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Interactive comment

Interactive comment on "Sigmoidal Water Retention Function with Improved Behavior in Dry and Wet Soils" *by* Gerrit H. de Rooij et al.

Anonymous Referee #2

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GENERAL COMMENTS The standard parameterization of soil hydraulic functions that is used in the modeling of unsaturated water flow may imply 'non-physical' behavior for certain parameter combinations (i.e. soil types). The authors propose a new approach with better description of the processes under very dry and very wet conditions. Based on numerical experiments, the authors could show that the simulations using the new proposed hydraulic functions were more stable compared to other parameterizations (simulations could be completed for more scenarios with fine textured soils). The discussion of the limits of the standard approach is important to ensure that it is not applied in an uncritical way. In addition, the parameterization of hydraulic functions allowing stable simulations for a wide range of soil types and conditions is relevant. Hence, the motivation and objective of the paper are good but I'm questioning (i) the

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model interpretation and the testing based on (ii) comparison with measured hydraulic properties and (iii) numerical experiments using Hydrus 1D. In short, I propose to use different data sets for model comparison and a more detailed discussion of representing flow processes under dry conditions.

A) In general, none of the discussed models matches the measured unsaturated hydraulic conductivity data (as confirmed by the authors in lines 318-319). The authors hypothesize that these differences between model and measurements are a result of the contrast between measuring soil water retention (and saturated conductivity) in the lab and unsaturated hydraulic conductivity in the field. I agree that conducting measurements in the field will have a big effect and introduce uncertainties - but because the comparison between hydraulic conductivity functions is in the core of this paper, the comparison must be done with measurements that do not have this lab/field-problem. The authors should look for a few measurements from different soil types with very reliable and consistent measurements of both unsaturated conductivity and water retention done in the lab. Specifically, instead of choosing data from UNSODA data base, it would be important to select measurements from papers that are measuring both properties in consistent systems (lab studies).

B) Related to the problem of inconclusive comparison with experimental unsaturated hydraulic conductivity data is the comparison with numerical simulations between the "VGA" model by Ippisch et al. and the new RIA-model. The results are very sensitive to differences in hydraulic functions at high and intermediate water contents and depend on the accuracy of matching the unsaturated hydraulic conductivity in this water content range. The authors should explain in more detail the differences between the conductivity function of VGA and RIA and – based on comparison with high quality measurements – which approach may be more appropriate.

C) The authors state that the 'behavior' at the dry end with water content dropping to zero for finite pressure values is more appropriate. I agree that at some point even the last molecular layer of adsorbed water will be removed - but can the flow processes

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under such conditions be described properly with the hydraulic functions proposed in this paper? Are the appropriate physical processes under dry conditions (film flow, vapor transport, ...) described properly with eq. (12) and (13)? As far as I understand, the hydraulic conductivity functions used in RIA (and VGA and VGN) are based on eq. (12) but this expression relies on capillary flow and does not reproduce the physics of film flow (or vapor transport). So, the simulations at the very dry end – that should reproduce the dynamics of water adsorption and film flow – are based on equations valid for capillary flow. The authors should comment on that.

D) Similar to the discussion of the hydraulic properties at the wet end, the authors should compare the model with measurements (also of K(theta)) at very negative pressure levels.

SPECIFIC QUESTIONS I'm questioning the choice of the title using the terms 'Improved behavior in dry and wet soils' because it was not shown that the 'behavior' was improved.

Lines 58-62: Please expand this paragraph by 1-2 sentences to explain how the water uptake capacity becomes unlimited.

Lines 105-117: The authors should expand on the physical differences between adsorption and capillary forces. The entire paragraph is on water retention only and not on water flow under such dry conditions. The authors must discuss different types of flow related to film and corner flow and how this could be implemented (see Tuller and Or, Hydraulic conductivity of variably saturated porous media: Film and corner flow in angular pore space, Water Resources Research, 37, 1257-1276, 2001)

Line 161, eq. (5): I understand that the value of parameter beta is determined using eq. (7). I would have expected that beta should depend on the surface area of the soil (determining the amount of adsorbed water). Could the authors please comment on that?

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Line 209, eq. (13): Is tau chosen as 0.5?

Lines 225 – 231: I propose to choose less but better data with (i) consistent lab measurements for both SWRC and K(theta) and (ii) some data with K(theta) values at very negative pressures

Figure 1: Why RIA and VGA are different for the loamy sand?

Table 2: You should add Ks and hd (and beta) values for RIA

Figure 2: Choose different color for VGA for silt loam – it is too similar to clay

Lines 293 - 298: The reference to Bitelli and Flury is illustrative – maybe the authors could use some of those data as well to fit SWRC

Line 320: the only sample with K(theta) measured in the lab (soil 4450) is poorly described by RIA . . .

Lines 331-335: There is no experimental evidence that the RIA trends are better – this is just a description of modeled behavior

Figure C5 & C9, Soil 1122 (and other examples): The authors obviously cut the curves at pF=6.8 for VGA and VGN. Probably this should be stated and explained in the captions

Lines 363-365: The statement that ‘small differences between SWRCs can have a significant influence through different hydraulic conductivity curves’ should probably be revised; even for the very same SWRC curve the water flow will be different due to different conductivity functions

Lines 385-386: The statement that ‘RIA was better able to produce a conductivity curve with a substantial drop . . .’ is misleading because we do not know if this ‘substantial drop’ is in agreement with measurements

TECHNICAL CORRECTIONS/COMMENTS Line 17: State that the infinite slope at

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saturation is considered to be physically impossible

Line 37: Write out SWRC at the beginning of the main text

Line 96: the shape of the samples (‘cylindrical’) is not relevant; maybe ‘equilibrating short soil samples’?

Line 157: delete ‘the’ (‘. . . the the logarithmic . . .)

Line 238: Is Tamale not in the tropical climate zone?

Line 315: delete ‘goes’

Line 344-345: what do you mean with ‘reduced Ks-value’ and the ‘high Ks-value’ for clay soils?

Line 352: for the loamy sand under temperate climate, the results ranged from 92 to 104 % (Table 3) – why is this difference more than 10%? Was the deviation in the silt loam (84-115%) not higher?

Line 353: For loamy sand in ‘semi-arid’ climate, the 89% value for evaporation has higher deviation than 10%

Line 354: Temperate, not ‘temperature’

Line 439: delete ‘of’

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