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Discussions

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

Anonymous Referee #3

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This paper deals with the important issue of the choice of the appropriate water retention (WR) and hydraulic conductivity (HC) model combinations to be used for simulations of hydrological processes. The objectives of the paper were:

1. Reviewing a number of WR and HC models and introducing a general criterion for WR functions parameterization to ensure physical plausibility of HC curves, especially near saturation. This was done by generalizing the approach proposed by Ippisch et al. (2006) (originally by Vogel et al., 2000) and by extending it to WR and HC parameterizations different from those analyzed by Ippisch et al.;

2. Verifying the performance of the different model combinations in matching experimental WR and HC data for a wide range of water contents, between saturation and

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very dry conditions, and for four soils (sandy, silty, silty-loam and clayey). In order to account for the different experimental errors related to the different experimental techniques for measuring WR data, the authors introduced an objective function accounting for errors varying with the range of water contents considered;

3. Finally, the performance of the model combinations was analyzed in terms of functional properties, by looking at the numerical predictions of drainage, evaporation and infiltration processes obtained by the different WR and HC models in different soils.

The main finding was that different parameterizations may lead to drainage fluxes that may vary of more than one order magnitude, while they may have only limited effects on evaporation fluxes. Also, for a given WR model, the Bourdine (B) and Mualem (M) models for the HC provided similar hydrological behavior, while the Alexander and Skaggs (AS) model gave physically unreasonable predictions of the processes examined.

Based on my reading of the manuscript, I think it is in general significant even if the approach is not novel. The manuscript is fairly structured. The introduction of the paper illustrates quite clearly the rationale and the objectives of the work. However, it does not provide an exhaustive literature review about the approach used, with references missing important papers (since the 1990s) dealing with the same issue. Figures and Tables supports the findings, especially the part on the prediction of the hydrological processes selected for analysing the model performance in terms of functional properties.

The strength of the work lies in the fact that the authors provide a systematic and comprehensive review of the WR and HC models available for hydrological analysis. Crucial in the manuscript is the effort to unify and generalize the analyses of the WR behavior provided by the different models, especially near saturation.

On the other side, I see some limits in the manuscript that can be summarized as follows: 1. The comparison among models is not novel and is based on a too limited WR and HC dataset . There are papers in the past dealing with the same issue of

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analyzing the performance of WR and HC functions, based on huge datasets, that the authors do not consider at all. I mainly refer for example to the work by Leij et al. (1997). The authors assembled different types of mathematical formulations and tested them on a large data set crossing practically the whole textural triangle. I would also add Cornelis et al. (2005).

2. The approaches used are all unimodal. As noted by the anonymous Referee #1, the dataset misses most of the information on structural pores. It is not a case that most of the WR curves in figure 2 provide a similar flat behavior in the pF range 0-1. And yet, central in the manuscript is the behavior of the WR functions near saturation. It is well known that HC models based on the statistical capillary-bundle approach, which are based on the Hagen-Poiseuille law and which integrate the reciprocal of the pressure head to obtain the hydraulic conductivity (as in the case of the Mualem conductivity expression), are particularly sensitive to the slope of the water retention near saturation (Durner, 1994; Coppola, 2000). The effects of a wrong description of the WR close to saturation may have impressive effects on the hydraulic conductivity estimation, with an impact on the soil hydrological processes predictions which may well be larger than the effects observed by the authors in their unimodal analysis (see for example Coppola et al., 2012). Bimodality may also exist quite far from saturation. By looking at the figure 2d, the data trend may well suggest a bimodal behavior in the pF range 2-3 (more or less). It is thus not a case that for the silty loam all the WR functions give a poor description of the data in the drier region.

3. There is no effort for recommending which model combination is the most suitable to be used in a given water content range;

4. One of the objectives of the paper is " . . . a robust fitting method applicable to various parameterizations and capable of handling data with different data errors" (see end of page 2). The authors introduce the reciprocal of the variance of the errors for weighting differently the single data in the various water content ranges. This is an extension of what one generally does when introducing different quantities in the

objective function. And yet, they do not give any information on how they estimated these variances. Selection of data error variances seems to be crucial for determining the performance of the different WR models in describing the experimental data. In other words, the fitting results shown in the figure 2 may be partly an effect of the model parameterization and partly an effect of the error selection for single data;

Some other remarks

By looking at the HC curves in the figure 3, it seems that the AS curves of the three soils are almost the same. The same may be said for the Mualem and Burdine curves for the silt and the silt-loam. It may only be a result of the axis extent used for the HC curves. Please, consider to show the curves for a smaller HC range of values. In general, the AS curve remains stably higher than the B and the M HC curves. As the authors are providing "...a critical evaluation of parametric expressions", the authors should even shortly explain the reason for this behavior compared to the Burdine and the Mualem models.

In any case, in the section 4.2.1., what the authors consider as "physically implausible" behavior (when discussing about sustained, constant flux leaving the silt soil profile during prolonged dry periods in the AS case) strictly depends on the high values the AS HC curve keeps even for very dry conditions. I would not like it to be also the effect of numerical problems. For example, by looking at the graphs in the panel 4c, the silt-RNA_Mualem/Burdine cumulative drainage curves cross the silt-RNA_AS curve. The latter remains unexpectedly lower, given that the WR curve is the same and the AS curve is systematically higher than the Burdine and the Mualem curves). Maybe, the authors should give more details about the evolution of the pressure head at the bottom boundary conditions during the numerical simulations. They do this only for sand. The reader could do an effort for extending the discussion for the sand to the silt soil. However, the authors may agree that this may be quite laborious.

In the figure 5, it seems that the evaporation fluxes are inversely related to the drainage fluxes. Higher drainage induces lower pressure head in the soil profile resulting in lower upward fluxes. Again, one should have a look at the pressure gradients at the soil surface. Nonetheless, in the dry region the AS curve may be even five or more orders of magnitude larger than the Mualem and Burdine HC curves. Thus, in the panel 4d (just as an example), I would not expect higher VGA_Mualem than VGA_AS evaporative fluxes, unless the hydraulic gradient at the soil surface in AS case be five or more order of magnitude lower than in the M/B cases.

Overall, I have no major problems with the manuscript and recommend publication after the authors have discussed these remarks.

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