

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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Received and published: 26 May 2009

Reply to the comments of Gerrit de Rooij

We thank Gerrit de Rooij for his thoughtful and constructive comments.

Major comments:

1) *The fact that your vegetation cover closely resembled the reference crop helped to reduce the parameter space. [...]*

Generally, inverting the crop factor is also feasible for other crops or – as an average

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value – for mixed vegetations. In our case, the conditions are rather simple since the grass is completely developed and generally kept at a certain height, hence we typically do not have to account for different growing stages. However, even in such a simple situation one has to consider the actual evapotranspiration to differ from the one prescribed by the type curve, e.g., due to differences in crop height, albedo, surface resistance or evaporation from the soil surface (Allen et al., 1998).

When dealing with developing crops, the temporal change of the crop factor would become even more important. Guidelines how to handle this are for instance provided by the FAO (Allen et al. (1998)). In principle, a temporally changing crop factor could also be obtained from inverting appropriate data that would have to refer to the physiological state of the plants. Unfortunately, such data are not available at our site. Hence, we cannot focus on a detailed representation of the soil-atmosphere coupling. We still emphasise, however, that when using the FAO Penman-Monteith reference evapotranspiration, the crop factor, also when considered as a constant, should be estimated from the hydraulic state variables. The reason for this is that the average water flux across the upper boundary is of fundamental importance for the soil water balance and the hydraulic state. Any offset, whatever its origin, leads to characteristic deviations between measured and inverted quantities as is illustrated in Fig. 5. We comment that this issue is neglected in many modelling exercises. We will add a discussion on this in a revised version of the paper.

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

The often-used approach of calibrating with one time interval and validating with another one checks (i) the stationarity of the system and (ii) the stability of the parameters. The former is an important and often neglected consideration of its own, but it is not at the focus in this work. The latter is relevant when only short data sets are available for calibration. We believe that this is not the case here, however, as the system passes through a number of wetting-drying cycles.

The correct approach to validate a model with respect to a given data set are appropriate statistical tests that explicitly account for the uncertainty of the data. An example is the χ^2 -test which assesses the probability that the data originate from a process represented by the model with the given parameterisation. This test is only feasible, however, if the model represents all the involved processes, at least all those whose impact on observed quantities is larger than the measurement uncertainty. This is certainly not the case here. Obvious aspects that are not represented in the model include (i) preferential flow, (ii) hysteresis of soil hydraulic properties due to wetting/drying, shrinking/swelling, or wettability, (iii) three-dimensionality and microscopic heterogeneity, and (iv) dynamics of the plant cover.

Hence, the approach to validation is judgement of characteristic deviations or agreement. Obviously, this depends on the questions that are envisaged to be going to be addressed with the model and it is not objective. This, however, is the current state of most, if not all, environmental modelling studies since (i) some relevant processes are not known, not understood, or not parameterised in a useful way and (ii) the database is not sufficient to unequivocally resolve the subprocesses. Typically, modelling efforts suffer from both problems simultaneously and no amount of statistical sophistication can dissolve these fundamental challenges.

3) 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. [...]. You focus on field scale flows, and yet you work with only two profiles with TDR sensors that are located close to one another. [...]

The attribute “field-scale” is used to indicate (i) direct determination in the field, as opposed to lab measurements on an extracted sample, and (ii) relevance of the estimated values at a scale that is appropriate to simulate water flow at the field scale. The second point does not imply that the parameters obtained from essentially point-measurements in a single profile can be used to calculate the water flow in an

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entire field with extents of hundreds of meters or even kilometers. The well-known heterogeneity of natural soils impedes this. The correct approach for this case appears to be to run many of such profiles, which is feasible due to the low cost of installation and operation, thus obtain the parameter field with the desired extent and resolution, and to run a corresponding high-resolution, three-dimensional numerical simulation. The sometimes proposed “field-averaged soil hydraulic material properties” is for a heterogeneous field, in our view, an invalid concept since the very definition of material properties demands local equilibrium at the scale of representation. This, in general, cannot be ascertained for the Richards equation under typical atmospheric forcing (e.g., Roth, 2008). We will add a paragraph concerning the field-scale application in a revised manuscript. Concerning the use of “effective” properties please refer to our reply of issue 1a) raised by A. Coppola.

Minor comments:

4) p. 1496, l. 5 and other occurrences: *parameter alpha is not the reciprocal of the air-entry value [...]*

We will rephrase the text to “ α [m^{-1}] and n [-] are shape parameters” and remove the term “inverse of air entry value” from the text.

