Dear Mauro Giudici,

Thank you for handling our manuscript. In the following we reply to each of the 3 reviewers and indicate where we made changes in the manuscript. For convenience, we indicate in which lines we changed the text of the revised manuscript and rewrite the text passages directly in this document. We are convinced that the comments of reviewer #1 and John Nimmo considerably improved our work and thank them for their constructive input. Regarding reviewer #2, it was hard to filter constructive elements out of his/her review. As will be shown, we found 4 points, which we used to improve our manuscript.

Additional to the changes made upon the reviewer's suggestions, we slightly changed the text at several places to improve the readability. Moreover, we added zoomed subplots to figure 4 in order to better visually distinguish between the different capillary bundle models.

Kind regards,

Andre Peters, Sascha C. Iden, and Wolfgang Durner

Replies to Reviewer #1:

Dear Reviewer #1,

again, 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 appreciate this constructive input. In those cases, in which we disagree with your assessment, we provide a 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 and indicate in which lines we changed the text of the revised manuscript.

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:

2. 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 SHP models. As a side note, the proposed functions add up to a very large number (>40) that have been proposed in the past. Most of them have not been tested by independent researchers. Your short selection is arbitrary from our point of view. We refer to Weber et al. (2019) and Li et al. (2023) in the revised manuscript (Lines 32-35):

"*Therefore, more recent models extend these SHP models (e.g., Tuller and Or, 2001; Peters and Durner, 2008; Lebeau and Konrad, 2010; Zhang, 2011; Peters, 2013; Weber et al., 2019; de Rooij et al., 2021; de Rooij, 2022) to account for these processes. Over the last 10 years, a variety of SHP models have been proposed, see e.g., Li et al. (2023) and the references therein.*"

In the following, we repeat our brief explanation why we did not consider some of your suggested models:

Li et al. (2023): We did not include Li et al. (2023) because they do not propose models which account for non-capillary storage and conductivity. The authors introduce several sigmoidal functions for "basic retention functions" (in our terminology). These functions will not perform better than the Fredlund-Xing (FX) or the van Genuchten with flexible parameter m (vGm) basic retention functions as they have only two free fitting parameters for the saturation function (as the van Genuchten model with $m = 1 - 1/n$, and the lognormal Kosugi models in our study).

Wang et al. (2022) and Rudiyanto et al. (2020): Both models build up on the Wang et al. (2018) model. This model is inconsistent: it combines the FX retention model with the analytical formulation of Mualem's capillary model, obtained for the constrained van Genuchten retention function. This means that the hydraulic conductivity function is decoupled from the water retention curve and this contradicts the theory upon which our models are built.

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.

3. 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 misbehavior of capillary conductivity models close to water saturation. This issue has been first addressed by Vogel et al. (2000) and was discussed in a broader context by Ippisch et al. (2006) who also presented a general solution to the problem. We use the "hclip approach" of Iden at al. (2015) instead of the Ippisch et al. (2006) approach. In the Vogel-Ippisch model, an explicit air-entry value is introduced to the water retention function, in the hclip-approach, the retention function remains unchanged but a maximum pore size is introduced in the capillary bundle model. Therefore, the issue of misbehavior close to saturation is addressed in our manuscript and it is not "outdated". 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. We describe the hclip-approach in more detail in the revised manuscript (Lines 197-204):

"Capillary bundle models can lead to unrealistic drops in the HCC close to water saturation if the pore-size distribution underlying the WRC is wide (e.g., Vogel et al., 2000, Ippisch et al., 2006, Madi et al., 2018). To prevent such unrealistic decreases of $K(h)$, we applied the "hclip" approach" of Iden et al. (2015). In this approach, an upper bound for the pore size is assumed in the conductivity calculation by the pore-bundle models. This is equivalent to limiting the suction to a minimum value h_{crit}, i.e. setting $h = min(h, h_{crit})$ in Eqs (8) to (11). For the Mual model, this leads to:

$$\beta \tau_s {S_c}^{0.5} (\theta_s - \theta_r)^2 \left[\int_0^{S_c} (min(h, h_{crit}))^{-1} \, dS_c\right]^2 \quad (17)"$$

The effect of the hclip-approach on the hydraulic conductivity curve near saturation is illustrated very clearly in Figure 6 (right) and discussed in section 4.3 entitled "Behavior of the capillary bundle models in the wet range". We see no reason to extend the discussion further or to include other models in our study because the problem of the unrealistic behavior close to saturation does not exist in our models.

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"). 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 4 different capillary bundle models at h_{crit} is due to their different formulations.

