Response to reviews of the manuscript titled "Hydro-pedotransfer functions: A roadmap for future development" - egusphere-2023-1860 - by Weber et al.

Replies to Reviewer Comments 1 (RC1)

Thank you very much for your time and effort to dedicate yourself to this extensive manuscript. Your work is very much appreciated.

Main Comments[1]

We summarize the concerns as

1. This long manuscript needs a clearer structure to be more accessible to the reader by
    a. summary outlining the structure
    b. introducing a glossary

AGREED.
ad a: We will add a summary at the end of the introduction, providing an overview of the sections to come.
ad b: We will introduce a *glossary* and at the beginning of the manuscript. Doing this will technically improve the manuscripts' readability. We also make more consistent use of abbreviations.

2. Manuscript presentation: trim the text down and, to the best of their ability, strive to focus on the main takeaways. For example, Fig. 9 seems to portray many (if not most) of the points the authors are making. Fig. 9 could feature much earlier in the manuscript and be used as a vehicle for presenting or better structuring the discussion.

AGREE partly. With reference to R2, who states the paper is "*well-organized and well-written*", we will take your valuable point and carefully go through the manuscript, streamline where feasible and ensure to make it more concise

With more clarity about the contents and an enhanced coherence with the glossary, we will seek to emphasize the takeaways of each section. Because section 6 is at the most abstract level, Fig. 9 alone would lose much of the main points of the former sections if taken as the central vehicle.

3. As *minor* comment: Explicitly mention composite soils, and how microbial activity and plants modulate soil hydraulic properties and how this relates to hyPTF.

AGREE. Although this seems to slightly contradict point 2, we now write "*Another factor that has been neglected so far is the temporal evolution of SHPs. Swelling and shrinking processes may change soil saturated and near saturated hydraulic conductivity radically*

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within a few hours (Stewart et al., 2016). Burrowing of soil macrofauna like earthworms can increase hydraulic conductivity by orders of magnitudes in a matter of weeks (Bottinelli et al., 2017). Several studies have meanwhile provided evidence of seasonal dynamics, which may be strongly modified on a temporal scale of days to months to years (Messing and Jarvis, 1993; Horn et al., 1994; Bodner et al., 2013; Sandin et al., 2017). Droughts have also been found to alter SHPs significantly (Robinson et al., 2016; Gimbel et al., 2016)."

This stresses the point of seasonality and temporal evolutions of SHP more clearly.

Minor Comments

Extracted from: https://doi.org/10.5194/egusphere-2023-1860-RC1

In each comment, sentences from the paper are transferred here and the highlighted words in PDF are shown in red color. Then, the reviewer's comments are pointed to as Reviewer comments.

1. Line 5: Hydro-pedotransfer functions (PTFs) :

   Reviewer comment : just wondering if not better to call them HPTFs?

There is no call for action, thus we do not change anything.

Detailed rationale: Using the acronym PTF might align better with the terminology employed in the literature thus far. For instance, PTF is utilized for predictions which are based on easily available soil properties, STF is employed when spectral information is the input, and co-GTFs are prediction methods that include various environmental covariates as input (encompassing not only soil but also topography, vegetation, meteorology, etc.). In this context, the acronym reflects the nature of the input used for the prediction.

2. Line 17: Most system settings are not captured by existing PTFs, which have been built mostly for agricultural soils in temperate climates:

   Reviewer comment: settings is a bit unclear, could you please provide an example here? or rephrase?

AGREE. We delete the words *settings* and made changes to the text. This is then sufficiently clear. (changes in italics)

"Most soil *systems are not represented* in PTFs, which have been built mostly for agricultural soils in temperate climates"

3. Line 17: Thus.

   Reviewer comment: replace dot with comma

AGREE. Done

4. Line 18: existing PTFs largely ignorie how parent material, vegetation, land use, and climate affect processes that shape soil hydraulic properties:

Reviewer comment: revise ignorie

Agree. Corrected

5. Line 23: These aspects are addressed here in a joint effort by the members of the International Soil Modelling Consortium (ISMC) Pedotransfer Functions Working Group with the aim to systematise PTF research and provide a roadmap guiding both PTF development and use.

