Dear Erwin Zehe,

thank you for editing our manuscript. Below is a point-by-point response to the reviewers' comments. Any change made to the manuscript is indicated with reference to the line in the revised manuscript. We hope that we have clarified everything satisfactorily.Kind regards,

Andre Peters, Tobias Hohenbrink, Sascha C. Iden, Martinus Th. van Genuchten, and Wolfgang Durner

Dear Gerrit de Rooij,

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 have led 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, Tobias Hohenbrink, Sascha C. Iden, Martinus Th. van Genuchten, and Wolfgang Durner

Major comments

1. The paper is generally well-written and clear, and the contribution to soil physics is relevant and suitable for HESS. Below are a few (somewhat) major comments. These, in addition to minor comments, also appear in the annotated manuscript.

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

2. The Introduction is well-written and convincingly argued. I think the paper can be embedded in the literature a bit better. I provide two additional references that themselves have additional references that may be worthy of inclusion. I have been following the work of some of the authors, so I know they are well aware of developments in the literature. Perhaps they can use the depth of that awareness to add a few relevant papers. There is no need for a full-blown review though.

   Thank you for the suggestions. We will consider them carefully. We agree with the statement that there is no need for a full-blown review, and have tried to find the right balance between recalling model features that have already been published and the need to introduce readers that are not that familiar with the issue to some model basics. In general, we want to keep the presentation concise and want to focus on the innovative feature of our paper, i.e., the absolute prediction of capillary conductivity. We will, however, incorporate the two suggested references into the introduction.

   We introduced Assoline und Or (2013) in lines 50f and Madi et al. (2018) in lines 262f.

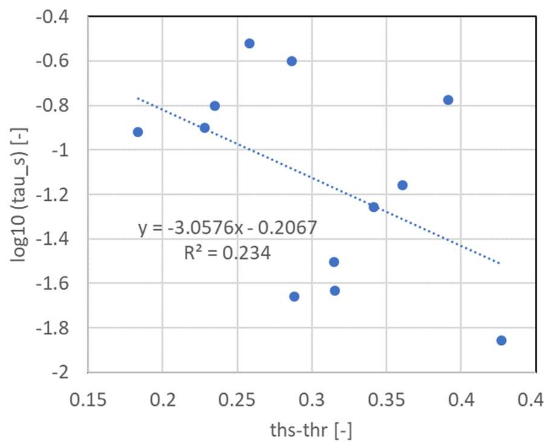
3. L. 201-202

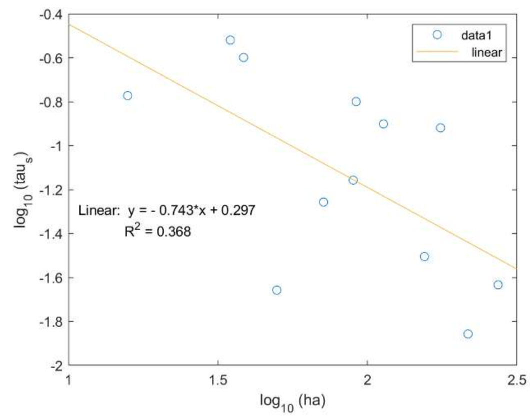
Does the hypothesis of a mildly varying tau_sub_s not implicitly require that the conductivity of a soil that is so wet that all but the largest pores (whose size and shape are determined almost entirely by the soil macrostructure) are filled does not vary much for different textures? I am not convinced that is the case, but have to admit that my reservations are based more on intuition than hard data. I understand the proof will come later in the paper.

The texture, and thus the pore size distribution, can be completely different within the framework of the developed theory. Thus, the predicted Ksc does not vary much within a texture class, but of course it varies greatly between textures. This, of course, also leads to very different conductivities throughout the moisture range. In contrast, the parameter tau_s would theoretically vary only in a very small range, even between soils of different textures (see lines 208ff). Note also that our interpretation of tau_s takes into account not only path lengthening due to tortuosity, but also other effects in general, such as different fluid properties near particle surfaces and pore linkages deviating from the assumption of the Mualem integral (see lines 214ff). Therefore, we determine it empirically and expect a somewhat larger variation than for a theoretically derived tau_s (considering only the orbital strain), but still a variation that is orders of magnitude smaller than for the classical Ks parameter.

