Response to the third round of reviews by Gerrit de Rooij and Raneem Madi.

Reponse to the review by Dr. Coppola

Dr. Coppola's review indicates that he is largely satisfied with the revisions but would like to have seen multimodal functions included. We therefore carried out an analysis of the three multimodal models we discussed in the Introduction. Only one of them met the criterion for plausible behaviour of the hydraulic conductivity near saturation. None of the multimodal models are well adapted for dry conditions. A modification of the unimodal version of the one multimodal model that was well-behaved near saturation was already analyzed in the supplemental material with discouraging results.

We included this information in the revised version, thereby including multimodal models in our analysis, albeit in concise form. The conclusion must be that at this time, multimodal models have not yet reached a stage of development where they can be used with confidence over the full range of moisture contents.

We discussed the problems with the multimodal models in a way that reflects our optimism that they can potentially be solved. If this paper appears in published form it may stimulate researchers with an interest in multimodal curves to address these issues. If so, we look forward to reading their papers.

By including the multimodal models we also addressed the concern that we discussed multimodal models in the Introduction without addressing them in the analysis.

Response to the review by referee nr. 4

This review consists of four paragraphs. We discuss these one by one.

First paragraph

We are not sure what is meant by the comment that suggests that the paper is more organized like a report than a paper. The Introduction (mentioned as an example by the referee) reviews the available literature (expanded as recommended by earlier reviews), provides a rationale for our work, and declares its objectives. This is pretty standard for a research paper.

The paragraph in L 407-421 (also brought up as an example) explains why and where the available literature provides compelling arguments against publishing parameter correlations (as suggested in an earlier review) and offers an alternative for detecting parameter interdependence. This too is valid material in a research paper and relevant for the work we present here. It also directly addresses a point of concern raised in the review process.

Second paragraph

The referee notes that we spent more space on the wet end (air-entry value, near saturation) than on the dry end of the retention curves. Our review of available retention curve parameterizations showed that most of them potentially can give completely unrealistic hydraulic conductivities near saturation caused by the infinite hydraulic conductivity of non-existent huge pores. In order to demonstrate this rather unsettling result we needed to develop a test criterion and then apply this criterion to all reviewed functions. This naturally takes up space in the paper.

In the dry range the review of the existing parameterizations showed that one has the choice between an asymptotic and a logarithmic dry end. The literature demonstrates that the logarithmic form is superior,

and more often than not was introduced as a modification to an earlier model with an asymptotic dry end with the specific goal to improve its performance for dry conditions. This is clearly documented in the reviewed papers and well established, with no need for additional research or analysis by us. Hence the shorter page count dedicated to this aspect.

These very different demands placed on us to carry out our analysis and reach our conclusions both explains and justifies the difference in amount of space devoted to the dry and the wet end of the retention curve.

In the second part of the paragraph the referee focuses on differences between the suction table method and the evaporation method for measuring data points in the wet end of the retention curve. We are not sure why this is brought up for this theoretical, physical-mathematical paper. That being said, we believe the referee underestimates how short the columns are in modern versions of the evaporation method.

The referee suspects that, because the evaporation process is initiated with the bottom of the column at zero matric potential, a soil column to which this method is applied starts at desaturation because the air entry value may already have been exceeded at some depth range within the column. In modern, commercially available set-ups the columns are five centimeters high, the same height as that of most sampling rings used on suction tables. Suction tables commonly do not exceed zero matric potential at the bottom, so the methodologies suffer from the same issue. We therefore do not share the referee's point of view that this problem arises with the evaporation method but not with the suction table method. With sample rings of 5 cm height, the problem is limited to coarse soils.

The water retention data points at the start of the measurement process (when the soil rings in both methods are at hydrostatic equilibrium) therefore have a comparable accuracy for both methods. The conductivity data in the wet range (which the evaporation method can measure but the suction table method cannot) can be noisy when the soil is so conductive that the evaporation causes only a small matric potential gradient. The two tensiometers in the short columns naturally are only a few centimeters apart and each has its own measurement error, leading to a large potential error in the matric potential gradient for conductive soils. Hence, the problem of the evaporation method in the wet range lies in the conductivity measurement, not in the water retention measurement.

For completeness we point out that our fitting code takes into account the vertical variation of the water content over the height of the column for those data points for which the user specified that the method used relied on hydrostatic equilibrium.

For what it is worth, most of the wet-end retention data for the soils in the UNSODA database were obtained through suction table methods.

Third paragraph

The referee expresses some confusion about the text discussing the initial condition (hydrostatic equilibrium) and the lower boundary condition (free drainage).

A careful reading of this comment reveals that the referee understands matric potential profiles under hydrostatic equilibrium and unit gradient conditions. We therefore can safely assume s/he is aware of the change in the lower profile that the change from a hydrostatic initial condition to a free drainage lower boundary condition causes. This invariably involves rapid drainage during the initial stages as the matric potential profile in the lowest part of the column changes from linearly increasing with depth to being constant with depth. Depending on the initial matric head at the bottom of the column, the shape of the retention curve around that matric head, and the corresponding hydraulic conductivity, the duration and amount of the initial drainage varies.

The referee seems to recognize this as well, as evidenced by her/his remarks. All in all it appears that the viewpoints of the referee and us are quite close. We rephrased the text to make it clear that the duration of the transition and the amount of drainage produced by it can be small. We believe this is the only point where we seem to have lost the referee in our train of thought.

We point out that the burn-in period was long enough to ensure that the effect of this initial perturbation of the system did not affect the results.

Fourth paragraph

In a paper investigating the effect of soil hydraulic parameterizations it makes sense to select a test problem that is sensitive to these parameterizations. The final paragraph of the referee report therefore supports our choice of the lower boundary condition.

We agree with the referee that the temporal dynamics of flow through the lower boundary are affected by the choice of parameterization. But we are less firmly convinced than the referee that the unit-gradient lower boundary condition should be expected to have a large effect on the magnitude of the cumulative bottom flux. The amount of water available for downward flow is strongly affected by evaporation and storage changes, and therefore heavily depends on processes in the top soil. Our simulation scenario was designed to ensure the occurrence of arid episodes to allow us to examine the effect of the dry end of the retention curve on the partitioning of infiltrated rainfall between evaporation, storage change, and downward flow, and this worked out well: we were able to interpret these aspects in detail. The referee perhaps did not fully appreciate the near-surface dynamics that were an integral element of the simulation part of the study. We slightly modified the text in Materials and Methods to make this more clear.