Reviewer comments: the aim, and how the study strives to achieve it, is a bit unclear. Given the relevance of this study, i suggest the authors provide a more concise and clearer aim statement followed by the application/impact of the study. It is also unclear whether the study focuses on reviewing existing evidence or if the latter was verified with new research.

AGREE. Also in reference to Major comment #1 of RC1, we will structure the manuscript more clearly, by giving a overview of what is tom come at the end of the introduction.

Though I still need to read the following sections, i missed a reference to soil composites . One can assume that PTFs were developed for soil materials alone but did not consider reinforced soil, vegetated soil, or soils with high microbial biomass, thus presenting totally different properties.

AGREE. Also in reference to Major comment #3 of RC1, we include a list of further porous media/soils and forces for temporal variations which are unaddressed in section 3.3.

6. Line 26 (Introduction section):

Reviewer comment: a list/table containing abbreviations and their meaning would be useful if provided after the abstract

AGREE. We add a glossary.

7. Line 42: (capacity models)

Reviewer comment: replace with i.e.

AGREE. Will do.

8. Line 42: Water flow in soils is also described by simple models based on basic mass balance calculations (capacity models) (Gilding, 1992)

Reviewer comment: full stop

AGREE. Will do.

9. Line 50: PTFs refer to a linear or non-linear regression relationships between explanatory and predictor variables that allow the estimation of SHPs from data available in soil maps or easy-to-measure soil properties.

Reviewer comment: full stop

AGREE. Will do.

10. Line 50: Thus, provided the spatiotemporal states of soils are known (Gerke et al., 2022).

   Reviewer comments: but this is something very hard to know accurately for some soil attributes, as the soil is a very heterogeneous compartment. I suggest the auhtors make a point in relation to this issue here.

AGREE. Will do

   Change full stop with comma.

AGREE. Will do

11. Line 56: Predictors generally include sand, silt, clay content, soil texture classes, bulk density (BD), and soil organic carbon (SOC), although some attempts have been made to include additional chemical and morphological properties and soil structure information (see Van Looy et al., 2017) or water retention properties such as water content at field capacity and at wilting point (Schaap et al., 2001).

   Reviewer comment: this clarifies my point above, but it is a very long sentence. Please, split it into two.

AGREE. Will do to read:.

"Predictors generally include sand, silt, clay content, soil texture classes, bulk density (BD), and soil organic carbon (SOC). Some attempts have been made to include additional chemical and morphological properties and soil structure information (see Van Looy et al., 2017) or water retention properties such as water content at field capacity and at wilting point (Schaap et al., 2001)."

12. Line 60: The majority of PTFs predict parameters of the Brooks-Corey or van Genuchten (Brooks and Corey, 1964; van Genuchten, 1980) and capillary conductivity functions (Mualem, 1976).

   Reviewer comment: perhaps a reference to the value of this equations could be made here? e.g., estimation of matric suction under unsaturated conditions (which is very hard to predict and measure) using soil moisture as predictor (which is relatively easy to measure and estimate).

AGREE. We will include a sentence about the value of PTFs in section 1, highlighting the motivation to use them at all.

13. Line 64: However, the scale of application typically ranges from field or pedon scale of several meters (Vogel, 2019) to regional or global scales where application is done on grids >> 1 km resolution by PTF users, typically modelers interested in the representation of different Earth System processes (e.g., Pinnington et al., 2021).

Reviewer comment: please revise this - i would suggest to use text instead of a symbol.

AGREE to use "much larger than" instead of ">>"

14. Line 85: research directions for the definition of a more robust and versatile next generation of PTFs.

Reviewer comment: A short paragraph outlining the paper's structure could follow the aims section, so it is easier to follow by the reader. Also, it is unclear how these aims are going to be achieved. a brief reference to the methodology employed throughout would also help the reader follow and understand better the message.

AGREE to some extent. The manuscript will receive a brief outline summary at the end of the introduction. This manuscript is something between a review manuscript, a perspectives paper and an opinion and as such does not require a methods section.

15. Line 86: Soil hydraulic property models and egregious shortcomings

Reviewer comment: i love the word choice :D

Thanks!

16. Line 109: Based on regression with data from 45 soils, Mualem found tau=0.5 as the best value.

Reviewer comment: revise

AGREE. We now write: "*Based on regression with data from 45 soils, Mualem (1976) found that a value of 0.5 for the so-called tortuosity parameter (often tau or lambda) is a suitable choice value and has been used in the predominant cases.*"

17. Line 110: This model has become so widely used because

Reviewer comment: which model?

