Dear Reviewer,

thank you very much for reviewing our manuscript. We have considered all of your statements carefully. Please find below our detailed answers to all comments. We are convinced that your comments will lead to considerable improvement of our work and thank for this constructive input. In those cases, in which we disagree with your assessment, we provide a detailed justification for not following your recommendations.

In the first part, we answer to all major comments, in the second part we list all the annotations you provided in the pdf together with the specific replies. In cases, where the annotations are similar to the major comments, we refer to them. For convenience, we numbered the comments.

Kind regards,
Andre Peters, Sascha C. Iden, and Wolfgang Durner

Major comments

1. Overall, the paper is good, and the subject matter worth publishing. I provided detailed comments throughout the manuscript to improve clarity.

   We thank you for this quick and detailed review and your positive overall judgement.

   There are two main concerns I have that I believe should be addressed:

   - In the Introduction, the literature overview on soil water retention curves is not up to date. I offer a few references below that might be of help. But there are additional recent papers worth citing in the overview.

   Li et al. WRR 2023 doi: 10.1029/2022WR033160

   Wang et al. WRR 2022. doi: 10.1029/2021WR031297 (has some good references)

   Rudiyanto et al. J. Hydrol. 2020. doi: 10.1016/j.jhydrol.2020.125041

   Weber et al. WRR 2019 doi: 10.1029/2018WR024584 (some of you were involved)

   We thank you for these suggestions. However, we disagree with your statement that our model selection is outdated. In the last ten years, there have been several new formulations for the SHP models each year. Most of them have not been tested by independent researchers. Your short selection is arbitrary from our point of view. As a side note, the proposed functions add up to a very large number (>40) that have been proposed in the past. We will cite some of them in the revised introduction and briefly explain in the following why we did not consider these models in more detail:

   Li et al.: The authors introduce several sigmoidal functions for “basic retention functions” (in our terminology). These functions will not perform better than the FX or the vGm basic retention functions as they have only two free fitting parameters for the saturation function (as the vGc and the Kos models in our study).

   Wang et al. and Rudiyanto et al.: Both models build up on the Wang et al. (2018) model, which combines the FX retention model with the analytical formulation of the Mualem capillary model, obtained for the constrained van Genuchten retention function. In our view, this is incoherent because the hydraulic conductivity function is decoupled from the water retention function.

Weber et al.: In our manuscript, we focus on the capillary part. The Weber et al. formulation for the capillary parts of the SHP functions is identical to the earlier PDI model. The non-capillary part differs from the PDI but has conceptual shortcomings (see Peters, A., and S.C. Iden, 2021. Comment on ''A Modular Framework for Modeling Unsaturated Soil Hydraulic Properties Over the Full Moisture Range" by Weber et al., Water Resources Research, e2020WR028397 (open access).

We note that the focus of our study is on the analysis and comparison of the capillary bundle models. The use of more than 4 saturation functions does not seem to add further value to this study. However, we would be happy if researchers will extend this study by using other saturation functions to compare the capillary conductance models.

2. Partially as a consequence of this, the authors use outdated expressions for the WRC. For two of those (vGm and FX), recent papers proposed improvements that address the shortcomings that are highly relevant for this paper. References are provided below. This weakens the paper considerably. Section 4.3 looks awkward because of this.

Ippisch et al., Adv. Water Resour. 2006. doi: 10.1016/j.advwatres.2005.12.011

de Rooij, HESS, 2022. doi: 10.5194/hess-26-5849-2022

Wang et al. WRR 2022. doi: 10.1029/2021WR031297.

Here you address the comparison of the capillary conductivity models specifically with respect to their behavior at the wet end. We use the "hclip approach" of Iden at al. (2015) instead of the Ippisch et al. (2006) approach (which is pretty much the same as the approach of Vogel et al. (2000)). The Iden et al. (2015) approach is well suited and applicable to any model of the water retention curve and (more important for this study) capillary conductivity model. Thus, the terminology "outdated" is simply wrong.

