Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-168-AC3, 2016

Hydrology and Earth System Sciences Discussions
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Interactive comment on "Parametric soil water retention models: a critical evaluation of expressions for the full moisture range" *by* R. Madi et al.

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Received and published: 4 August 2016

Reply to referee 3

Gerrit H. de Rooij, Raneem Madi, Henrike Mielenz, and Juliane Mai

Reviewer 3 gives a good summary of our work and its significance, but states that our literature review has some omissions. The reviewer gracefully provides these missing papers, and we gladly will incorporate these in the text once we studied them. Reviewer 3 more strongly than reviewer 1 criticizes the fact that we used only four soils to test the data on. This may in part be related to the fact that s/he places an emphasis on the behaviour near saturation, while we are equally interested in the very dry range,

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Discussion paper

where, as reviewer 1 correctly noted, there are fewer data sets that cover that range.

Furthermore, the reviewer mentions two papers in addition to those we already referred to that tested more parameterizations over a wider range of soils, and many of the parameterizations we cover were included in those tests. As we point out in the text, these tests looked at various goodness-of-fit criteria of the retention and the conductivity curve when compared directly to data, but did not evaluate their importance in terms of calculated fluxes when implemented in a Richards' solver. This is an element of our work that contributes something that has not been done to this extent before. We only found something similar in Coppola et al. (2009) (reference provided by this reviewer), for a single field, wet conditions (mainly downward flow) and a short time period (10 days). The focus of that work was different, as it also accounted for field-scale heterogeneity of the soil hydraulic parameters, which we do not consider at this point. Still, our model test covers four soils instead of one, offers a more comprehensive set of atmospheric forcings for a much longer period of time (nearly 3 years!), and tests a much wider array of combinations of retention and conductivity curves. We believe this aspect of our work to be a bit underrated by the reviewer.

2. (There is no comment numbered 1). Like reviewer 1, reviewer 3 point out that we only consider unimodal soils. In response to the comments of these reviewers we will add a section to the Introduction discussing these models in more detail. The references provided by reviewer 3 will be of considerable help in this. An initial reading of these papers revealed that parameter correlation is a point of concern for such models, and detailed and accurate measurements near saturation are of utmost importance. Since our work is done within the framework of groundwater recharge in semi-arid areas that are notoriously data-scarce we believe we should leave the intricacies of multimodal retention curves alone at this stage of the work, and limit ourselves to unimodal retention curves for the time being. This choice notwithstanding, we will endeavour to rewrite the paper in such a way that this choice is explicitly stated, and that the problems associated with aggregated or otherwise structured soils are better represented

in the text in order to let the readers know this is worthy of consideration in the future.

Reviewer 3 suggests that the retention data for the silt loam (Fig. 2d) may point to bimodality. However, different measurement methods typically have different systematic errors leading to shifts in the retention data at the matric potential where two measurement methods meet. Since in this case the shape is not convincingly bimodal, we are wary to jump to conclusions. The other soils show even less evidence of multimodal behaviour.

3. We refrain from recommending different model combinations for different water content ranges because of our interest in semi-arid regions. There, the top few centimeters of the soil in particular experience the full range of water contents from saturation during infrequent but hydrologically important catastrophic rainfall to nearly oven-dry after months of rainless heat.

4. The error variances for the difference measurement methods were estimated based on such things as sample size in conjunction with the accuracy of the balance. This is explained in lines 554 – 557 in the paper. The reviewer may have overlooked this.

The review lists some additional unnumbered remarks. The first of these suggest to check if we perhaps should rescale the axes of the figures because some curves look very similar. We will review these figures in detail and see what can be done.

Reviewer 3 would like us to discuss why the Alexander-Skaggs conductivity model behaves so differently from Mualem's and Burdine's. We observed that as well, and will see if we can come up with an explanation.

Reviewer 3 is not sure if the flux leaving some of the simulated columns is a numerical artefact. The continued, constant bottom flux during prolonged dry periods is consistent with a mass balance discrepancy that we saw in the simulations, which is why we attributed it to numerical difficulties. Consistent slow convergence also pointed in that direction. A constant flux leaving a drying soil seems physically unlikely: it would require the gradient at the bottom of the profile to increase proportionally to the decrease of the hydraulic conductivity there, and higher up in the profile something similar must occur to deliver the water to the bottom of the profile. If the soil remains equally wet over time, such a constant flux would be consistent with a unit gradient profile with a flow rate that reflects the long-term average net infiltration, but the flux rate is too high for that. But we will revisit these graphs with the reviewer's comments in mind and see if a fresh look brings new insights. The same applies to the comments related to Figure 5.

We thank the reviewer for the suggestions regarding the missing literature, and for offering an interesting analysis of some of our results and other useful suggestions and remarks.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-168, 2016.