To clarify this behavior, we added the following sentence in the methods section (Lines 206 to 207 in revised manuscript):

"Within the context of the proposed absolute prediction scheme, the "clipped" models are identical to the "unclipped" models for suctions exceeding h_{crit}."

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 are lumped. We refer to this model in the revised introduction (see reply to comment #2), but would like to constrain our study to the PDI model family. Again, the focus of our paper is on the comparison of the 4 different capillary bundle models and not on the water retention models.

4. 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

      Thanks for your hint. We have updated the color scheme 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.

      We carefully corrected the text and hope that the English is now sound.

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

      Has been changed, thank you.

    - 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.

5. 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 mentioned earlier, we disagree with the assessment that our models are "outdated." While we have included some of the suggested references in the revised manuscript, we have chosen to restrict our analysis to the PDI framework. Our primary focus is on comparing the capillary bundle models, and we believe that 16 model combinations are sufficient to achieve the objectives of this study. Of course, we would welcome other authors to explore alternative models (potentially with more measured data) and further develop this approach

Minor comments given in the annotated pdf

6. Line 50: that (without preceding comma)

   Thanks, has been changed to “*In this work, we focus on the models that derive the pore-size distribution from the capillary water retention function and …*” (Lines 51 to 52 in revised manuscript).

7. Line 62: Assouline

   Thanks, has been changed (Line 64 in the revised manuscript)

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

   We tried to carefully shorten this paragraph without losing clarity (Lines 97 to 104 in the revised manuscript).

9. 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 provide a detailed derivation in the revised manuscript in the appendix A2. We refer to it in line 177.

10. 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.

11. 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.

12. Line 195 to 196: A goodness-of-fit test for the HCC, not 'prediction' in a functional sense, i.e., by running a 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 **an absolute (**in the original version “full”**) prediction** of the hydraulic conductivity curve on basis of the fitted retention function. This is in contrast to the calibration section.

    We changed the text to

    “*The HCC prediction performance of the various model schemes…*” (Line 229 in the revised manuscript)

13. 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.

14. 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 added the following sentence in the revised manuscript:

"*Note that τ_s has the unit [m] for the AS model, whereas it is dimensionless for the other models.*" (Line 267)

Furthermore, we changed Figs. 3 and A2 and Table 6 accordingly.

15. 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.

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

    We thank you for this statement.

17. 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.

18. 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 term S_c^2 is close to unity, whereas in the dry range it is very small.

19. 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 given in the context that the capillary conductivity model must not have an unphysical sharp drop near saturation. 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.

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

    Thank you for bringing this to our attention. We inadvertently omitted the definition and explanation of the parameter ξ. This parameter signifies the selected quantile of Sc used in the derivation of ha, and in our case, ξ is set to 0.75. We have included this clarification in the revised version of the document (Lines 431 to 432).

Replies to Reviewer #2:

Reviewer #2 provided a critical assessment of our manuscript, and their feedback did not include many specific suggestions for improvement. We only found the following few hints, which we tried to account for:

- The two references Jackson (1972) and Jackson et al. (1965). We discuss them together with the 4 references suggested by John Nimmo in lines 66 to 88 in the introduction section.
- we substituted the words “full prediction” to “prediction of absolute…” in the title and text.
- “*I have other specific comments regarding presentation (similar colors of lines with no legends)*”: We use a new color scheme and introduce a legend to Fig. A4.
- We thoroughly reviewed the entire text but did not identify a 'gradual erosion of qualifying statements'. In the introduction, we explicitly clarify that the prediction of non-capillary conductivity (Peters et al., 2021) is grounded in physics, while we do not make the same assertion for capillary prediction, neither in the introduction nor throughout the manuscript

Unfortunately, we did not get other constructive comments which could be considered in the revised manuscript.

Replies to John Nimmo:

Dear John Nimmo,

thank you very much for reviewing our manuscript and for providing such a constructive input. We have considered all of your statements carefully. Please find below our detailed answers to all comments. We are convinced that your comments led to considerable improvement of our work. For convenience, we numbered the comments.

Major comments

1. This paper evaluates the relative merits of four different capillary bundle models, applied within the framework established in the earlier paper P23 (Peters and others, 2023), for predicting unsaturated hydraulic conductivity from retention data. The tests are rigorous and conducted for data from twenty-three widely different soils, and using four different commonly used formulas for representing the soil water retention. The results are satisfyingly definitive in showing that the Mualem model gives the best results. Another contribution of this paper is in exploring and elucidating the function of the saturated tortuosity coefficient introduced in P23.
   An important insight revealed in lines 230-241, and noted in line 179 and elsewhere, is that the value of the saturated tortuosity coefficient τs depends on the particular conductivity model it is used with. This is not surprising, though it brings out the fact that unless τs can be computed independent of a K model, it is not universal and not a property of the medium. This feature seems

at odds with the hypothesis of a universal value as described by P23. It should be explained and perhaps elaborated in the discussion.