AGREE. Now reads. changes in italics. "*The VGM* model has become [...]"

18. Line 120: In spite of its wide adoption, the use of the VGM model to represent SHPs is challenged

Reviewer comment: this is great stuff

AGREE. Thanks!

19. Line 130: Also, authors have excluded the conductivity data > -6 cm pressure head, and estimated the VGM parameters, but then used the matching point conductivity (K_0 [LT-1]; (Weynants et al., 2009; Zhang and Schaap, 2017a; Zhang and Schaap, 2017b) to describe datasets of WRC and HCC.

Reviewer comment: this is hard to follow, please, rephrase

AGREE. We now rewrite this sentence as:

"A number of approaches exist, in which all conductivity measured at pressure heads larger than -6cm were excluded. The motivation is that the remaining data is related to the soil matrix, only, discarding the data describing the conductivity of the macropores. The subsequent model fitting still requires a conductivity parameter, which is termed the matching point conductivity ($K0$ [LT-1]; (Weynants et al. 2009; Zhang and Schaap, 2017a; Zhang and Schaap, 2017b). This matching point conductivity is the saturated hydraulic conductivity of the soil matrix."

20. Line 133: This also indicates the presence of bimodality, something which has been corroborated by a systematic analyses of some data bases by Zhang et al. (2022).

    Reviewer comment: revise

AGREE. Done.

21. Line 152: This PTF translates any set of VGM parameters to the BW parameters and it was shown that it was possible to outperform the VGM model, even if the model was not directly fitted to training data.

    Reviewer comment: this is quite interesting

AGREE.

22. Line 157: Capillary hysteresis results from pore scale processes, mainly due to the irregular shapes of pores (ink bottle effect, (Haines, 1930)), the hysteresis of contact angles

    Reviewer comment: between which elements these contact angles form? water film and soil particles? please specify

AGREE. We now write:

Capillary hysteresis results from pore scale processes, mainly due to the irregular shapes of pores (ink bottle effect, (Haines, 1930)), *inducing hysteresis of contact angles between the soil water and the soil particle.*

23. Line 161: as hysteresis has been shown to impact the simulation of water fluxes and storage in the soil.

    Reviewer comment: is that hysteresis impacts the simulation, or that it actually impacts on soil hydrological processes? and so the simulation without hysteresis can not realistically predict water retention and flux. please, clarify

AGREE. deleted "*the simulation of*"

24. Line 170: The reason is that it requires extensive laboratory measurements to determine the boundary curves (drying and wetting curves; the solid red and blue lines in Fig. 1

    Reviewer comment: i guess this could be delted. as it clear from Fig.1 which are the wetting and drying curves, and Fig.1 has a caption for reader interpretation

AGREE.

25. Line 172: model parameterization is mainly based on the use of "effective properties", whereby effective WRC and HCC models are calibrated to match observed average state variables

    Reviewer comment: please, provide examples

DISAGREE. The very fact of averaging over a soil sample makes the properties effective. This does not require examples, but is the mere nature.

26. Line 181: Dynamic non-equilibrium and transient SHPs

    Reviewer comment: please, spell out here

AGREE

27. Line 184: In other words, under transient conditions, the water phase is not instantaneously in equilibrium with the pressure head, so that the water content may lag behind

    Reviewer comment: please, clarify behind

AGREE. We rewrite:

"*In other words, under transient conditions, the water phase is not instantaneously equilibrated with the pressure head and water content in soil which is continuously drained (wetting), attaining the equilibrium curve described by the WRC.*"

28. Line 204: An example for this time-dependence is considering information about soil tillage operations and post-tillage pedogenic processes leading to transient SHPs.

    Reviewer comment: what about natural soils subjected to other changes? e.g., forest soil subjected to biomass decay, changes in soil microbial communities, etc. I think the the application of SHPS is beyond agricultural soil, and this should be acknowledged here somewhow.

    In any case, i find the idea of time-dependent PTFs very interesting

AGREE. We include here, as suggested in the major comments, other, naturally induced changes to the SHP, like microbial activity, and root growth, cryo- and bioturbation.

