HESS-2022-173. Reviewer 3

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

The author proposes an adaptation to the new analytical relation presented by de Rooij (2021) to describe the water retention curve in a full matric head range. The analytical expression was tested on 21 soil samples taken from UNSODA database. The evaluation of this manuscript is based on the following questions:

Is it a novel work based on a reliable scientific technique?

Is it clearly structured and well-written?

Are the experimental design and analysis of data adequate and appropriate to the investigation?

Generally speaking, I would like to state that HESS is a high-ranked Journal and should receive novel, robust, scientifically-sound applications based on big data. This technical note does not fit in HESS for the following reasons:

HESS does not base the decision to accept or reject a paper on the requirement that big data is used.

A new, or an adaptation of a new, analytical expression should be tested on ad-hoc water retention measurements to resolve the data fitting in the dry region. The soil sampling should cover all texture classes. The author took only 21 soil samples from the UNSODA. This number is not sufficient to draw strong conclusions. Moreover, the new expression should be compared to the existing expressions (van Genuchten, Brooks and Corey, Kosugi, to mention a few) by considering performance indexes like RMSE, R2, KGE and especially AIC (that penalizes the performance if the number of parameters is too high).

As explained in the text, dry-range data are often not present or unreliable, yet we need a SWRC that is realistic in that range as well. de Rooij et al. (2021) showed that parameter fitting without taking this into consideration could result in unrealistically high values for the matric potential at oven dryness. As the Introduction and the title state, one of the objectives of the paper is to offer a SWRC parameterization that can overcome such limitations. Testing such a SWRC on new data with reliable values in the dry range would defeat that purpose. Furthermore, de Rooij et al. (2021) already showed that when such data are available, a good fit is possible. This paper can give realistic curves even when this is not the case.

I am not sure why the reviewer thinks 21 soils not sufficient to test a SWRC parameterization. A quick sampling of the literature that introduces new parameterizations reveals that these were typically tested on four (e.g., Ippisch et al., 2006) to six or seven (e.g., Rossi and Nimmo, 1994; Tuller and Or, Water Resour Res. 41, W09403, 2005) samples, and the conclusions drawn in these papers have not been challenged in the literature. Larger sample populations were used for comparing different existing SWRCs.

The query used to select the UNSODA soils from the database required at least some retention data in the dry range as well as the availability of unsaturated hydraulic conductivity data. That combination returned about 30 soils, some of which were not usable because the range was still too small to fit the SWRC to. Others were dropped from the selection because sandy soils were overrepresented. The reviewer is referred to the texture triangle in Madi et al. (2018), which shows that the range in textures is, in fact, quite large, with only heavy clays missing. There is no reason why our selection of 21 soils should limit the strength of the conclusions.

A comparison between RIA and other parameterizations was already performed by de Rooij et al. (2021), and the reviewer is referred to that paper for details.

The parameter fitting optimizes five water retention curve parameters on six to fourteen data pairs (Fig. 5) which are unable to provide basic information on the water retention curve. The author should show the uncertainty bands around the optimized curve and other performance indicators. I would be curious to see the parameter crosscorrelation for each soil sample in Supplemental Material. What are the parameter ranges?

I am not sure why the reviewer states that the observations did not provide basic information on the water retention curve. As the paper points out, only the dry-range data points were questionable, and their weighting factor in the fitting operation was reduced accordingly. I also do not know why the reviewer only refers to Fig. (5), which only depicts sandy soils.

Uncertainty bands are difficult to quantify. But as the paper and the user manual of the parameter fitting code (which is part of the publication package) state, the fitting code automatically generates three fits for each optimization run, and warns if the fitted values or the RMSE deviate by more than 10%. The fits and their RMSEs did not give cause for concern. Neither did the correlation coefficients, as discussed in the paper.

The permitted parameter range for the saturated water content were set on input to envelope the range of observed saturated water contents on input, with a limited buffer on either side of the range. The range of the air-entry value was determined based on the wettest unsaturated data point and the driest saturated point, with a generous buffer. The range limit at the wet end was often set to zero. The matric potential at oven-dryness was mostly fixed and occasionally varied over a relatively wide range depending on visual examination of the data points. The permitted range for alpha spanned several orders of magnitude (0.001 to 0.5, except for 1142, where alpha could go as high as 100.0). After some trial and error, the range of n was set relatively narrow (between 1.05 and 2) because even with wider ranges the fitted values fell within this range. Any time a fitted value was close to one of its limits, the fit was repeated with an expanded range.

This technical note resolves the fitting only for the water retention function by ignoring the hydraulic conductivity function that represents an essential prerequisite to solve the Richards equation in process-oriented hydrological models

See my reply to a similar comment by Reviewer 2, and the proposed clarification in the text. Reviewer 4 identifies several applications of SWRCs outside process-oriented hydrological models.