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

We expect that a matric head of -6 m at 4 m depth could have been used as well for the lower boundary condition and that the results would not have been significantly different. With the water table some $-6 \text{ m}/\alpha \approx 87$ scale-heights away and the saturated hydraulic conductivity orders of magnitude higher than the mean water flux, the influence of the water table is deemed negligible for all practical purposes.

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6) p. 1501, l. 4-11: *Is parameter optimization on one TDR probe at each depth consistent to your desire to determine field-scale soil hydraulic parameters?*

Please see reply to item 3 above.

7) 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 favourable circumstances (as in early June and late September).*

The observation of the reviewer is correct that a potentially higher reference ET on rainy days should have caused lower simulated water contents at the uppermost TDR probe which would lead to a better fit of measured and simulated data, especially in late May. However, the calculated rather low reference ET is reasonable for rainy weather conditions. We can only speculate about the reason for the deviation between measured and simulated water contents. One reason may be an enhanced evaporation from the wet soil surface after single rain events as suggested by Allen et al. (1998) which is not accounted for in our single crop factor approach. This would remove a certain amount of water before entering the soil profile. In addition, the grass may have been withered, especially after long droughts, which may also have influenced the crop properties.

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

Yes, we believe so in the sense that a global optimisation algorithm would potentially yield the single global minimum. In contrast, our approach reveals several minima. However, for those solutions with acceptable RMSE, we find that the corresponding fluxes (not fitted) cluster in a very narrow band. In addition, computational resources are not a limiting factor for the type of small problem we are studying.

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9) p. 1504, l. 20-28: *This suggests that alpha does not matter too much. Correct?*
In this case, yes.

10) 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.*

Please see response to items 8) and 3).

11) p. 1505, l. 17 to the end of the paragraph: *It is sometimes hard to follow the line of reasoning. [...]*

From the different reviews we realised that the discussion of the deviations in measured and simulated water contents is far too short and we will extend it in a revised version of the manuscript. The observed deviations in our very simple model may originate from a number of factors and we can only speculate on the true reasons. Focusing here only on processes, for the strong deviations at the uppermost TDR probe after wetting events we suspect evaporation from the bare soil surface directly after wetting events to be the main reason (see also item 1) above). The large deviation at 0.63 m and 0.92 m during August could indeed originate from root water uptake since the grass during this time was not mowed and had grown to a height of about 0.3 m. Here we suspect a change in plant water requirement to be the main reason for the higher simulated water contents. During October the calculated residuals between measured and simulated water contents at 0.63 m and 0.92 m show a peak after two distinct rain events when the soil at these depths is already pre-wetted after the longer rainy period during August/September. This may indicate preferential flow since the wetting at these depths occurs faster than simulated by the model. Evapotranspiration is already rather low during this time.

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

From our study we can identify two different issues for accurately modelling field-scale soil water dynamics. One is the correct determination of soil hydraulic properties which is addressed by many modelling studies. The other is the correct representation of the upper boundary condition which has a very strong influence on the soil water balance. Here, of course, land use and climate play an important role since plant cover and meteorology determine evapotranspiration. We will add a comment on this in a revised version of the paper.

13) p. 1506, l. 15-17: *Adding a validation on independent data would provide additional justification.*

Please see reply to item 2).

14) *Figure 3:*

We accidentally used the initial condition calculated for the last inversion loop step of the inverse simulations to generate Figure 3 instead of the real initial pressure head profile. We will replace the figure with the right one in a revised version of the manuscript. The reviewer is right that this initial condition as it is accidentally displayed for the last inversion loop is non-physical. The spin-up period was indeed expected to take care of it.

Technical comments regarding the presentation

We will rephrase the text in a revised version of the paper according to the suggestions of the reviewer and also make sure that the figures are large enough.

p. 1494, l. 9: We will add the sensor heights and a note on the correction of wind speed data for calculation of evapotranspiration fluxes.

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p. 1498, l. 4-5: We will remove the sentence from this section as it is explained in more detail in section 2.2.4.

p. 1501, l. 14-21: We will move this paragraph to the end of section 2.2.4.

p. 1501, l. 22 – p. 1502, l. 2: We will reduce the content of the paragraph to the interesting material.

p. 1504, l. 14: Also with respect to the comments of the other reviewers we will rewrite the final part of the discussion and consider the relevance of the modelling strategy and the resulting fluxes.

References

Allen, R. G., Pereira, L. S., Raes, D., and Smith, M. (1998): Crop-evapotranspiration (guidelines for computing crop water requirements), FAO Irrigation and drainage paper, No. 56, FAO – Food and Agriculture Organization of the United Nations.

Roth, K. (2008): Scaling of water flow through porous media and soils. *EJSS*, 59, 125-130.

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

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