Thank you for this important comment. We have added a brief discussion to the revised manuscript that the value of τ_s depends on the capillary bundle model. Actually, this is in accordance with our previous interpretation of τ_s in Peters et al. (2023):

"In real soils, however, the deviation from flow in straight capillary bundles is not only affected by tortuosity in the strict sense but also by other soil-related factors such as the surface roughness of pore walls, non-circular capillaries, and dead-end pores. Additionally, not only the geometry of the pore space may differ from the ideal case but also such fluid properties as surface tension and viscosity likely will be different from those of pure free water. Finally, capillary bundle models will not represent the pore distribution and connectivity in an ideal way. Therefore, we seek in this contribution an empirical value of τs that lumps all these effects." (Peters et al., 2023, section 2.3).

Your remark is a welcome add-on to the current manuscript. We added the following sentences to the theory section:

"P23 discussed that τ_s does not only describe the saturated tortuosity in the strict sense (eq. (1)), but lumps also other soil- and fluid-related factors, i.e. the surface roughness of pore walls, effects of non-circular capillaries, dead-end pores, and deviations of surface tension and viscosity of the fluid from those of pure water. Moreover, the chosen capillary bundle model will not represent the pore distribution and connectivity in an ideal way." (Lines 160 to 164 in the revised manuscript)

We furthermore extend the first sentence, which introduces this fact in the results section (lines 261 to 264 of the revised manuscript):

"*Figure 3 shows that the different conductivity prediction models give different optimal values for the saturated tortuosity coefficient, τ_s. This is in accordance with the discussion of the nature of τ_s in Peters et al. (2023) who acknowledge that the notion of a universally applicable saturated pore tortuosity is untenable. Rather, it must be seen as a general parameter in the context of the specific conceptualization of a capillary bundle model*."

2. This paper needs added material in the introduction or a separate section that reviews previous tests and comparisons of capillary bundle models (e.g. van Genuchten and Nielsen, 1985; Hoffmann-Riem and others, 1999; Kosugi, 1999). That will help make clear the context of this work and the contribution it adds to the existing literature. Although I hesitate to mention my own work in a manuscript review, a paper of mine (Nimmo and Akstin, 1988) is directly relevant and has some parallels with the present work. In it, we tested four capillary bundle models, three of which are among the four tested in this new manuscript. Our test was done on different samplings of identical soil material, with variations in packing and preparation to produce samples that varied modestly in porosity and hydraulic properties. As in the present work, the model of Mualem (1976) was found to be preferable. The test made a convincing demonstration of the basic utility of capillary bundle models in showing that measured retention curves for different samples, plugged into the capillary bundle models, gave rise to predicted conductivity curves that differed from each other, in direction and in approximate magnitude, in the same way the four sets of measured conductivity data differed. At the time of that study, this result raised my previously dubious regard for the usefulness of capillary bundle models.

Thank you for your valuable comment which adds a lot of useful information to the study and discussion. We added the following paragraph to the introduction:

“Several comparisons of capillary bundle models have been published. Jackson et al. (1965) compared four models, which are all variations and modifications of the original CCG model, and either predicted the absolute hydraulic conductivity or used one matching factor to scale $K_r(h)$. In their work, the predictions overestimated the conductivities drastically, and the CCG version of Millington and Quirk (1961) with a matching factor gave the best results. Jackson (1972) compared the CCG model versions of Millington and Quirk (1961) and Marshall (1958), which differ in the way tortuosity and pore connectivity are accounted for, by predicting $K_r(h)$ and scaling it with the measured K_s as a matching factor. He found that the models either over- or underestimate $K(h)$ and suggested an intermediate value for the tortuosity and pore connectivity term. Van Genuchten and Nielsen (1985) compared the Mualem (1976a) and Burdine (1953) models in terms of predicting $K_r(h)$ and found the Mualem (1976a) model to perform better. Nimmo and Akstin (1988) compared the models of CCG, Purcell (1949) adapted by Gates and Lietz (1950), Burdine (1953), and Mualem and used one measured unsaturated conductivity as a matching factor. They found, by visual inspection, that the model of Mualem outperformed the other models. Kosugi (1999) compared the Burdine and Mualem models to predict $K_r(h)$ with his generalized version of the Mualem and Dagan (1978) model, which was first fitted to the data to obtain the general parameter values. Not surprisingly, his version outperformed the predictive models. Moreover, the Mualem model performed better than the Burdine model. Hoffmann-Riem et al. (1999) fitted also a general version of the Mualem and Dagan (1978) model to data and compared it with the models of Mualem and Burdine. They concluded that a fit of the models to data should be conducted to obtain a good description. Finally, Madi et al. (2018) compared the capillary bundle models of Burdine (1953), Mualem (1976a), and Alexander and Skaggs (1986) in terms of their applicability in predicting $K_r(h)$. They found that the Alexander and Skaggs model strongly overestimated $K(h)$ for most soils, whereas the performances of the Burdine and Mualem models were superior. None of these studies considered non-capillary conductivity. Moreover, besides the comparison of Jackson et al. (1965), none of the studies conducted a prediction of $K(h)$ without adjusting conductivity parameters.” (Lines 66 to 88 in revised manuscript)