The two approaches (Iden et al., 2015 and Vogel et al., 2000 or Ippisch et al., 2006) mainly differ in their description at suctions below the critical suction, hcrit (0.06 m). In section 4.3, we show (i) the "un-clipped" (dashed lines) versions and discuss the different behavior of the 4 different K-models, if no correction (either the one of Vogel et al. or Iden et al.) is applied. We think this is worth being mentioned because many researchers do not use any of the corrections. We additionally show (ii) the "clipped" versions and discuss the conductivity extrapolation between h = 1 m ("wettest point where measured data are available") and h_{crit} (i.e. h = 0.06 m; "wettest point up to where the capillary flow model theory is applied"). In this moisture range we expect both correction approaches to behave almost identical. Notably, the hclip approach does not alter the K-course at suctions higher than h_{crit}. Thus, the difference of almost 2 orders of magnitude between the different capillary bundle models at h_{crit} is due to their different formulations.

Regarding the model of Wang et al, we refer to our reply to major comment 1.

Regarding the model of de Rooij (2022): De Rooij improved the model of de Rooij et al. (2021), which is a new retention model to describe the water retention from saturation to oven dryness, by using the approach of Ippisch et al. to improve the K-prediction near saturation. This model is combined with a capillary bundle model to predict the complete hydraulic conductivity curve. Thus, capillary and non-capillary conductivity is lumped. We will refer to this model in the introduction but would like to constrain our study to the PDI model family. Again, the focus is on the comparison of the 4 different capillary bundle models and not on the water retention models.

3. Some minor points that are not limited to a single place in the paper:
    - the color scheme of the graphs involves two very light shades that I could not see terribly well

      We tried to use a barrier free color scheme but agree that this can be improved. We will do so in the revised version.

    - the English needs a little work - commas appear in strange places, for instance. But nothing that hampers the readability of the text.

      Will be done

    - I do not think 'Table' is abbreviated in HESS, or in any other journal.

      Will be changed

    - The paper requires familiarity with earlier work by this group. The referencing is adequate, so readers can easily find the earlier papers if needed. I therefore do not consider this a problem.

      Yes, we agree. A complete repetition would be far beyond the scope. Note, however, that we tried to find the right balance by repeating the basics of the PDI model and its current improvements in the appendix.

4. I am in limbo about recommending minor or major revisions. Because the use of outdated WRC models really worries me and I realize that taking care of this will require some effort I am gravitating toward recommending major revisions. But the editor has the final say, of course.

   As outlined above, we will mention some of the suggested references but would like to limit our analysis to the PDI framework as we focus on the comparison of the capillary bundle models and 16 model combinations seem adequate to us for reaching the objectives of this study. However, we would be happy to see other authors pick up the thread and explore the approach further with alternative models (and perhaps more measured data).

Minor comments given in the annotated pdf

5. Line 50: that (without preceding comma)

   Thanks, will be changed

6. Line 62: Assouline

   Thanks, will be changed

7. Line 77 to 87: Some parts repeat the Introduction, sometimes almost verbatim.

   We will carefully consider how to shorten this paragraph without losing clarity.

8. Line 150 to 151: I am having a hard time understanding why the AS assumption leads to these expressions for tau-sub-s and beta. Some guidance would improve clarity.

   We understand and will provide a detailed derivation in the revised manuscript either in the appendix or the supplemental material.

9. Line 159 to 162: But they lead to very steep dK/dh slopes near saturation (Madi et al., 2018). Alternatives exist:
   Ippisch et al., Adv. Water Resour. 2006. doi: 10.1016/j.advwatres.2005.12.011
   de Rooij, HESS, 2022. doi: 10.5194/hess-26-5849-2022
   Wang et al. WRR 2022. doi: 10.1029/2021WR031297.
   N.B. Ippisch et al and de Rooij specifically set out the improve vGc, and Wang et al. aimed to improve FX. It seems you are using outdated models for which better versions are available.