4. Still, I would be interested in a more elaborate treatment of the implications of the range of tau_sub_s for the range of Ksc in Eq. (20). It seems to me that the additional variability in Ks must stem from the other terms in Eq. (20) except the constant beta. Perhaps dot plots of those terms for the soils for which you predicted Ksc could help. I am not sure if that is the best way to explore this, but the interaction between the three non-constant terms in Eq. (20) is of interest but largely neglected.

This is basically right and we are quite thankful for this comment. We even thought about introducing such a correlation. However, since this correlation can only be done for the 12 calibration data sets (which contain the sufficient information (see line 259)) we decided not to do so. Since beta is constant and (ths - thr) varies only moderately, the main correlation stems between tau_s and the shape of the capillary saturation function. Below you can see on the left the correlation between tau_s and (ths-thr) and on the right the correlation between tau_s and ha. Note that the parameter ha (air entry for the non-capillary part) is here given as the suction at which capillary saturation is 0.75 (outlined in appendix 2, Lines 477ff). We used this value because it lumps the effect of the shape parameters alpha, n, and m together. We see a weak correlation for both. Especially on the right-hand side there is a tendency for lower values for tau_s with higher values for ha (i.e. for fine textured soils).





However, in considering whether it is advisable to include this information in the paper, we feel that it would distract from the central content, the development of the theory. Therefore, we believe that this topic deserves its own discussion once more data is available to validate our model.

5. Eq. (20)

The presence of a residual water content confuses me a little given the tendency in the past 10 - 15 years or so to get rid of it. As a case in point, you quote Schneider and Goss, who provided a finite matric potential for oven-dryness. Does this not contradict the existence of a non-zero residual water content?

We are sorry that there is a misunderstanding here. The meaning of parameter thr in the PDI model is defined in Eq (2) and in the text (line 120) as the "maximum adsorbed water content". The water content of the non-capillary part decreases towards zero at h = 10^4.8 m according to Schneider and Goss. The PDI model thus covers the entire water content range from a maximum water content at saturation to zero at oven dryness. There is no residual water content in the conventional sense.

6. L.228

Please add some explanation for your choices for the capillary saturation functions. There are many alternatives, some of which are part of a set of curves that distinguish between adsorbed water and capillary-bound water. There are also versions without asymptote at zero or a non-zero residual water content. Given the argumentation in the Introduction, I did not expect equations with an asymptote to be included. Perhaps you indeed used the non-asymptotic version of Fredlund and Xing? If I recall correctly, they propose several functions, some of which have an asymptote at zero water content.

With the previous explanation, it is hopefully now clear that the capillary saturation function as part of the PDI model system is only that part of the overall retention curve that reflects the "capillary" pores, and that the asymptote marks the transition from the capillary-dominated to the "film flow"-dominated water regime. This is described in the manuscript (Section 2.1). Any sigmoidal curve that covers the range of 1 to zero can be chosen as the basic capillary saturation function. The fact that this function asymptotically approaches zero at a certain suction does not mean that the total water content converges towards this asymptote. For calculation of the capillary fraction of the conductivity function, we consider this capillary saturation function to be the correct function to be used in the Mualem integral.

We use in this paper the van Genuchten, the Kosugi and the Fredlund-Xing basic saturation functions for the capillary saturation part. These are the most commonly used functions in the field of soil hydrology (VG cited ~30'000 times in Scholar, Kosugi ~850 times) and geotechnics (Fredlund-Xing ~4300). Of course, other models, which account for decreased water contents towards oven-dryness exist and may be used. But we do not think that they add any further gain in knowledge to the aim of the paper, which is the prediction of total conductivity in the wet range. We refer to Table 2 for the mathematical expression of these (also the basic Fredlund and Xing) models and give a short explanation for the choice of the four capillary saturation functions we used in the revised manuscript (lines 239f):

"We chose these 4 functions because they are the most commonly used functions in the field of soil hydrology and geotechnics."

7. L. 263

Basically, you fit tau-s instead of Ks to fit the data everywhere except in the range near saturation. In effect you are still scaling a conductivity curve, you just call the scaling parameter something else.

The new element is that you use the fitted value as a predictor for soils where you did not fit it to. These soils each have their own values of the residual and saturated water contents, F(1), and F(Gamma-0), from which emerges an individual value of Ksc using Eq. (20).

Is this a correct description of the procedure? If so, it makes any relationships/correlations between the three non-constant terms of Eq. (20) even more interesting (see my earlier comment).

