HESS-2022-173 Reviewer 2

The reviewer comments are in *italics*, the reply in regular font.

de Rooij's submission aims primarily at presenting some adjustments to a previously developed sigmoid-logarithmic soil-water retention function (WRF) and evaluates the new parametric relationship in terms of better describing the dry range of WRF. Specifically, this technical note is an extension of previous works by Prof. de Rooij's group on the subject and deals with a slightly different parameterization of de Rooij et al.'s (2021) WRF which places greater emphasis on the role exerted by the "alpha" parameter of the sigmoid component of this analytical relation.

This is partially correct. The main contribution of the paper in terms of practical applicability is twofold: 1) the possibility to fix the matric potential at oven-dryness; 2) providing access to open-source software to fit the parameters of the SWRC and its documentation. Fundamental contributions are indeed the role of alpha, but also the fact that the model of Rossi and Nimmo arises as a special case, which is related to the fact that the sigmoid shape morphs into a power-law shape for large alpha. All these elements are new.

The text reads well and is properly organized, although some parts of Sections #2 and #3 seem too wordy for a technical note. The figures and tables are satisfactory, but I suggest improving the readability of Figs. #2, #3, and #5 (and subsequent) by enlarging a bit more the fonts of the numbers.

I will increase the fonts in the figures.

Section 2 sparked considerable interest from reviewers 1 and 4. Reviewer 1 even asked for further elaborations. Section 3 (which is only 2.5 manuscript pages long, translating to 1 to 1.5 pages in the published version) is almost entirely devoted to the parameter fitting software that is an integral part of the publication. The code is considerably more extensive than currently available parameter fitting codes, evaluating many more convergence criteria, allowing a map of the objective function in the parameter space to be generated, determining the parameter correlation matrix, and running and comparing three separate parameter fitting operations. These features deserve to be mentioned in this paper so that the readership knows what they get when they download the software.

Reviewer 3 also suggested to shorten the paper, but was not as specific, except for recommending to move some of the figures to a supplement. But in my reply to other comments by this reviewer I had to lean on Figs. 5 – 8, while other reviewers specifically commented on Figs. 1 – 3. Given the totality of reviewer comments, it is not so easy to find material that can be cut from the paper. The current text and tables will take up an estimated 5 to 6 pages in the published version, which is not very long.

I have some criticisms and concerns that offer to the author for possible revision of the original submission. As a comment to the editor in charge, I shall make my opinion clearer below that the topic of this manuscript might be of scant interest to the HESS readership. Therefore, this paper should not be accepted in its current form, requires major revisions, or should be rejected altogether.

The impression that a soil physical paper is not suited for HESS surprised me, given that HESS has several soil physicists on its editorial board, and boasts a rich and growing portfolio of published papers from multiple branches of soil physics. Furthermore, a brief inspection of the reference list of this paper as well as the references offered by the reviewer reveals that papers on this subject have appeared in general hydrology journals such as Water Resources Research, Advances in Water Resources, and, indeed, HESS.

1) The author should, first of all, explain well and in detail to the reader why he feels the need to present this parametric analysis only on the analytical expression of the water retention function. Nowadays, it is quite clear that the real advantage of using a relatively simple or more comprehensive closed-form WRF is to infer the hydraulic conductivity function (HCF). I believe it might be worth putting one's attention to only a better (one expects) analytical description of the measured water retention data-points whether one employs the WRF for a certain scope (e.g. to get a better estimation of the water content at the condition of field capacity, or to better compute the integral of WRF).

I agree with the reviewer that the combination of an SWRC and an unsaturated hydraulic conductivity curve (UHCC) is necessary for Richards' solvers. I am working on several UHCCs associated with the SWRC presented here, and found that current approaches combine conductivities of different domains in a way that requires implicit assumptions that are not always physically sound. Solving this issue requires so much work that I took out the SWRC to publish separately in order to avoid a very lengthy paper that runs the risk of becoming murky because it tries to achieve too many things at once.

In the current research climate, the SWRC has merit on its own, as Reviewer 4 points out. The dry-range behavior of SWRCs is experiencing a revival, possibly because of increased droughts. My co-workers and I reviewed these developments extensively (Madi et al., 2018; reference in the paper), so there is no need to repeat that here. Suffice it to say we found many papers that focused on the SWRC without considering the UHCC.

One application not mentioned by Reviewer 4 is in large-scale hydrological models with a spatial resolution of multiple square kilometers. These models incorporate the unsaturated zone but rarely solve Richards' equation. They typically describe the soil as a storage reservoir for water. To better describe the behavior of this reservoir under atmospheric forcing (rainfall, potential evapotranspiration), SWRCs are increasingly being used. I hope and expect that the increasing exchange between the soil physics community and the climate modeling community will lead to the implementation of better SWRCs to replace the often outdated models that are currently used.

That being said, the suggestion to briefly discuss such deliberations to clarify the rationale of the paper is a useful one, and I can incorporate that in the Introduction. I propose the following addition to the second paragraph of the Introduction:

"An SWRC with improved behavior in the dry range even if dry-range data are scant can help improve the behavior of Richards' solvers (references), be useful to improve conceptualizations of the soil reservoirs in large scale hydrological models (references), and for investigating microwave dielectric properties of dry soil and associated soil backscatter (references)."

2) P.3, L.60. On the subject of "hydraulic equilibrium" and relevant constraints on the soil hydraulic parameters, the author can find useful inputs if the review paper by Assouline and Or (2013).

I will re-read Assouline and Or (2013) to verify its merits regarding the issues with the pressure plate apparatus brought up by the references I quoted.

