Comments of reviewer 2

Comments in *italics*, reply in regular font.

General comments

The manuscript provides a functional evaluation of three unsaturated hydraulic conductivity models, namely the Kosugi (KGV), additive (ADV), and junction model (JUV), in simulating the water balance under different soils and climate conditions. To this end, multi-year simulations are conducted with the HYDRUS 1D model, and the results are compared in terms of aggregated mean fluxes at both the upper and bottom boundaries of the soil profile. The selected functional approach is noteworthy as it allows to compare soil hydraulic models with different characteristics (i.e., considering vapor diffusion) directly in terms of their effects on simulated outputs. However, it should be pointed out that despite being fairly conducted, the study is basically a numerical exercise with a limited link with real data either in terms of the selected soils or simulated water fluxes. In fact, the soil hydraulic conductivity data are gathered from literature while the simulated fluxes are not compared with measured ones, and, therefore, their validity can only be assessed in relative terms. Thus, this study adds a limited contribution beyond what was already investigated in previous studies conducted by the same author(s).

Reply:

The main role of the soil water retention curve (SWRC) and the unsaturated hydraulic conductivity curve (UHCC) is to define the soil hydraulic properties (SHPs) for numerical solvers of Richards' equation. A test of different parameterizations by examining the fluxes calculated by such a solver is intrinsically relevant for this reason alone. Our reply to Reviewer 1 (who made a similar suggestion) explains the vast cost and effort required for a test involving measured data, as well a some of its pitfalls.

In the literature, the vast majority of comparisons of different parameterizations of SHPs limit themselves to evaluating the fits to data points of the SWRC and/or the UHCC, and completely ignore the effect of the different fits on the fluxes calculated by the Richards' solvers for which they are chiefly developed. Evidence of this can be found in the papers reviewed by Assouline and Or (2013, cited in the preprint) for the UHCC, and by Khlosi et al. (2008) and Madi et al. (2018, cited in the preprint) for the SWRC.

For the modeling community that runs Richards' solvers for practical applications, it is important to know what kind of parameterizations are available, and how they perform. For modelers, performance in terms of the goodness of fit against measured conductivity is less important if the resulting fluxes calculated by different fits are not that different. It is highly relevant for this community if a parameterization with a comparable or even somewhat better fit than its competitors leads to more frequent crashes of their numerical model. The reviewer did not consider this aspect, which naturally requires test runs with a numerical model. We selected the scenarios in such a way that the numerical model needed to negotiate a wide range of forcings, and was confronted with the numerically notoriously difficult case of infiltration into dry soil. The scenarios chosen were, therefore, not at all ad hoc, as reviewer 1 suspects.

To our knowledge, comparisons of parameterizations of the SHPs by testing them using numerical models for unsaturated flow is exceedingly rare. Apart from this paper, we found only two: Ippisch et al. (2006), who used as a test case a single 2D case study of a two-layer soil with macropores, and an earlier paper by the second author with coworkers (de Rooij et al., 2021, cited in the preprint). This study uses a modification of the set-up of de Rooij et al. (2021) that is more thorough, encompassing 135 test cases in total. The reviewer does not acknowledge the dearth of numerical tests of SHP parameterizations like the one we performed, and does not seem to appreciate the very large number of test cases covered in this paper.

Specific comments

The presentation of the selected hydraulic conductivity models is poor and their properties are difficult to understand if the readers do not refer to the previous studies. I think the manuscript's readability could be improved if the models were presented, even showing the related equations. There are several symbols in text and tables (e.g., h_j, K_{sc}, K_{sa}, τ, γ) that are not defined when they are introduced for the first time or not defined at all. Furthermore, the choice of using the Kosugi model for comparison should be motivated as well as the advantage of ADV and JUV over classical models should be highlighted.

Reply:

We found guidance in rules 5 and 6 of the Obligations for authors on the HESS website, specifically the following elements:

- An author should cite those publications that have been influential in determining the nature of the reported work and that will quickly guide the reader to the initial work essential for understanding the present investigation.
- Fragmentation of research papers should be avoided. A scientist who has done extensive work on a system or group of related systems should organize publication so that each paper gives a complete account of a particular aspect of the general study.

The referencing is complete, save for the explanation of the acronym RIA for the SWRC parameterization we used, and the references linking to the KRIAfitter code. These oversights by us, we will of course correct in case we are allowed to revise the paper. With those corrections, the information necessary to follow the paper can be readily found from open access sources. That being said, we believe the suggestion to include the underlying equations is good, and will increase the readability of the paper without undue repetition of already published material. If a review is granted, we will propose to add a Theory section after the Introduction, where equations for the RIA parameterization for the SWRC and the three UHCC parameterizations will be provided and briefly explained, backed up by adequate referencing to the two source papers (de Rooij, 2022, 2024b, cited in the preprint). This will also resolve the unclarities of the parameters, as the reader will then be able to see where they appear in the equations.