It is true that we „fit tau_s instead of Ks to fit the set of calibration data everywhere except in the range near saturation." As described earlier in the manuscript, the essence of our reasoning is that this tau_s parameter - unlike Ks - is independent or only weakly dependent on the pore-size distribution and we seek therefore a more or less "universal value" for it that is independent of texture and other parameters.

Eq. (20) was introduced to give a simple link between the classic scheme and our proposed scheme. And you are right, although tau_s is constant, Ksc is individual for the different soils, which is reflected in the integration if 1/h with respect to capillary saturation in Eqs. (18) to (20). For an explanation of the nature of tau_s, please refer to our response to major comment 3.

8. Figs. 5 and 6

Due to 'space limitations' you present only 6 of the predicted curves. Particularly in the box plots of Figs. 5 and 6, there seems to be plenty of space for more red dots. I would not mind seeing a few more, perhaps even all of them. But what is the meaning of the red cross in the vGc column of the left panel in Fig. 5?

Please note than all of the 23 curves are shown in the supplemental material, while the 6 examples are only randomly selected cases, shown for illustration in Fig 5. We are of course happy to present more or even all of the illustrations, but felt that they did not add any value to the paper if they were shown directly in this manuscript.

Minor comments

9. Lines 42 - 44: "Recently, Peters et al. (2021) combined the mechanistic models of Lebeau and Konrad (2010) and Tokunaga (2009) with the conceptual model of Peters (2013) to obtain a simple prediction scheme for the absolute non-capillary conductivity function Knc(h)."

   Perhaps, the work of Weber et al. should be mentioned here as well:

   Weber, T.K.D., W. Durner, T. Streck, and E. Diamantopoulos (2019): A modular framework for modeling unsaturated soil hydraulic properties over the full moisture range. WRR 55, 4994-5011. doi 10.1029/2018WR024584.

   Addendum: I see you mention this paper in l. 130.

   Since Weber et al. did not develop an absolute prediction scheme, mentioning it here would not be in the right context.

10. Lines 47- 48: "Today, the capillary bundle model of Mualem (1976a), who refined the assumptions of the CCG model, is most frequently used." Assouline and Or (2013) offer an insightful critique of this and similar models. (Assouline, S., and D. Or (2013): Conceptual and parametric representation of soil hydraulic properties: a review, Vadose Zone J. doi:10.2136/vzj2013.07.0121=

    Thanks for this useful hint, wel added the following to the manuscript (lines 50f):

    "see Assouline and Or (2013) for a critical review of this and similar models."

11. Line 78: Fig. 1 (add a period). Also I thnik HESS prefers labeling of panels in figures, so you can refer to the panel by its label here.

    Figs. 1, 3, 5, 8, and 10 have been updated with labels.

12. Line 185: "Applying his model to a variety of data, Mualem found empirically that $\lambda \approx 0.5$."

    This is one of the issues discussed by Assouline and Or.

    We are aware of this issue (see Peters et al., 2011). However, for a complete prediction, we need one certain value. For our calibration data set, the value of 0.5 worked quite well. For the test data set, the full prediction with $\lambda = 0.5$ worked also very well. This might be due to the fact that we use the Mualem model to describe only part of the data, which are dominated by capillary effects. In our view, including a discussion of λ would distract from the key point of the paper and thus rather complicate than add value to the paper.

13. Line 196: "…Mualem's integral (occurring in Equation (16) in Equation (12),…"
    Correct this, please.

    Agreed. We apologize for this negligence and corrected it (line 202).

14. Line 202 "We hypothesize that τs varies only moderately among different textures,..."

Does this not implicitly require that the conductivity of a soil that is so wet that all but the largest pores (whose size and shape are determined almost entirely by the soil macrostructure) are filled does not vary much for different textures? I am not convinced that is the case, but have to admit that my reservations are based more on intuition than hard data. I understand the rpoof will come later in the paper.

Neverthless, I would be interested in a more elaborate treatment of the implications of the range of tau_sub_s for the range of Ksc in Eq. (20).

See our reply to the major comments 3 and 4.

15. Eq. (20): $Ksc = \beta\tau s(\theta s - \theta r)2[F(1) - F(\Gamma0)]2$

The presence of a residual water content confuses me a little given the tendency in the past 10 - 15 years or so to get rid of it. As a case in point, you quote Schneider and Goss, who provided a finite matric potential for oven-dryness. Does this not contradict the existence of a non-zero residual water content?

Please see our reply to the major comment 5.