29. Line 206: This section is intended to assist the reader in the choice of PTFs for modelling applications while presenting the numerous limitations surrounding PTFs.

Reviewer comment: this opening text for each section could be moved, or outlined, in the introduction, so it is easier to follow

AGREE. Outline included at end of introduction and remove it in all instances at the beginning of a section.

30. Line 221: 3.1.1 Spatial Appropriateness

Reviewer comment: i find this section very interesting, but it also feels very long. I suggest the authors condense it and focus on the key message(s) or main takeaways from the review

AGREE somewhat. While the main point cannot be made rapidly, since it is a multi-faceted .

31. Line 228: "In a review on PTFs, Pachepsky and Rawls (1999) and Pachepsky and Rawls (2004) recommended the use of PTFs for regions or soil types similar to those in which they were developed".

Reviewer comment: why this in italics?

AGREE. Well spotted. We now add that this is a direct citation from Barros et al. (2013) by adding "*Barros et al. (2013) state in their review on PTFs. [...]*"

32. Line 248: They found that it is similarities in the correlation of the data, rather than climate

Reviewer comment: please, revise

AGREE. Done.

33. Line 250: More studies are required to substantiate and verify this transfer learning which is used in soil mapping (Malone et al., 2016) or lean on meta-models

Reviewer comment: this is a bit unclear, please, rephrase

AGREE. Language will be improved

34. Line 255: An alternative approach to tackle this lack of site-specific data is to develop PTFs that explicitly incorporate soil taxonomic classes and/or diagnostic horizons

Reviewer comment: I understand the authors are referring here to FAO/USDA classifications, but what about other more quantitative classifications such as that used in soil mechanics? i.e., using a combination of soil attributes such as particle size distribution, porosity, bulk density, % organic matter, ettc

DISAGREE. This must be a misunderstanding, since the idea is to precisely include more than the standard repertoire. This is exactly what the manuscript is about.

35. Line 273: While this is never included, a meta-PTF has been developed

   Reviewer comment: please, clarify

<mark>AGREE…XYZ</mark>

36. Line 276: For simplicity and due to a lack in knowledge, these LSMs apply the same soil hydraulic parameterization as used for the rest of the terrestrial surface, even though sediments and unsaturated rocks may show substantial differences in SHPs compared to the soils located close to the surface.

   Reviewer comment: please, clarify, some rocks are impervious

AGREE somewhat. Yes, some rocks are impervious, but we are discussing soils, here, which are porous as a result of pedogenesis. No changes to the text required.

37. Line 291: As stated, parent material, climatology, and land use are important drivers that determine SHPs.

   Reviewer comment: this paragraph is redundant

AGREE. Deleted.

38. Line 319: SHPs are controlled considerably by plant root processes shaping soil structure.

   Reviewer comment: this and the impact of soil microbial communities (related to the vegetation) should be akcnowledged in the abstract and introduction

AGREE somewhat. It is not important enough to be mentioned in the abstract, but, as stated in a reply to an earlier comment, here, microbial acitvitiy as a soil morphological forming process will be included.

39. Line 332: Most Earth System models also do not explicitly represent the litter layer (the so-called 'O horizon')

   Reviewer comment: organic matter may be very hydrophobic, so i guess here is where the difficulty of deriving PTFs might lie.? that O horizons simply do not like water.? could the auhtors comment on this?

DISAGREE. We do not want to speculate about the reasons, here.

40. Line 338: A common solution to account for litter layers is to parameterize them as a 'pseudo-litter' layer by reducing the BD and estimating the SHP from given PTFs

   Reviewer comment: please spell out, this does not appear as often as SHP and PTFs. The same for other acronyms which appear for the first time in a section

DISAGREE. With reference to the now available glossary, and since BD has been introduced at this stage, we kindly disagree.

41. Line 368: In this context, the accuracy of ML-based maps of soil properties such as those provided by SoilGrids

    Reviewer comment: ML?

AGREE. ML now spelt out as machine learning.

42. Line 378: These effects cannot be modelled with the current approaches that assume a rigid porous medium.

    Reviewer comment: very good

AGREE :)

43. Line 405: For soil textural information this kind of approach is generally unsuitable.

    Reviewer comment: very good

AGREE

44. Line 426: Then, they fitted a soil hydraulic property model to all synthetic data points; this can be considered a suitable averaging procedure and has also been used by Weber et al. (2017a).

