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Hydrology and Earth System Sciences Discussions

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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Reply to referee 1 Gerrit H. de Rooij, Raneem Madie, Henrike Mielenz, and Juliane Mai

Reviewer 1 provided some thoughtful comments and useful suggestions in a review that is overall positive. We adopt the numbering of the review in our response.

1) Reviewer 1 would like to have more information on the soils. This is available and more complete information can be provided in a revised version of the paper, should the editor decide that a revision is warranted. In this stage it is sufficient to clarify that the data sets we used were obtained with undisturbed samples.

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Discussion paper

2) Reviewer 1 expresses concerns about the lack of representation of structural pores in the retention functions that we analyzed. If the soil is aggregated, one could end up with a system in which the pores within the aggregates and the pores between the aggregates combined produce the retention curve of the soil. W. Durner (1994, Water Resour. Res. 30: 211-223) proposed bimodal retention curves for this, for which he chose superpositions of two VGN curves (abbreviation according to the manuscript). This approach can be generalized to other retention models, and conductivity curves should be able to be derived from those, but we considered that a step too far at this stage of the work, since this approach has its own sets of issues (see our response to Reviewer 3, who also brings up soils with multimodal retention curves). We therefore intend to discuss the issue of multimodality in the Introduction to better indicate the context of our work, but at this stage we would like to limit ourselves to unimodal soils too avoid cluttering the paper.

When structural pores are understood to be macropores, it transpires from our literature review that not representing these in retention curves seems to be the rule, and there are some valid reasons for this. The flow through macropores is generally considered separated from matrix flow. Macropore flow may well be turbulent, and tends to respond very rapidly to rainfall events heavy enough to fill these macropores. The exchange of water between the macropore network and the matrix is orders of magnitude slower, as is the matrix flow itself. Models that incorporate macropores therefore are of the multidomain-type: one domain is reserved for the macropores, and at least one other domain comprises the matrix. Different equations describe the flow in each domain, and there is at least one coupling equation that governs the exchange of water between the domains. For such models, a soil water retention curve that only describes the matrix is ideal.

A second reason to exclude the macropores from the model equations is the limited size of the samples that are used to determine the data points to which the equations are fitted (we will report these as well if we are permitted to revise the paper). These samples cannot be too large since it is desirable that the water content at hydrostatic equilibrium does not vary too much over the vertical extent of the column (even though the fitting code we developed takes this into account). Also, if one wishes to capture soil heterogeneity, one should aim for a sample size that is not much larger than the representative elementary volume. In most soil physics labs, the samples used are about 100 cm3. This usually is too small to be able to realistically sample both the matrix and the macropore network. A macropore may well be present, but it is likely to be cut off by the sample cylinder and by necessity disconnected from the rest of the network. The presence or absence of a macropore may also be a stochastic process if the macropore spacing is significantly larger than the sample diameter.

With this in mind we decided not to try to find retention curve expressions that include macropores to be included in the evaluation. We agree with the reviewer that the role of macropores is significant though, so we intend to clarify the text by incorporating elements of this reply in the main text.

3) The reviewer asks a theoretical basis for the nature of the soil water retention and conductivity models. Durner (1994) (reference provided by reviewer 3) provides a thorough overview of this in his introduction, and there are some review papers on this. We will expand on this in the revision, probably by taking the key elements of earlier discussions in the literature and referring the readers to the relevant papers for more details.

4) The reviewer would have liked to see a larger database of soils. We agree with the reviewer that such data are not always easy to come by in the dry end, but more importantly we wanted to examine the effect of the parameterizations on various fluxes for various textures. With four widely different textures we were already quite comprehensive, and each additional texture would increase the number of model runs considerably without adding much more clarity. We also point out that these parameterizations are not new and each was presented in the literature with its own tests on various soils, and then reviewed using data from additional soils by Khlosi et al. (2008, reference in

the original paper) and Leij et al. (1997, reference provided by reviewer 3 – will be included in the revised version). Adding even more textures to this already rich spectrum did not seem to offer clear benefits, and would expose us to the risk of repeating Khlosi et al.'s and Leij et al.'s work.

5) The reviewer argues that the simulation period should be extended to ensure that the storage change between the start and the end of the simulations is nearly zero. This approach is popular in moderate climates with groundwater tables within a few meters of the soil surface. In such climates evapotranspiration is small in winter, and winter rains and snowmelt wet up the soil profile. In summer, evapotranspiration kicks in and depletes the water in the soil profile. There typically is no 'memory' in the unsaturated zone of rainfall in the previous years. At some time well after snowmelt and just before evapotranspiration starts to rise, the soil profile will generally be well wetted but not so strongly that there is still a high downward flux siphoning off excess water to the groundwater. If one uses that date to start a one-year simulation one should be able to achieve the near-zero storage change that the reviewer desires.

Desert climates and desert soils have a different dynamic: the water delivered by small or isolated rain showers will not infiltrate deeply and evaporate completely. Only rainfall clusters and/or massive showers delivering large volumes of water will cause deep infiltration. This water will create a wetting front of which trial calculations for a related study (not yet published) showed that it will be visible in the soil profile for many years, contributing to the storage term all the time. Extrapolating this to unsaturated zone that can be several hundred meters deep this leads to the conclusion that the unsaturated zone in a desert can have a memory of past major rainfall events that spans many decades if not longer.

This makes the zero-storage change somewhat arbitrary: in the presence of such a deep wetting front that will eventually provide groundwater recharge, the occurrence of small, irrelevant rainfall events shortly before the start or the end of the simulation period will affect the sign and the magnitude of the storage change over the simulation period, but this will not provide any useful information about the magnitude of groundwater recharge. We therefore did not make this an objective of our simulations and believe it does not constitute a valid benchmarking criterion for comparing models.

Also, the magnitude of the storage change is not so important for this particular study. We were interested in seeing how unsaturated zones of different textures described by different parameterizations responded to strong atmospheric forcings: long periods of high potential evapotranspiration interspersed with heavy rainfall. We designed a scenario using meteorological data from a climatic region that had these extreme characteristics.

That being said, the initial condition of hydrostatic equilibrium had an effect on the simulated results. As we mentioned in the paper, the effect of the initial condition became very small well before the first rainfall in the simulation period. Particularly the effect on the bottom flux damped out almost instantly during which there was little or no precipitation. We therefore propose to consider this initial rain-free period the 'warm-up period' and analyze the simulation results for the remaining period of time, starting with a rather dry soil. Given the fact that this is a test of several parameterizations and we are not evaluating any scenarios for the area from which the data were obtained we believe this to be a satisfactory solution.

We note that this will, in fact, increase the difference in storage between the start and the end of the simulation. Given the rainfall data record in Fig. 1 (no rainfall for the nearly 300 days at the start, but considerable rainfall during the last 200 days), the soil will be relatively wet at the end of the simulation period. With the inclusion of the 'warm-up period', the simulations will start after a prolonged dry period. We consider this an artefact that is inherent in this extreme environment.

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