The selection of the three UHCC parameterizations was based on the findings of de Rooij (2024b). An explanation will be provided. In short, the explanation will be along the following line:

The Kosugi model (KGV) is a generalization of Mualem's model, which has been the most popular model for the past 4 decades, supplemented with a model for the equivalent conductivity that represents the diffusion of water vapor. This model only considers capillary water and water vapor. The additive model (ADV) represents a generalization of the PDI model (introduced in Peters and Durner (2008), Peters (2013), and Weber et al. (2019) – all cited in the preprint) that accounts for water adsorbed in films as well. De Rooij (2024b) elucidated the implicit underlying assumptions (and a flaw) of PDI. He developed alternative models by introducing other, equally (or more) plausible assumptions, and found that the flaw was impossible to fully eliminate. He also found evidence of overparameterization, which is one of the reasons he introduced the simpler junction model (JUV), which has the same number of fitting parameters as KGV, and one less than ADV, but still accounts for water in films. The alternatives to ADV developed by de Rooij (2024b) did not perform better, as demonstrated by de Rooij (2025) (cited in the preprint), so there is little incentive to further pursue them. Hence, the model of choice for the past 40+ years (KGV), its challenger with a more comprehensive description of the UHCC (ADV), and the more parsimonious alternative to ADV (JUV) are the models chosen for the test of their performance when used in a numerical models of soil water flow.

I hardly understand the rationale for considering the different combinations of fixed and fitted parameters in model simulations. Indeed, if the soil hydraulic conductivity data are measured, the best choice is the one that yields the lowest values of fitting statistics (RMSE, R2, AIC, MBE….), and comparing alternative strategies in terms of model outputs is trivial without independent reference data. However, it should be recognized that the selection of the parameter fitting strategy is a crucial step once the experimental data are obtained and guidelines helping practitioners in this choice are probably lacking but, in my opinion, this point should be addressed with larger soil databases including soil with different origins and characteristics.

Reply:

Reviewer 1 also wondered about the different sets of fitting parameters. We refer to out reply there for that part of this comment. We also explain there our reasoning for examining the effects of these fits on the calculated fluxes in addition to comparing goodness-of-fit measures.

As explained above, the reviewer underestimates the time, effort, and resources needed to acquire independent reference data. Furthermore, a model-based comparison revealing effects of parameterizations and the choice of fitting parameters has intrinsic value: it shows if and how the choice of UHCC and its set of fitting parameters affects calculated fluxes. There are two possible outcomes: significant effect and hardly any effect. Both are tangible, actionable results. Especially in the case of the former, independent data would be very welcome, but in case of extreme differences, expert judgment can be used to assess the plausibility of the calculated fluxes. In case of the latter, the simplest model is probably the best.

This paper is about a numerical test of three UHCC models. For the extensive test that the reviewer requests, involving fits of UHCC parameterizations on a wide range of soils, we refer to the 780 best fits in total presented by de Rooij (2025), cited in the preprint.

Minor comments

L.11: What is RIA?

Reply:

The other reviewer also noted this omission. As explained in our response there, will rectify this if we are allowed to revise the paper.

L.49-64: Is the discussion conducted here strictly necessary for the study? I checked that these points were already discussed in the papers where the models were developed.

Reply:

This section summarizes the more elaborate discussions to explain the backgrounds of the tested UHCC parameterizations, and their relation to one another. We can shorten it of course, or refer the reader to these papers and leave it out altogether. However, elsewhere, the reviewer asks for more information about the UHCC parameterizations and asks us to include far more information from these papers. If we do that (as we intend to do), it seems a bit awkward to leave out this short segment of text.

L.71: On parameter less than what?

Reply:

Well, one parameter less than the more complicated models that were discussed in the text preceding this sentence (and which the reviewer wishes us to remove). We will clarify this if we are allowed to revise the paper. The proposed modification reads: '…than the more complicated models derived by de Rooij (2024a) based on those by Durner and Peters (2008), Peters (2013), and Weber et al. (2019).'

L.84: "all of them including diffusive movement of water vapor": is it true also for KSG model? If yes, how the water diffusion was accounted for in KSG model?

Reply:

We do not use the acronym KSG in the paper. Our model KGV adds the vapor conductivity of Peters (2013) as modified by de Rooij (2024a) to Kosugi's model for the conductivity of capillary water, as explained in de Rooij (2024a). In the revised version with the added Theory section, this will be clear to see directly from the equations.

L.153: What is the KRIAfitter code?

Reply:

Thank you for noticing this. We omitted two references there: de Rooij (2024c, 2025). Both were already cited, and discussed in some detail, in the Introduction, but the link with the KRIAfitter code was not made there. The references will be added.

L.155-163: Most of this information was already given in L.90-103. I suggest to unify these sections.

Reply:

Good suggestion, thank you. We think it is best to keep the reference to the earlier paper in L. 90-92, and transfer the rest of that section to L. 155-163, removing any overlap in the process.

L.214-215: Is this conclusion of general validity? I don't think so given only three soils were considered.

Reply:

Three very different soils, three very different climates, and a total of 135 model runs totaling 1350 simulated years (excluding 810 years of burn-in). This is by far the most extensive numerical test of UHCC parameterizations to date.

Reference

Khlosi, M., Cornelis, W. M., Douaik, A., van Genuchten, M. Th. and Gabriels, D.: Performance evaluation of models that describe the soil water retention curve between saturation and oven dryness, Vadose Zone J., 7, 87-96. doi:10.2136/vzj2007.0099, 2008.