Review of the revised manuscript HESS-2016-168

Parametric soil water retention models:
A critical evaluation of expressions for the full moisture range

Dear Editor:

After having read the revised manuscript, together with the referees' comments on the original paper and the related responses, I report below my appraisal. The authors made a number of corrections that the previous referees asked for, but I feel that some key points still require being discussed and addressed adequately.

I have listed one general comment and several specific remarks below, the most significant of which are starred (*).

General comment

This study fits within the trend of articles that propose refinements or changes to some parametric relations of the soil water retention function (WRF), enabling this property to be better described in the entire range of matric suction head (namely from 0 cm to 10^7 cm of H_2O). In particular, the Authors have designed this work to address the following two main issues (as stated in the abstract): (*i*) to develop a general criterion [i.e. their Eq.(4)] that needs to be met by soil water retention parameterizations to ensure a physically plausible hydraulic conductivity function (HCF) to be obtained, and (*ii*) to select suitable soil hydraulic parametric relations enabling basic hydrologic processes to be well simulated.

The paper reads well, but I would suggest it should be re-organized in a different manner, whereas the tables and figures appear to be satisfactorily. I suggest the dots (observations) in Figs. 2and 3 should be made clearer and well visible.

However, I think the Authors fail to meet their targets adequately as the readership of HESS would expect. The paper, as it stands now, has more the aspect of an internal report than a scientific article. Overall, the manuscript requires major revisions, or should be rejected altogether. At least, after having been re-organized appropriately and better focused, it might be considered for publication as a technical note.

Specific remarks

- (*) P.5-26, sections 2.1 and 2.2. Section 2.1 is rather confusing. The title refers to "hydraulic conductivity models", but actually line 149 and some other parts talk about the water retention function. As stated already by the previous Ref.#2, I also strongly suggest that most of section 2.2 should be put in an appendix, while leaving that this part only discusses about the "critical evaluation" of the criterion expressed by Eq.(4). In this way the paper will definitely read better and, more importantly, the reader will capture more directly the essence of the aim of this section.
- (*) P.26, section 3.1. It is not completely clear to me why the Authors used only four soils for their evaluations. This question was also raised by Ref.#1 at his point 4). Two comments from my side on that issue. First of all, it is well known that the problem at hand is highly nonlinear and that the hydraulic response of natural soils varies greatly. In view of this need, putting forward the limitation of computational costs does not seem very convincing. Working with more soils provides a sort of sensitivity analysis of the problem at hand. Schelle et al. (2013) used 8 soils and there are papers in the literature that use as many soils as possible [e.g. Zhang (2011), Chen et al. (2014), Rudiyanto et al. (2015)]. I may agree that, for the sake of effectiveness, in the final part of the paper the scenario analyses are presented only for a limited number of soils.

- P.26, section 3.1. This comment is linked somehow to the previous one and to the Authors' reply to point 4) of Ref.#1 as well as to Ref.#3. To my knowledge, there are papers that have evaluated the effects on estimating or predicting unsaturated hydraulic conductivities. These studies were carried out on the wave of having acknowledged that fitting poorly the WRF, especially close to full saturation (at low suction heads), leads often to a poor prediction of the HCF [e.g. Priesack and Durner (2006), Lebeau and Konrad (2010), Romano et al. (2011)]. Moreover, I would bring to the Authors' attention the recent study by Romano and Nasta (2016) who not only compared different parametric models (both unimodal and bimodal), but also performed functional evaluations when computing the soil-water balance for short-term and long-term periods. Please note that the papers dealing with bimodal and multimodal parametric relationships have also shown indirectly that there is a very good improvement in the description of the water retention toward the dry and very dry range.
- P.28, L.620-626. I understand the rationale behind this choice, although I disagree slightly with this effort. My reasoning for that criticism is that it is easy to show (by simulation, for example) that during the drying of a soil core (relatively small in height and placed on a suction plate or subjected to an evaporation experiment), the soil-water contents change little between its upper and lower ends. The matric suctions change more between these two boundaries and this is why the evaporation experiment works well when using not less than 2 or 3 (or even 4, depending of the height of the core) tensiometers. As far as I know, only the paper by Romano and Santini (1999) discussed somehow that point while indirectly justifying the monitoring of time variations in the average soil-water content by weighing the soil core (at each step in the case of the suction plate, or continuously during the evaporation experiment).
- (*) P.28, L.642-647. One point that deserves attention, and is definitely of interest for a number of more specialized readers, is how the Authors obtained the terms σ_h and σ_θ from their experiments. Are these standard deviations constant or depend on the measured h or θ, respectively? It has been found, for example, that the values of σ_h increase as h increases. Please provide specific values or relations. This point is also interesting to ascertain the differences in deviations among the different methods employed to cover the full range of the WRFs. On this matter, the Authors may be interested in reading and citing the work by Bittelli and Flury (2009).
- P.28, L.645-646. The reason why the standard deviation, σ, has to be scaled is not very clear (at least to me). It seems an artifact to adjust the optimization procedure, but this is definitely not in the spirit of the maximum-likelihood method. The errors between observed and computed values are already scaled by the respective standard deviations. The additional scaling of σ to improve the fitting performance is not justified by the theory. Did the Authors plot the response surfaces and see what happen around the minima? What kind of improvements are the Authors looking for?
- (*) P.30, section 3.3, L.699-700. I have perhaps missed something, but the selected initial and boundary conditions seem a bit weird and not really consistent among them. Specifically, the imposed initial condition of a soil profile being at hydrostatic equilibrium clashes a bit with the choice of "free drainage" as lower boundary condition (i.e. the unit gradient of the total potential head during the entire process). Commonly, the hydrostatic equilibrium is a final condition that a soil profile reaches when infiltration stops and the profile drains under the effect of gravity, with a constant water table at the lower end of the flow domain. Accounting for a "spin-up time" is a good thing, but I think that the paper improves if the choice of the initial and boundary conditions allows simulating hydrologic processes much closer to real situations.
- P.30, section 3.3. Simulating the long-term water balance in a soil profile initially at hydrostatic equilibrium and under dry conditions (especially at the uppermost soil horizons) is numerically challenging. The Authors should make an effort to convince the reader that the unavoidable numerical errors are lower than the discrepancies associated to the use of the different soil hydraulic parametric relations.

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