    Reviewer comment: this claim is arguable - as the SHP are as heterogeneous as the soil classes - though the spatial interpolation process is more complex, it may not be better, as it may simply add noise and uncertainty to the outputs. Could the authors comment on this?

DISAGREE. In fact this is not a correct assessment,

45. Line 505: From EU-HYDI, we selected those records that included information on soil texture, BD, and organic matter.

    Reviewer comment: did these records included SHPs, too?

AGREE. We write (changes in italics) "From EU-HYDI, we selected those *soil hydraulic property measurements* that included information on soil texture, BD, and organic matter."

46. Line 508: Figure 6 and Figure 7 show the results for water retention at a suction of -100 cm, and K_{sat} , respectively.

    Reviewer comment: a comment on the differences between sand and clay regressions is missing here

This comment does not fit here. We checked, and could not reconstruct what is meant, while the text of the manuscript remains very clear.

47. Line 515: 4.2 Harmonization and standardization of methods

Reviewer comment: this section is very interesting and needs to feature in this manuscript, as it is essential that more validation is carried out following standard approaches. However, i am finding it too long and sometimes it is hard to make an instant connection with PTFs. I suggest the authors trim this Section down and stick to the key message(s). Overall, this section contains detail that sometimes does not fully add to the study or key message and which in any case can be written is a more direct and brief style.

AGREE. Section will be streamlined in language and structure, but kept.

48. Line 526: In Figure 8, we present a comparison of BD on a dry mass basis determined on soil clods that were equilibrated at -33 kPa water content and oven dried with the volumes determined separately.

Reviewer comment: above, matric suction was expressed as hydraulic head and here as pressure, which makes it hard to follow. Please, use the same units throuhgout the manuscript.

AGREE. Changed as required.

49. Line 566: Furthermore, the X-ray tomography is also sometimes used to infer water retention curves, it is unlikely that these data are directly comparable with, for example, data from pressure plate experiments.

Reviewer comment: please, revise. the meaning of the sentence is not clear

AGREE. (changes in italics) "Furthermore, the X-ray tomography is also sometimes used to infer water retention curves. *However, to date far, it appears unlikely that these data are directly* comparable with, for example, data from pressure plate experiments".

50. Line 580: (see section Fehler! Verweisquelle konnte nicht gefunden werden.)

Reviewer comment: please, delete

AGREE. Silly old MS Word.

51. Line 619: Harmonization and standardization significantly increase the possibilities for data (re-) use.

Reviewer comment: this seems redundant with the previous sections - the point has been made sufficiently clear above

AGREE. Deleted

52. Line 651: Careful consideration of the use of hydraulic conductivity in models is warranted though, as it is impacted by the scale of observation (Roth 2008), and possibly by the atmospheric conditions (Oosterwoud et al., 2017), or by easonal effects

Reviewer comment: please, revise

53. Line 690: Deriving a PTF for bimodal models requires robust measurements of near saturation unsaturated hydraulic conductivity.

    Reviewer comment: please, revise. it is not clear how this is connected to soil structure characterisation. the same goes for the subsequent points. it may be worth starting the section with a brief sentence introducing how soil structure emerges (given that it is an emerging property of the soil ecosystem)

54. Line 752: While advancement to the quantification of soil structure is expected to enhance our ability to better characterize the wet end of the water retention curve and especially saturated and near-saturated conductivity, other opportunities have emerged that may help infer the dry range of soil water retention – whether in one step or two steps.

    Reviewer comment: i suggest the authors make a clearer connection to PTFs. one thing is understanding and characterising better water retention and hysteresis, another modelling them with simple analytical models

55. Line 764: For example, the near infrared sensor can be mounted in a penetrometer to measure soil spectra with depth. Some infrared hyperspectral sensors can be attached to satellite, aircraft or unmanned aerial vehicles, offering detailed soil surface spectra reflectance (e.g., (Lagacherie et al., 2020).

    Reviewer comment: what about other wavelengths that can penetrate in the soil?

56. Line 766: by relating the spectra to the measured soil properties by (multivariate) regression functions.

    Reviewer comment: this seems redundant - it is already mentioned above?