16. Line 220: "Therefore, we seek in this contribution empirically a value of."

...an empirical value...

Has been changed (line 225).

17. Line 228: "The basic capillary saturation functions are given by the function of Kosugi (1996), the constrained and unconstrained van Genuchten functions (van Genuchten, 1980), and the Fredlund and Xing (1994) saturation function."

Please add some explanation for your choices here. There are many alternatives, some of which distinguish between adsorbed water and capillary-bound water, and did away with the asymptote at the residual water content. Given the argumentation in the Introduction, I expected at least some of those to be included. Perhaps you indeed used the non-asymptotic version of Fredlund and Xing? If I recall correctly, they propose several functions, some of which have an asymptote at zero water content.

Please see our reply to the major comment 6.

18. Line 229: "the constrained and unconstrained van Genuchten functions (van Genuchten, 1980)"

What does that mean? m = 1-1/n for the constrained version, and n and m independent for the unconstrained version?

We refer to Tab 2, where it should become clear. Furthermore, we will change the text (line 234f) to

"...the van Genuchten functions (van Genuchten, 1980) with the usual constraint ($m = 1 - 1/_n$) and also in unconstrained form (m independent from n), and ..."

19. Line 229: "… and the Fredlund and Xing (1994) saturation function."

The version with the correction factor that ensures there is no asymptote at zero water content?

Please see our reply to the major comment 6. Tab 2 shows the functions we use. We use the basic function without the correction, because this is not required in the PDI model framework. To clarify this issued, we added the sentence (lines 235f):

"The latter is the function given in the last row of Table 2."

20. Lines 234 – 235: ---"and that closed-form expressions can be derived easily also for "classical" models that use a residual water content and neglect the non-capillary components." This suggest that none of the functions you chose have a residual water content, but they have, don't they?

Please see our reply to the major comment 5

21. Line 252: "Mualem's model (as all capillary bundle models) in combination with water retention models that gradually approach saturation can produce a non-physical sharp decrease in the hydraulic conductivity near saturation (…)"

Madi et al. (2018) developed a mathematical criterion that can be used to test individual WRC models.
Madi, R., G.H. de Rooij, H. Mielenz, and J. Mai (2018): Parametric soil water retention models: a critical evaluation of expressions for the full moisture range. HESS 22, 1193-1219, doi 10.5194/hess-22-1193-2018.

Thank you. We added this information in the revised manuscript (lines 262f):

"Madi et al. (2018) developed a mathematical criterion to test individual WRC parameterizations for physical plausibility."

22. Line 262: "Details about the soils are given in the original literature and are summarized in Table 3. For each of the 4 models in Table 2, we determined a value for τs by fitting them to the 12 data sets and estimating the WRC parameters and τs."

Basically, you fit tau-s instead of Ks to fit the data except in the range near saturation. In effect you are still scaling a conductivity curve, you just call the scaling parameter something else. The new element is that you use the fitted value as a predictor for soils where you did not fit it to. These soils each have their own values of the residual and saturated water contents, F(1), and F(Gamma-0), from which emerges an individual value of Ksc using Eq. (20).
Is this a correct description of the procedure?

Please see our reply to the major comments 3 and 7.

23. Table 3

Note that Mualem did not use the original lab-measured data points but instead used smooth curves through these points to derive the points presented in his catalogue.

Thanks for the hint. Not many know this but we are aware of this.

I am unable to find the source paper (Pachepsky et al., 1984). The only link Google dug up leads to another paper.

We are sorry about that and admit that the early original paper is hard to find on the internet. We have a pdf of that publication and upon request are happy to share it.

24. Fig. 5: Is there a red cross here? What does it signify?

Point well taken - the red cross indicates an outlier. Following the default setting in Matlab® (https://de.mathworks.com/help/matlab/ref/boxchart.html), data are treated as outlier if they are 1.5 inter quartile range away from the top or bottom of the box. We will explain this in the figure caption in the revised manuscript:

"*The red cross indicates an outlier, defined by the Matlab® default settings as 1.5 times the inter quartile range away from the top or bottom of the box (*https://de.mathworks.com/help/matlab/ref/boxchart.html)*)*."

25. Line 353: In such cases, λ might be estimated and only τs might be fixed.

Assouline (2010) even set it to zero. See Assouline and Or (2013) for a discussion of this and the full reference to Assouline (2010).

Setting it to zero means that the tortuosity is independent of saturation. We think that this is not a preferable choice.