The author should convince the readers the advantage in using this equation and the disadvantage in using the most widespread equations. The three above mentioned analytical relations (and others) provide a closed-form expression for the hydraulic conductivity curve and for this reason are widely used by the hydrology community

This was already done by de Rooij et al. (2021). The discussion of that paper addressed the hydraulic conductivity curve as well, perhaps it is of interest to the reviewer.

Madi et al. (2018) developed a criterion that SWRC parameterizations need to meet to ensure that the slope of the UHCC does not approach infinity near saturation. Almost all parameterizations since Brooks-Corey fail this criterion, including that of van Genuchten (1980). Kosugi's models need to evaluated on a case by case basis because their derivatives are unwieldy. For values of kappa of 1 and 2, Kosugi's model fails the criterion (see the supplement of Madi et al., 2018). Given the importance that the reviewer attaches to the UHCC, it is remarkable that two SWRC parameterizations that have been shown to yield non-physical conductivity curves are recommended in this review.

The RMSE is calculated on soil water content data (line 219)? If so, the RMSE values (the author should report the units) in Table 1 are dramatically high. In line 276, the author states that maximum allowed RMSE is 0.1 but in Table 1 I see many values higher than 0.1

The RMSE is based on the volumetric water content as the reviewer suspects, and is therefore dimensionless. Hence, units are not reported.

Inspection of Fig. 5 – 8 reveals why some of the RMSE values are high. Several soils have well-aligned data points, except near saturation. Some even have two different values at or very near saturation. I am not sure what the cause is (possibly air inclusion), but it is clear that such discrepancies lead to enlarged RMSE values, even for good fits. In other cases (soils 3250 and 4450), the data are noisy even in the unsaturated region. In a few cases (soils 1180, 1181, 1182) the unimodal RIA model cannot reproduce the shape of the observed SWRC.

In addition, in regions where the slope of the water retention curve is not zero or infinite, this slope is used to determine the weighting factor of the observation error in the matric potential. In regions where $\mathrm{d}\theta/\mathrm{d}h$ can be small (in the very wet and the very dry range), this can lead to a relatively large weight, thereby increasing the contribution to the RMSE. I am not sure if other parameter fitting codes account for errors in observed matric potentials, so a comparison of RMSE values from different fitting codes may not be valid.

Section 3 indeed states that one of ten convergence criteria requires the RMSE to be smaller than 0.1. The section also states that no more than four of those criteria were allowed to fail to achieve convergence. The section explains that, as long as convergence is not achieved, the code keeps on trying until the maximum number of iterations is exceeded. Section four discusses which criteria were met, and which ones were not. From that discussion it is clear why several soils had RMSE values that are higher than the one specified in the convergence criterion.

The RMSE has a global minimum of which the value is not known *a priori*. It is therefore incorrect to assume that if a certain ceiling is specified, the code will always succeed in finding a value lower than that ceiling. If the global minimum is found, the algorithm performed well, even if the RMSE exceeds the target value.

The text is too long for a technical note. There are too many figures that can be moved in Supplemental Material. The title should be concise and line numbering should be continuous for all lines. Figures must be improved consistently (enlarge fontsize, add grid, clarify axis titles by adding symbols and units) This technical note should be rejected in this present form. I suggest to revise it thoroughly and re-submit

The reviewers have mixed opinions about the length of the text. The only other making that comment (Reviewer 2) was more specific. The suggestions made by that reviewer were partly in conflict with a request by reviewer 1 for more explanations, so it may be difficult to shorten the text based on the combined set of reviewer suggestions. I estimate that the main text (without figures, with tables) will be fewer than five pages, which should be OK for a technical note.

Which figures should be moved to a supplement? Figures 5 – 8 are perhaps the most likely candidates, but from the exchange above it is clear that they are important for a correct interpretation of Table 1.

The HESS guidelines state that the title should be concise but informative. The title has 20 words (excluding 'Technical note') which is on the long end of the usual range but not over that range. There are few low-impact words in it, and is informative enough to attract the relevant subset of the HESS readership.

Line numbering is continuous, I do not understand this comment.

The figures adhere to the HESS guidelines. The font size will be enlarged (another reviewer also raised that issue). The tick marks are on opposite axes, so there is no need for a grid, and the HESS guidelines do not require one. The axis titles are all correct. The coordinates are all dimensionless (and the corresponding parameters and variables declared as such in the text), so the comment about missing dimensions is moot. In case logarithms of dimensional quantities are plotted, the units of these quantities are specified in the figure captions.