Furthermore, the following sentence was added to the discussion:

“These results support the findings of van Genuchten and Nielsen (1985), Nimmo and Akstin (1988), and Kosugi (1999), who also found the Mualem model to perform best in their model comparisons.” (Lines 306 to 308)

3. Though the work in this manuscript shows little real innovation, it has value in its thorough testing of widely used models and in providing helpful information for anyone considering the hydraulic conductivity-predicting model put forth in P23. It should be published after moderate revision.

   We agree that this study is less innovative than the previous study (P23) but agree also with the notion that it adds to scientific knowledge.

4. other comments:
   - 28: Reword. Functional form is not mandatory. There are alternatives, like tabulated values, though little used.

     “Mandatory” has been replaced by “useful” (Line 28 in revised manuscript).

- 33: “Any liquid flow ceases” is too definite a statement. Better to just say vapor flow becomes the dominant transport process.

  You are right. Has been changed accordingly (Line 35 in revised manuscript).

- 50: Seems like a misplaced comma.

  Has been changed to “*In this work, we focus on the models that derive the pore-size distribution from the capillary water retention function and …*” (Lines 51 to 52 in revised manuscript).

- 92: Better to say “particles” than “molecules” because molecules are subject to Brownian motion and do not individually follow a streamline.

  Right. But we hesitate to name it “particles”, because in our understanding (we are non-native speakers), the term is referring to the solid phase. Hillel uses the term “(flickering) clusters” (Hillel D. 1988. Environmental Soil Physics, Academic Press, page 26). In the revised manuscript we use now “parcels of water” (Line 109).

- 99: Subscript sc is reversed in equation.

  Has been adjusted in Eq. (2).

- 133: Define θs and θr.

  Has been done (Line 155 in revised manuscript).

- 161: Insert “among”—they are among the most commonly used . . .

  Has been done (lines 188 in revised manuscript).

- 220: In Fig. 1, curves for CCG and Bur are faint and hard to see—should be thicker. Also colors should be different to show more contrast than between the blue and green shown. Similar effects in Fig. 4.

  We originally tried to use a barrier free color scheme but agree that this can be improved. In the revised version, we use a new color scheme.

- 311: To help the reader, for the left side of Fig. 6, note briefly what is different to give four slightly different retention curves when the same FX model is used for each.

  Thank you for your comment. To clarify, we fitted the model combinations, which include both the retention and conductivity models, to the data. While we consistently used the same parameterization for the retention model (alpha, n, and m), it's important to note that these retention parameters also influence the shape of the conductivity curve. As a result, the retention fits may differ slightly for the various model combinations. We introduced the following sentence in the revised manuscript:

  “The WRC fits differ slightly for the different model combinations although we used always the same retention model because the retention parameters α, n and m influence the shape of both hydraulic functions, which were simultaneously fitted by minimizing eq. (18).” (Lines 328 to 331)

- 345-387. Appendix A1 is highly duplicative of original publications and the appendix in P23. It should be omitted, except possibly for part A1.3. The material in part A1.3 might be better placed in the main text.

You are correct that this section is somewhat duplicative, and we have thoroughly considered your suggestion internally. However, we believe that Part A1.3 cannot be fully understood in isolation without referring to Parts A1.1 and A1.2. Additionally, including these sections might benefit readers who are interested in the model comparison but not familiar with the PDI model, as it allows them to read the paper without needing to consult other publications. Therefore, our suggestion is to retain the complete appendix.

- 411: There is no Figure 7. Must be Figure A4.
  Thanks for that hint. You are right, it must be Fig. A.4 and has been corrected accordingly.

Cited literature

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