

Interactive comment on “Functional test of pedotransfer functions to predict water flow and solute transport with the dual-permeability model MACRO” by J. Moeys et al.

Anonymous Referee #1

Received and published: 29 March 2012

General comments This paper presents a test of pedotransfer functions for predicting water flow and solute transport for a model with preferential flow. The development of pedotransfer functions for preferential flow models is an important issue and not many studies are available. For this reason, publication of this paper in HESS is justified. However, there are some concerns which need to be addressed before the paper can be published. My most important concern is about the calibration procedure used to compare the blind simulations with simulation with calibrated model parameters. The authors have not used a comprehensive calibration procedure, but instead used a limited calibration procedure in which first the water flow component and then the solute transport model was calibrated. The calibration parameters chosen appear

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to be quite arbitrary, for example the water uptake and anion exclusion factors are calibrated to mimic faster transport in the soil matrix. An increase of the dispersion length would have been a more appropriate choice, and would probably have given dispersion lengths that are more in line with the median value of 5 cm as suggested by Vanderborght and Vereecken themselves. I would suggest carrying out a more detailed calibration exercise for a subset of lysimeters to test if this limited procedure is adequate. Another concern pertains to the modelling experiment. The authors have selected a zero tension lower boundary condition for the water flow model. This is not an appropriate boundary condition for plant-covered lysimeters. The soil will often be unsaturated throughout, and in this case the zero tension boundary condition will give completely wrong modelling results. I suggest redoing the simulation with a free drainage boundary condition, in which the outflow stops if the soil is unsaturated (a lysimeters boundary condition). In some cases, the pedotransfer functions appear to be unnecessarily simple, giving only four classes. As an alternative, Jarvis et al. (2007) presented a continuous pedotransfer function for the effective diffusion path length based on organic matter and clay content. The authors should justify why this more sophisticated pedotransfer function was not used in this study and how these two estimation procedures relate to each other.

Specific comments Page 2246 – line 15 and 21: I don't agree with the statement that water flow is reasonably well simulated. A modelling efficiency of 46% does not justify this. Page 2251 – line 8: There is no relation between the organic carbon content mentioned in the text and the values presented in table 2. The median value is lower than the lowest value in the table! Please replace T/m³ by kg dm⁻³. Page 2252 – line 5: please give the model version here, and discard it at page 2253 Page 2253 – line 6: pH water, pH CaCl₂ or pH KCL? Page 2253 – line 15: It is not clear from the text how the entire $\theta(h)$ relationship is build. The authors describe the estimation procedure for the saturated water content and the water content at wilting point, but not for the water contents in between. Page 2254 – line 8: Is there an explanation for the systematic difference of the data from the three authors? Page 2255 – line 16: As this relates to

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a crop, this is not a PTF Page 2256 – line 10: The choice for the 3.4 cm needs to be justified in more detail. According to the author of this paper, not all experiments with high flow rates exhibited preferential flow, so eliminating these experiments is rather arbitrarily. As mentioned in the general remarks, a higher value of the dispersion length would be more logical. Page 2260 – line 25: A perennial crop does not automatically imply that the Leaf Area Index is constant in time. Probably in FOOTPRINT, but not in reality. And a seasonal course of the LAI is also normal in temperate climates, so not limited to Nordic countries. Page 2274 - table 4: is the effective diffusion pathlength also set to 3 mm for macropore flow class I?

Technical comments The quality of the figures is not acceptable for publication in HESS, please improve the quality of these figures.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2245, 2012.

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