Reply to reviews accompanying the revised version of hess-2022-173: **A sigmoidal soil water retention curve without asymptote that is robust when dry–range data are unreliable**, by G.H. de Rooij

Overall reply

Two reviewers (1 and 4) reviewed not only the technical note but also the parameter fitting software and its user manual. Both of these reviews were very positive, suggesting only minor changes and asking for some additional explanations to be added to the text. Two other reviewers (2 and 3) focused on the technical note only. They were more critical, suggesting that the paper be shortened, the unsaturated hydraulic conductivity curve (UHCC) be added, and a functional evaluation be carried out, i.e., an evaluation of the performance of a numerical Richards solver running with the proposed SWRC. One of the more critical reviewers stressed the importance of a closed-form UHCC while also suggesting a pivot of the focus of the paper on multimodal retention curves, for which closed-form UHCCs cannot be derived.

In response to the more critical reviews it is helpful to sketch the relationship between this paper and two preceding papers (both referenced in the manuscript). In Madi et al. (2018), my colleagues and I evaluated most parameterizations of the SWRC published to date. We found that 14 out of 18 examined parameterizations produced UHCCs with an unphysical infinite slope near saturation. Neither the most popular nor the most recent SWRCs belonged to the four that gave physically acceptable behavior of the UHCC near saturation. This stimulated us to develop our own SWRC by combining two independent lines of research that produced the SWRCs of Rossi and Nimmo (1994) and Ippisch et al. (2006). We combined these two, termed the resulting parameterization RIA, and carried out a functional evaluation comparing three sigmoid SWRCs: that of van Genuchten (1980) because of its wide-spread use, that of Ippisch et al. (2006) as an improvement of van Genuchten (1980) in the wet end, and RIA as an improvement of van Genuchten (1980) in the wet and the dry end. The result was published in de Rooij et al. (2021). That paper includes an appendix in which two additional recent parameterizations were shown to lead to non-physical UHCCs.

I then started working on UHCCs for RIA, but in the process found a fundamental issue with the way UHCCs have been constructed in the recent literature from separate UHCCs for water in capillaries, in films, in an the vapor phase. Addressing this issue required both a deeper look at RIA, and the development of a substantial body of theory for combining UHCCs of different categories of soil water. I decided it was best to publish both aspects separately to avoid a very long paper with multiple messages. This explains the focus of the technical note and the absence of a functional evaluation.

These developments informed the choices I made in the revision for those cases where the reviewers made conflicting recommendations. These decisions are explained below.

Reply to reviewer 1.

Comment 1: The poor performance of the model for C2 and C4 soils is discussed in more detail now.

Comment 2: The comments in the code now include references to equations in the paper. If the Editor accepts the paper I will upload the code after publication of the paper so I can include a complete reference to the paper.

Minor comments: All typos were corrected, and an explanation about the location of the narrow band of valid values of alpha in Fig. 1 was added to the text as well as in the figure itself, as requested.

Reply to reviewer 2

Main comment:

The text of sections 2 and 3 was shortened, as requested. There was limited scope to so because some review comments requested more detail and other comments could only be addressed referring to figures that one reviewer wanted me to move to a supplement. When entire lines or paragraphs were deleted, a single word near the deleted text is highlighted in the marked-up version.

The readability of the figures was improved by increasing the fonts and increasing the line thickness. As requested by Copernicus, the color scheme was modified to accommodate color-blind readers.

Comment 1: The revised Introduction explains the relevance of SWRC parameterizations even if a corresponding unsaturated hydraulic conductivity curve is not provided.

Comment 2: Assouline and Or (2013) did not discuss the lack of equilibrium of dry-range data points of the SWRC. However, they addressed the effect of some parameterizations on the hydraulic conductivity near saturation, so I included a reference in the Introduction where that issue is discussed.

Comment 3: The revised text makes clear to which parameterizations the range of alpha-values reported in section 2 applies, as requested.

Comment 4: In my earlier reply to the reviewer I explained that the section of the text to which this comment applies does not discuss the objective function, but the relationship between alpha and the matric potential at the junction point. As Eq. (5) and Fig. 2 show, this relationship has only one minimum.

Comment 5: I agree with the reviewer that other factors than texture are important, but the UNSODA database has more comprehensive data for soil texture than for other soil properties that are not soil hydraulic properties. I gave the reasons for the emphasis on soil texture and the combination with Twarakavi et al.'s (2010) classification system in my earlier reply. I also would like to note that soil texture is the most common classifier used in the literature when different soils are used to introduce new or compare existing SWRC parameterizations. The reference list of this paper contains several examples, Madi et al. (2018) gives many more.

Comment 6: As I explained in the earlier reply, by asking for a refocus on multimodal soils, the reviewer essentially (and perhaps unknowingly) requests a complete overhaul of the paper that would obscure the most interesting findings for relatively little gain and makes it impossible to derive closed-form UHCCs based on RIA. The paper does, however, offer a starting point for those interested in multimodality for further development. As I wrote in the initial reply, the comment seems to contradict another comment by the same reviewer, making it difficult to address it.