3) P.4, L.80. Here one should warn the reader that the reported range of the "alpha" parameter depends on the parametric relation used to describe the WRF.

The values I perused were found for the original van Genuchten model and our model (RIA). I will indicate so in the revised text. I suggest the following modification:

“Many soils for which SWRCs were fitted according to van Genuchten (1980) or de Rooij et al. (2021) have values for α between roughly 0.001 and 0.3...”

4) P.5, L.106. Strictly speaking, if the first derivative is zero, this is a necessary, yet not sufficient, condition for a minimum (or a maximum). In a view of the detailed parametric evaluation carried out in this article, one should also discuss the likely presence of relative minima and global minimum.

The analysis here relates to Eq. (3) and its partial derivative with respect to alpha in Eq. (5), not the RMSE (the objective function) in the parameter space. Figure 2 covers the entire range of validity of Eqs. (3) and (5) except for the physically irrelevant section where the absolute value of h_j is smaller than 1 cm, so the minimum shown there is indeed the global minimum for the cross-section of Fig. 1 at $n = 1.4$.

5) P.9, Section 3, L.206. I believe the author emphasizes too much the role that the various soil textural classes exert on WRF. Of course, soil texture is important, but other soil characteristics play a role in determining the shape of the WRF and often also have dominant effects, such as the oven-dry soil bulk density, the organic carbon content, and soil aggregate composition and aggregate-size distribution.

I agree with the reviewer, but the UNSODA database focuses on texture more than anything else, and that is where I selected the soils from. Furthermore, Twarakavi et al. (2010) thoroughly analyzed the effect of soil texture on the soil hydraulic behavior of soils, which is why I used their soil classification system. UNSODA in tandem with Twarakavi et al. (2010) is a potent combination. I do not know to what extent the other characteristics mentioned by the reviewer had an effect on the simulations that Twarakavi et al. (2010) carried out to define their soil classes, but it is worth pointing out that many soil properties correlate with soil texture.

It is also worth mentioning that I used the underlying data of the UNSODA soils, not synthetic data generated by an algorithm that uses various soil properties (such as soil texture) as input to estimate the SWRC. The data points reveal a wide variety of soils in the selection used, and hence the suitability of the set to test the parameterization.

6) This comment is somehow linked to the previous point -5-. Some water retention datapoints used in this technical note (especially those shown in Fig.8) seem better described by a bimodal (or multi-modal) water retention relationship. Firstly, coarse-textured soils can have a bimodal retention behavior as happens in the cases of some volcanic soils (e.g. Romano et al., 2011). It should also be considered whether other parametric relations, perhaps even simpler (e.g. just a simply bimodal relation linked somehow to the final matric pressure head Hd) than those proposed in this article, can give equally satisfactory or even better results.

Three out of 21 soils exhibit multimodality. For that reason I offer the multimodal version as well, but because I wanted to have a parameterization that would work of scant data I have not developed that aspect further because adding parameters in a data-scarce environments increases the risk of overparameterization. But the equations are there, so if someone in the community is interested, the starting point is provided.

The second suggestion in this comment seems to contradict comment 1. As far as I know, multimodal (including bimodal) SWRCs lead to conductivity functions that cannot be expressed in closed form. In comment 1, the reviewer criticized the paper for not having a conductivity function, and here the reviewer proposes to introduce functions that do not allow a conductivity function.

Expanding the exploration of multimodality would also require a substantial expansion of the scope of the paper of which I am not sure it is warranted given that multimodal soils are not that widespread (the reviewer mentions volcanic soils), and the degree of multimodality is often not that strong, especially compared to the biases of different measurement methods used in different matric potential ranges. I am not sure sacrificing unimodality is worth the price of losing the possibility to find a closed-form expression for the unsaturated hydraulic conductivity, even though such a curve is not discussed in this note.

An expanded scope would also come at a steep cost: the introduction of Eq. (8) and the, in my view, wonderful result of Fig. 2 would be moved into the background. So would the relationship between the Brooks-Corey based model of Rossi and Nimmo (1994) and the Ippisch et al. (2006) - based model given here. Publication would be delayed by the need to add additional code to the parameter fitting program, which requires extensive testing before I can put in on-line. The issue of overparameterization would require significant attention, which entails the risk of losing the coherence that the paper currently has, according to the reviewer. In short, the reviewer asks for a complete overhaul of the paper that has significant downsides but limited gain. If multimodality is of interest, it is better to devote a separate paper to it.

7) As a final comment and allowing for HESS's aims and scope, if I were a reader of this journal, I would probably find this article of little use. This is definitely not because the analytical developments are not interesting, but mainly because I think it is more effective here to discuss a functional evaluation rather than a parametric one (see, for example, the paper by Iden et al, 2021). This article can be more profitably submitted to a soil physics journal.

I have read many evaluations of SWRCs in recent years during the preparation of Madi et al. (2018). The performance is typically evaluated strictly in terms of goodness of fit, and not at all in terms of the performance of Richards' solvers. Iden et al. (2021) improved on that by carrying out simulations. We did so too in the same year (de Rooij et al., 2021), but the reviewer does not mention that. Our simulations showed that the hydrological behavior of soils can vary markedly for different SWRCs, even though this is not evident from the fitted curves. It is quite possible that I will repeat a similar performance evaluation, but to repeat our simulations for the same SWRC model but with different parameter values seems to be overkill. I think it is better to do so once the UHCC models are finished so I can evaluate various UHCC models proposed in the literature.

Reference cited.

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Romano, N., P. Nasta, G. Severino, J.W. Hopmans, 2011. Using bimodal log-normal functions to describe soil hydraulic properties. Soil Sci. Soc. Am. J. 75:468-480