Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-168-AC2, 2016

Hydrology and Earth System Sciences
Discussions
Open Access

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

R. Madi et al.

gerrit.derooij@ufz.de

Received and published: 4 August 2016

Reply to referee 2

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

Reviewer 2 offers a very brief review without real recommendations for improvement.

Reviewer 2 states in the first paragraph of the review: 'The manuscript tries to address several issues, e.g., the deficiency of soil water retention (SWR) models near saturation, SWR models near the dry end, development of a general criterion for plausible hydraulic conductivity (K) curves, comparison of different SWR and K models, different methods for parameter optimization, numerical simulation to evaluate model selection

Printer-friendly version

Discussion paper

on drainage and evapotranspiration, and model calibration/inverse model. Even the abstract contains multiple paragraphs, each of which addresses a different issue. As a result, neither of the issues is convincingly addressed.' We disagree with this appraisal, and give three points to build our case.

1) It appears to us that 'the deficiency of soil water retention (SWR) models near saturation, SWR models near the dry end', and the 'development of a general criterion for plausible hydraulic conductivity (K) curves' are really issues that are connected in that they all relate to properties of the parameterizations of soil water retention curves. Therefore, they can all be considered elements of the 'critical evaluation of expressions for the full moisture range' we set out to provide. We fail to see how and why this is confusing, particularly since we explain our approach at the start of section 2.2. We (as well as the other reviewers) believe that section 2.2 does a good job revealing the strengths and weaknesses of the reviewed expressions. Reviewer 2 seems not to have grasped this and cursorily states that this section should be an Appendix.

2) The reviewer states that we were addressing 'different methods for parameter optimization' or 'model calibration/inverse model' (sic), but this is not the case. Neither of these are part of the three objectives we identified in the Introduction, and we did not devote paragraphs to them. The word 'calibration' appears once in the paper, when we use it to introduce the SCE algorithm. The term 'inverse modeling' also appears once, in a sentence in section 4.3 (General ramifications) where we express an interest to see if an inverse modeling of a dynamic experiment would lead to a different choice of parameterization that fitting a curve to hydrostatic data points. Later in the review, it is stated that we should give the method by which we estimated the parameters of the retention functions, which is exactly what we did in section 3.2 of the paper.

3) The statement 'Even the abstract contains multiple paragraphs, each of which addresses a different issue.' is puzzling. HESS guidelines do not require the abstract to be single paragraph, so we do not understand why the reviewer decided to object to that. The structure of the abstract is straightforward: the first paragraph provides

the rationale for the work, explains how we evaluated existing retention functions, and summarizes the main outcome by indicating that only a few parameterizations make physical sense. This part of the abstract therefore summarizes the Introduction and Theory sections of the paper. The second paragraph builds on the first by explaining how we fitted the parameters of the few useful parameterizations and explains that we subjected them to a numerical scenario study (Materials and methods). The third paragraph summarizes the main outcome of the numerical test and gives a recommendation for selecting a suitable parameterization (Results and discussion, General ramifications).

In the following we address some specific points brought up by the reviewer.

As we mentioned above, reviewer 2 recommends transferring section 2.2 to an appendix. In our understanding, appendices are useful for material that is needed in the paper but outside the central flow of thought of the paper. The reviewer does not present a rationale for this change of the paper's structure, other than that the section is long. In defense of the section's length, we would like to point out that if we are going to review soil water retention functions, we need to be reasonably comprehensive. We already left out those parameterizations that are very similar to the ones we included, that are more cumbersome in practical use without offering clear benefits to justify the extra effort by modelers, or that are evidently performing less well than those we included even after a cursory review. We used the critical review in section 2.2 to weed out the functions that we found to be physically unsound. This selection process is a novel element and a main contribution of the paper, as the other two reviewers acknowledge. These findings may have a significant impact on water flow and solute transport modeling practices in unsaturated zone research. Relegating this material to an appendix with the sole argument that the section is long reinforces the impression that the reviewer did not fully grasp the substance of the review or its consequences. We concur with the other reviewers on the relevance of section 2.2.

We presented a generalized criterion to ensure that the soil hydraulic conductivity does not increase at a physically unrealistically high rate close to saturation. Reviewer 2 states that the correctness of the criterion requires the consideration of experimental errors. This is simply untrue: the derivatives of the mathematical expressions used to describe the shape of the retention curve can be evaluated at saturation. A non-zero slope at saturation implies that there is at least one pore that is still saturated with water at zero matric potential. From the Laplace-Young equation it follows that at least one of the principal radii of curvature of this pore is then infinite, which is a physical impossibility in a realistic soil and for a soil sample of finite dimensions. With the hydraulic conductivity function used, a pore of unbounded size gives rise to an infinite conductivity of this pore. The product of the fraction of the soil cross-section occupied by this hypothetical pore and its conductivity must remain bounded in order to prevent the soil hydraulic conductivity to become infinite at saturation, which is the physical basis of our criterion. This requirement has nothing whatsoever to do with experimental error, it is a physical-mathematical imperative. This was already explained in some detail by Ippisch et al. (2006; reference listed in the paper).

The reviewer requires us to point out how the models that meet our criterion should be used. We believe that users of numerical solvers for Richards' equation have an understanding of the way in which soil hydraulic properties are used and how to implement them in their solver of choice. The exact operating details depend on the solver and can be obtained from the respective manual or model support website.

The reviewer states that numerical simulations cannot be used to validate models. That may be the case, but it is of limited relevance for our paper since we do not claim to do so. Instead, such simulations can be used to great effect to 'examine the difference in soil water fluxes . . . calculated on the basis of various parameterizations' as we stated in the last paragraph of the Introduction. As we explained we did so because we considered a simple goodness-of-fit test of the soil hydraulic property curves alone (as done by Leij et al., 1997, and Khlosi et al. (2008)) an insufficient comparison: we needed to see how strongly solutions to flow problems differ for different parameterizations. The results show that this was an effective approach.

We believe that the statement by the reviewer that different solvers can give different results because they use different algorithms is a bit too bold. In practice there are two numerical techniques underlying such algorithms: finite difference and finite element techniques. For both, the mass balance problems that plagued them early on have been resolved in the 1990s, and currently differences between solutions are largely caused by differences in spatial and temporal discretization, not by differences between the different algorithms. The appropriate choice of the spatial grid and the sizing of the time steps is the task of the modeller and depend on the numerical intricacies of the problem that needs to be solved. The reviewer recognizes this, but this does not really bear on the substance of our paper.

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