Comment 7: The reviewer and I disagree about the relevance of soil physical papers for HESS. I share the reviewer's interest in a functional evaluation, which is why we published one when we introduced the original parameterization (de Rooij et al, 2021). It will be better to carry out another functional analysis after I finish the work on the UHCCs.

Reply to reviewer 3

Main comment:

The reviewer states that the 21 soils selected from UNSODA do not represent a wider range of textures. The text contains a reference to Madi et al. (2018), where we present a texture triangle with the selected soils demonstrating that only heavy clays are not covered by the set of soils. Above, I explained the connection between Madi et al. (2018), de Rooij et al. (2021), and this paper. The preceding papers appear to cover much of the work requested by the reviewer.

The reviewer emphasizes Akaike's information criterion (AIC). In my view, AIC is not too informative if the choice is between 4 and 5 parameters, as would be the case with most parameterizations. For comparing very simple models (with one or two parameters) and multimodal parameters (with 8 parameters or more), AIC would be more interesting, although none of the reviews of SWRCs that I read applied it. Such an analysis would be outside the scope of this technical note and add little to our earlier work.

The reviewer states without explanation that the data pairs for the individual soils do not provide basic information on the SWRC even though the UNSODA database was specifically created to provide exactly that information. Perhaps the reviewer refers to the dearth or poor quality of data points in the dry range for some soils. As the title of the paper explains, this is part of the rationale explaining why this version of RIA is needed.

The reviewer would like me to add uncertainty bands around the fitted curves. Although the idea is good, its implementation is laborious and particularly difficult given that the UNSODA data were gathered in different laboratories at different times, and the measurement methods used were not always reported. I therefore had to guestimate error standard deviations to determine the weighting factors. Using these estimates to generate uncertainty bands would be a bridge too far in my view. The robustness of the fits was tested by automatically generating three fits with different starting values and checking if their RMSEs were not too different. This is already more than the single fits that are usually presented when a global search algorithm is used.

The reviewer would like to see correlation matrices. I explain in the paper that none of them were high enough (never exceeding 0.31, usually considerably lower) to warrant scrutiny. In order to keep the technical note as short as possible (the reviewer thinks it is too long), I therefore prefer not to discuss the correlations in detail.

The parameter ranges provided on input are given in a new paragraph in section 3.2, as requested.

The comment about the focus of this note on the SWRC was addressed by adding an explanation to the Introduction (see the reply to Reviewer 2).

The reviewer emphasizes the importance of the hydraulic conductivity. Conductivity data are available for all soils used in this paper (that is why they were selected), with the vast majority of those data measured in the field. It will be extremely difficult to determine the experimental error of those data points with a degree of accuracy that would allow the determination of uncertainty bands once parameterizations are fitted to those. In view of this, what would be the added value of uncertainty bands around the fitted SWRCs?

The comment about a comparison to other parameterizations seems to indicate the reviewer may be unaware de Rooij et al. (2021), where such a comparison was performed. Furthermore, Madi et al. (2018) showed that two of the three SWRC parameterizations mentioned by the reviewer give non-physical UHCCs, while the third (Kosugi) has to be evaluated on a case-by-case basis. This makes it hard for me to interpret the emphasis on the UHCC expressed in the previous comment. As explained above, we developed RIA very much with the UHCC in mind, so I am a bit at a loss about what changes are needed to address these two comments that appear to be mutually conflicting.

The RMSE was indeed calculated on water content data. This is clarified in the revised text.

I shortened the text a little (see the reply to reviewer 2). The title was shortened as requested. I would like to point out that the last part of the title (...*robust when dry–range data are unreliable*) contains an explanation that applies to the comment of this reviewer about the UNSODA data sets.

I kept all the figures in the main text because I need them all to address the issues raised by the reviewers, and because without Figs. 5 – 8, Table 1 is difficult to interpret. The readability of the figures was improved (see above). I do not understand why the reviewer asserts that units need to be added to the figures and the axis titles need additional information. I checked, and there are no missing units and all plotted variables are correctly indicated. The reviewer also proposed to add grids to the figures. I regularly read HESS papers and never saw a graph with a grid. The tick marks on all axes can be used to accurately determine the location of a data point, if so desired.

Reply to reviewer 4

Main comment:

Other reviewers asked for an explanation of the stand-alone value of the SWRC (without accompanying unsaturated hydraulic conductivity curve). This reviewer provides several arguments, which I gratefully adopted to amend the Introduction.

Minor comments:

The SCE parameter optimization algorithm is well established in hydrology, and many members of the HESS readership will be familiar with it. Given the desire to keep the paper short I prefer to limit the explanation of its use to that given in my initial reply, as it will be visible once the paper is published. Adequate references are provided in the paper for those interesting in learning more.

In the Introduction, alpha is mentioned because its behavior gave rise to one of the objectives of the study, which belong in the Introduction. A detailed analysis of the behavior of alpha is undertaken in the theory section.

The notation of dimensions of variables adheres to established practice and is frequently used without explanation in other papers, so I prefer not to explain them in the text to keep the technical note concise.

The text was modified as requested for all remaining minor comments.