26. Line 370: "Following Jarvis (2007), we may choose for this a suction of about 0.06 m (pore diameter approximately 0.5 mm) up to which the macropore conductivity can be neglected."

Repeats earlier text (except the publication year).

The repetition here is intentional to make the manuscript more readable. The publication of the paper year is indeed 2007. We apologize for this negligence and correct it 1n line 265.

Dear John Nimmo,

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 have led to considerable improvement of our work and thank for this constructive input. In few cases, where we disagree with your assessment, we provide a detailed justification for not following your recommendations. For convenience, we numbered the comments.

Kind regards,
Andre Peters, Tobias Hohenbrink, Sascha C. Iden, Martinus Th. van Genuchten, and Wolfgang Durner

Major comments

1. This paper addresses a very important longstanding problem. The prediction of unsaturated hydraulic conductivity from soil water retention measurements requires specific hydraulic conductivity information in addition to the retention data. The authors correctly explain that in practice the required information is unavailable or is available only in a form, for example a measurement of saturated hydraulic conductivity, that is inappropriate for this use and thus produces erroneous results.

   The main innovation here leading to an absolute hydraulic conductivity curve (HCC) is what the authors call an absolute tortuosity coefficient. A second necessary element is their finding that a universal value of a saturated tortuosity factor can be suitable for a wide range of soil types. Together with a well-known relation put forth by Mualem (1976), these permit establishment of an unsaturated HCC without requiring hydraulic conductivity measurements. It is a very useful contribution that the proposed model is formulated in such a way that it can utilize an approximate though adequately representative universal value of an appropriate parameter.

   This paper's treatment of the problem and the conclusions drawn are sound. The results have real utility for scientists working with water flow in soils. The text is easy to understand. With revision this can be a strong contribution to be published in HESS. Below I recommend some important clarifications and modifications to be made.

   We very much appreciated the above comments.

2. Noncapillary:

   Use of the term "noncapillary water" raises some problems. There is a need to recognize the effects of noncapillary processes at the wet end of the unsaturated range. I see the chief issues as these: Being inversely proportional to pore radius, capillary force in the largest pores is so weak as to be dominated by gravity and instability of air-water interfaces. Consequently, in soil that is nearly saturated, some of the water in large pores is present not because it is held by capillary forces but because it is supplied by water flowing from a copious supply, typically driven by gravity. Such water can be considered an aspect of nonequilibrium flow (e.g. Jarvis, 2007, European Journal of Soil Science, 58:523–546). Capillarity, exemplified by such phenomena as capillary rise, is inherently an equilibrium concept and thus is inappropriate as a classification for the soil water near saturation that is in a nonequilibrium state. This is a major factor underlying a central problem that this paper addresses, that measurements of saturated hydraulic conductivity, normally made under ponded or nearly-ponded conditions, are not suitable for use as a matching factor when predicting unsaturated hydraulic conductivity from retention data. Values obtained in such measurements are reflective largely of flow through pores that are filled not by capillarity but by the dynamics of inflowing water. Thus they have an incompatible mismatch with information from capillary-based water retention data.

These considerations point to a feature of the PDI model system, that it presumes the capillary range extends up to saturation. It should be revised, but revision of the PDI system is not the point of this paper, and I am not recommending an extension of scope for it to additionally evaluate the matter of noncapillary water at the wet extreme. That would be a substantial topic suitable on its own for future research papers. I also recognize that the main body of the work here is in harmony with the basic understanding I've described above. The statement in lines 221-224 in effect acknowledges this point. The paper appropriately refers to an emphasis on properties of soil matrix material (e.g. lines 23, 150, 369, and elsewhere in section 4.4), where capillarity is a dominant controlling factor. What I do recommend is that there be direct acknowledgement and description, early in the introduction, of what could also be termed noncapillary water at the wet extreme (or alternatively called "nonequilibrium water" or some other term).

We are very thankful for this insightful comment and will address in the revision the issue of non-capillary water at the wet end of the hydraulic curves. We added (line 32ff):

"*In this paper, the term "non-capillary" is used only for water held by adsorption, although water in very large pores (i.e. larger than 0.3 mm in diameter (Jarvis, 2007)) is also not held by capillary forces*."

3. Tortuosity:

Path extension, as implied by the term "tortuosity", is not the sole cause of deviations from a straight-tube model. This is well stated in 215-217. It would be good to give this concept more prominence, for example mentioning it when tortuosity is first discussed quantitatively with eq. 11.

