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.

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 will write:

"The range of τ_s values for the 12 data sets spanned less than 1.5 orders of magnitude. We interpret this as an indication that the hypothesis of relatively moderate overall variability in τ_s may be justified. When fitting the classical PDI scheme (with Ks as the fitting parameter) to these data, which are without measured K data at saturation, the estimated Ks values varied more than 3 orders of magnitude. In natural soils, measured Ks values can vary by even more due to the dominance of (texture-independent) structural pores and macropores on Ks (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 therefore will place it 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 will rephrase this sentence:

"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 will 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 382-383).

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.

   Will be changed.

6. 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!

7. 160: Missing word "conductivity."

   Will be corrected.

8. 174: Clearer wording would be "Since the degree of capillary saturation . . ."

   Will be corrected.

9. 196: Clarify "(occurring in Equation (16) in Equation (12),"

   Will be corrected.

10. 256: Jarvis (2007)

    Will be corrected

11. 262-263: How is τ_s determined, when it does not appear in the WRC equations that are fitted?

    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).

12. 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 will show 4 of the soils in the main manuscript and the other 8 will be put to the supplemental material.

13. 350: Awkward wording. I suggest ". . . if conductivity information is unavailable or insufficient . . ."

    We will change this sentence since it was misleading (see reply to comment 4). We will write:
    "*The new scheme is valuable not only for cases where no or insufficient information on conductivity is available. It is also useful when data are available for unsaturated hydraulic conductivity, but 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).*"

14. 400: Qualifier needed: ". . . such predictions mostly use . . ."

    Will be corrected

15. 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 will follow your suggestions in the revised manuscript.

16. 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.

    Will be done

17. 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.