this sentence.

p. 1501, l. 14-21: This belongs in Materials and Methods.

l. 21: can be observed at-> were recorded by

l. 24: insert comma after probe; during the -> in

l. 25: delete 'months'.

p. 1501, l. 22 – p. 1502, l. 2: This is all quite obvious, simply refer to Fig. 2 and focus on the interesting material.

p. 1503, l.5: flat site -> essentially level experimental field

l. 9-10: surface with specific...reference surface -> that differs from the reference crop

l. 18: delete 'or'; to name some -> etc.

p. 1504, l. 4: delete 'first'.

l. 14 to the end of the section: I find myself reading back and forth to assemble the information. Please try to enhance the structure of the discussion. One suggestion I

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would like to offer is to separate various features in the model output: evaporation flux, transpiration flux, downward flux from the root zone, water contents at depths of interest, water content profiles, storage in the unsaturated zone. For each of these (or those you consider relevant), discuss how well it can be modeled, and which parameters are the most important for that feature.

p. 1505, l. 26: slower -> more slowly

p. 1509, l. 23: add space before publication year.

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

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