We had already thought about the optimal positioning when writing the initial draft. It is currently placed where we state why we want to estimate tau_s empirically rather than deriving it from theoretical considerations. This proximity seems important to us in the presentation. Nevertheless, we will add the following sentence below Eq. (11):

"*Note that deviations from flow in straight capillary bundles are not only affected by tortuosity in the strict sense, but by additional effects which will be discussed in section 2.3 within the context of model development*".

Support for near constancy of τ_s is from results in Fig. 3. The key fact is in line 315, that even though 1.5 orders of magnitude may sound like much, it is small compared to the variability of K_s among soils. The K_s variability among soils, however, is based on a large number of measurements, many of which are dominated by effects of pores larger than about 0.5 mm in diameter. This fact would be worth pointing out, perhaps noting that the procedure used here may provide a result similar to what would result from excluding K_s data from soils with large pores from use as a K matching factor.

We agree that the variability of Ks in natural soils is caused largely by structural pores (effects of pores larger than 0.5 mm in diameter, which gives somehow similar highest values for soils of different textures) but it depends also on the pore-size distribution (soil texture), which controls more the lowest observed values. Thus, even without structural pores, Ks will vary much more than tau_s (see also Eq. (20)).

Our procedure provides estimates of hypothetical Ks values that are only due to soil texture (called Ks_matrix, see line 372). The results might be similar to what would result from excluding near-saturated and saturated conductivity data when fitting the classic conductivity function to data.

Following your suggestion to include this thought into the manuscript, we rewrote section (lines 327ff):

"When fitting the classic PDI scheme (with K_S as a fitting parameter) to these data, which do not include measured conductivity data at saturation, the estimated K_S values varied by more than 3 orders of magnitude. In natural soils, the measured K_S values can vary even more due to the dominance of (texture-independent) structural pores and macropores on K_S (e.g. Usowicz and Lipiec, 2021)."

4. Deletion and additions:

The specific values of fitted parameters, τ_s, and goodness-of-fit values for all tested soils would be very useful for understanding and evaluating the proposed models. These values should be included in a table in the main text. One particular benefit from such a table is that it would allow the reader to assess possible trends with soil type in the spread in τ_s values seen in Fig. 4. Similar reasoning applies to figs. 5 and 6.

Yes, we will provide the respective data in a table, but it has many entries (48 lines with parameters). We added Table A1 in the appendix.

The value of section 4.3 is doubtful. I recommend omitting it. The statement in 349-350, which suggests the new prediction scheme is worthwhile only for the case of unavailable or insufficient conductivity information, understates its value. Even though they are widely used for this purpose, saturated hydraulic conductivity measurements are in general inappropriate for use in predicting conductivity in the capillary range, and this new scheme usefully provides a way to avoid using them. I also do not see value in attempting to predict saturated hydraulic conductivity, as in Fig. 8, with the approach developed here. This approach is closely tied to capillary phenomena, which are not what dominates saturated hydraulic conductivity in the most general case. One additional point is that I don't think this paper is a good place to mention possible adjustments of Mualem's λ parameter. With his recommendation of setting λ at 0.5, Mualem saw the value in recognizing what can be regarded as essentially the same for a range of situations. This was a positive development analogous to the present work's recognition of a universal value of τ_s.

This section treats the important issue of applying the new scheme not only as a pure predictive model, but also as an improvement in combination with the commonly used evaporation method. The statement on lines 349-350 ("Thus, the new prediction scheme should only be viewed as a good approximation if no or not sufficient conductivity information is available.") was misleading. We rephrased this section (lines 365ff):

"*The new scheme is valuable not only for cases where no or insufficient information about the conductivity is available. It is also useful when data are available for the unsaturated hydraulic conductivity, but are missing in the wet range. This is the case, for example, with the commonly used evaporation method (Schindler 1980; Peters and Durner, 2008; Peters et al., 2015).*

Accordingly, we rephrase the title of 4.3 to "Improved estimation of K-functions when K-data are available

As the authors note (lines 383-384), the development in Section 4.4 has potential value for improving the performance of numerical models if near-saturated conditions are important. This is an appropriately modest objective, given that this development is based mainly on mathematical convenience, with attention to physical plausibility, and has extremely little support from measurements. Even so, it should explain what need there is for a nonzero h_s value and the reasons for assigning it a value of 0.006 m.