   As indicated in reply to major comment 2, we use the "hclip" approach of Iden et al. (2015) where the pore sizes in the K-prediction are limited to a reasonable maximum value (d_{max} = 5 mm at h_{clip} = 0.06 m in our study). This solves this problem in a reasonable manner and does not alter the function at suction larger than hcrit. Contrary to the approach of Ippisch et al., the dK/dh slope at and near saturation will not be zero but it is finite, and contrary to the formulation without the "h_{clip}" the dK/dh cannot go to infinity. See also our reply to comment 2.

10. Line 170 to 171: Why not use improved versions of vGc and FX that have been published recently instead (see comment above). They take care of that problem for you.

    See our reply to comments 2 and 9.

11. Line 195 to 196: A goodness-of-fit test for the HCC, not 'prediction' in a functional sense, i.e., by running am Richards' solver to see how well it predicts fluxes and water contents in a soil profile. This comment does not imply that I suggest you add such a model test, but it would perhaps be good to clarify this in the text and the heading of section 3.3 to avoid confusion.

    In section 3.3, we do not describe another fitting but indeed **a full prediction** of the hydraulic conductivity curve on basis of the fitted retention function. This is in contrast to the calibration section.

    We will change the text to "*The HCC prediction performance of the various model schemes...*"

12. Figure 1: The conductivity data points extend to very low values for pF values larger than 5 in some cases. How were these values measured?

    Those data stem from Pachepsky et al. (1984) and the Mualem (1976b) soil catalogue. These data are used since the early work of Tuller and Or (2001) to parameterize and/or test SHP models over the full moisture range. Unfortunately, we cannot go into the depth of the cited data sources.

13. Line 231: Is this the number that is dimensional? What are the units?

    Thanks for pointing at this. This number has indeed the SI unit m. We will change that in the revised manuscript.

14. Line 238 to 239: Exactly. You cannot really compare it to the others, can you? It is a different parameter altogether.

    We cannot compare and interpret the parameter value. However, we can compare the prediction performance since we use the model structure and derive a single scaling parameter for each model combination.

15. Line 262 to 270: This is a helpful paragraph to see through the data.

    We thank you for this statement.

16. Line 285 to 291: Particularly for this element of the study it is unfortunate that you relied on outdated WRC models for which improved versions exist that were developed to address the shortcoming you bring up here. Even though you clipped the models, the updated counterparts might have done better (or worse - who is to tell?).

    Please see our replies to major comment 2 and 4.

17. Line 307 to 308: Should then the conductivity at the lower range not drop more slowly than CCG and Mualem? The smaller pores remain water-filled and are not interrupted by larger sections that have already emptied. If that is the case, it will possibly affect the fitted parameters in such a way that the less profound drop in K is countered by the fitted parameter values. This would then lead to a more pronounced increase of K near saturation for Burdine.

    Interesting thought. However, we also see an almost identical course for all 4 models in the medium moisture range. Maybe, the effect is counterbalanced by the tortuosity correction: In the wet range, the tortuosity correction S_c^2 is close to unity, whereas in the dry range it is very small.

18. Line 367: Note that Madi et al. (2018, supplement) warned that b should be small.

    Equation A.4 takes care that parameter b becomes not too large and not too small. Moreover, the statement of Madi et al. (2018) is again given in the context that the capillary conductivity model must not have an unphysical sharp drop near saturation. Most importantly, in the PDI system, the capillary conductivity is not affected by the non-capillary saturation because it is solely calculated from the capillary saturation function.

19. Line 367: What is this, or did I overlook something?

    Thank you, we indeed forgot to define/explain parameter ξ. It represents the chosen quantile of Sc for the derivation of ha (in our case ξ is 0.75). We will add that in the revised version.