As you correctly note, this interpolation scheme is purely heuristic. We suggest to interpolate on a logarithmic scale, therefore hs = 0 is not possible. We chose hs = 0.006 m (pore diameter 5 mm) as the value for full saturation but other values for hs would probably be as good (see lines 399ff).

Minor comments:

5. 100: "The total hydraulic conductivity function . . ." Here, "total" is potentially misleading. Better would be "The unsaturated hydraulic conductivity function . . ." or more vague wording.

   Has been changed (line 104).

6.
7. 120: A commendable example of terminology that improves on most unsaturated zone literature is the definition of θ_r as the "maximum adsorbed water content", as opposed to the undefined "residual water content".

   Thanks for that comment!

8. 160: Missing word "conductivity."

   Has been corrected (line 164).

9. 174: Clearer wording would be "Since the degree of capillary saturation . . ."

   Has been changed (line 180).

10. 196: Clarify "(occurring in Equation (16) in Equation (12),"

    Has been corrected (line 203).

11. 256: Jarvis (2007)

    Has been corrected (line 265)

12. 262-263: How is τ_s determined, when it does not appear in the WRC equations that are fitted?

    Thanks for that comment. Here we were unprecise. As indicated at the beginning of the paragraph, here we matched the WRC parameters plus τ_s for each of the four PDI models. The data to be matched were the WRC data and the HCC data in the wet region (but not at saturation for the known reasons).

    We changed the sentence as (line 271f):

    "For each of the 4 PDI combinations with the capillary saturation functions given in Table 2, we determined …"

13. Fig. 2: Scale and other design elements are inappropriate. Fitted curves fall largely on top of each other. Moreover, the differences in color are subtle and hard to distinguish. All graphs need to be much larger. I recommend choosing only one soil for this figure and putting the rest in supplementary material.

We basically agree. We are aware of the problem and want to find a best-possible solution. We show now 4 of the soils in the main manuscript and all of them are given in the supplemental material.

14. 350: Awkward wording. I suggest ". . . if conductivity information is unavailable or insufficient . . ."

We change this sentence since it was misleading (see reply to comment 4). We wrote (lines 365ff):

"The new scheme is valuable not only for cases where no or insufficient information about the conductivity is available. It is also useful when data are available for the unsaturated hydraulic conductivity, but are missing in the wet range. This is the case, for example, with the commonly used evaporation method (Schindler 1980; Peters and Durner, 2008; Peters et al., 2015).

15. 400: Qualifier needed: ". . . such predictions mostly use . . ."

Has been corrected (line 419).

16. 406-411: Confusing use of the term "tortuosity." I suggest presenting facts in this order: (1) Pore bundles do not in themselves account for important characteristics such as path elongation due to tortuosity, physical properties of the liquid phase, ... (2) These effects can be accounted for with a parameter that here is called a tortuosity coefficient. (3) We separate this parameter into two factors, a relative and a saturated tortuosity factor. (4) The saturated tortuosity factor is shown to vary little among different soils, and we have determined a universal value empirically from data.

Well sorted! We followed your suggestions in the revised manuscript (lines 424ff).

"Pore-bundle models do not in themselves account for important characteristics such as path elongation due to tortuosity, surface roughness of pore walls, non-circular capillaries, dead-end pores or physical properties of the liquid phase, etc. These effects can be accounted for with a parameter that is called tortuosity coefficient. We divide this parameter into two factors, a saturated tortuosity factor, and a relative tortuosity function that takes the dependence of tortuosity on water content into account. The saturated tortuosity factor is shown to vary little among different soils, and we have determined a universal value empirically from data."

17. 412: This would be a good place to point out that the saturated tortuosity factor with an assigned universal value provides the basis for estimating conductivity from retention data without a matching factor that can only be determined from a measured conductivity value. The last two paragraphs of the abstract did this very well.

Has been done (lines 429f):

"The new scheme using a saturated tortuosity factor with an assigned universal value can be used to predict the hydraulic conductivity curve from the water retention curve when insufficient or no conductivity data are available."

18. 431-491: I do not find the appendices to be strictly necessary but they may serve as a convenience to some readers.

    We would like to keep them, mainly due to two reasons: (i) this is short summary of the PDI system, which might be of convenience for those readers who are not familiar with the PDI model system, (ii) section A1.2 deals with the K_nc prediction scheme of Peters et al. (2021), and (iii) section A.2 describes the scheme to determine the parameter ha, which differs from formerly published papers.