57. Line 783: They can open new directions in inferring soil (hydraulic) properties at the volume of soil surveys.

Reviewer comment: this is unclear, please revise

AGREE. Deleted

58. Line 801: Bayesian inference can identify the maximum a posteriori estimate of the model parameters.

    Reviewer comment: this is unclear, please explain

AGREE The used term was incomplete, we corrected the text and refer now to the well-established statistical quantity "maximum a posteriori *probability* estimate", often abbreviated as MAP, which can be readily checked in a statistics textbook.

59. Line 805: According to Bayes' theorem,

    Reviewer comment: this can be checked in a statistics textbook (as indicated by the authors below), so i would suggest the authors are more focused and direct in this section and throughout, as the MS is very long already.

DISAGREE. We explicitly incorporate this technical, but thought provoking part, here, since we think this is an essential novelty in deriving PTFs.

60. Line 826: This model-based evaluation of the prior predictive may provide a method to bridge the gap between the laboratory-based measurements commonly used in PTF building and field scale functional evaluation (section 6).

    Reviewer comment: for example, this is the key takeaway of this Section, but given the amount of text and detail, the message is not sufficiently clear. in reference to my comment above, the paper's flow and message could improve by focusing on the main points the authors want to make, reducing the extent of the context set to explain or take the reader to the key point.

DISAGREE. The intention here is clearly link the previous sections with a methodological approach, which also relates to a method of how to honor physical constraints (5.2.) and the functional evaluation (section 6)

61. Line 852: Class PTFs are typically not impacted by unphysical parameters estimated as the selected WRC and HCC models are directly fitted to all measured θ-h and (if available) K-h data for each combination of texture class (Wösten et al., 1999; Schaap et al., 2001; Tóth et al., 2015).

    Reviewer comments: what is class PTFs? what do the auhtors mean by unphysical parameters?

AGREE. This section hinders the flow and can be safely deleted.

62. Line 901: Rectifying such biases in current PTF estimates of SHPs requires a paradigm shift to build PTFs which are not purely the result of minimizing a cost-function but should be further anchored to a physically based framework (cf. section 5.1. for the methodological framework).

Reviewer comments: physically-based. section 5.1 illustrated a methodological framework based on Bayesian statistics but, as i understand it, this is not a physically-based framework. could the authors explain and revise?

AGREE we now correct these lines by: Section 5.1 introduced a framework for parameter estimation of physically-based models based on Bayesian statistics. We corrected the text to clarify as follows:
"*Rectifying such biases in current PTF estimates of SHPs requires a paradigm shift to build PTFs that are not purely the result of minimizing a cost-function but should be anchored in a modeling framework to obtain physically consistent PTFs using Bayesian inference (cf. section 5.1. for the methodological framework).*"

63. Line 906: Box 1. Constraints for the determination of soil hydraulic properties

Reviewer comment: could this box in feature in supplementary material? the constraints are introduced and discussed in Section 5.2, and this box only expands on it but it is not essential for the adequate understanding of the paper's message

AGREE somewhat. We think this is a typesetting question and will address it then.

64. Box 1: To provide reasonable results at larger scales, the determination of the parameter values must honour various constraints as discussed in this box.

Reviewer comments:where are the constrains shown? a list here would make it easier to follow.

I understand that the constraints discussed in the box are water content, n, Lc, and Tp? is so, water content and n need to be formated in bold

DISAGREE. The box gives the constraints in equation B.1-B.5. Also, we added constraints in the Glossary. The title of the Box states these are the constraints

65. Box 1: Methods how to include the constraints were discussed in subsection 5.1.

Reviewer comment: please, revise

DISAGREE. We changed the sentence to (changes in italics): "Methods *on how* to include the constraints *during the PTF building process* were discussed in subsection 5.1."

66. Line 921: However, when evaluating a result of regression or machine learning with general mean statistics, the performance of the resulting PTF remains opaque since the distribution and auto-correlation of residuals, non-unique variable combinations, or non-linear characteristics are not assessed.

Reviewer comment: it may be worth indicating that machine learning outputs can be cross-validated following similar, yet simple, approaches to linear regression.

DISAGREE. Cross-validation does not solve the problem stated, here.

67. Line 937: Building on the scale triplet (Blöschl and Sivapalan, 1995), potential reference data and PTF applications can be positioned along a scale axis (Fig. 9, x-axis).

   Reviewer comments: please, define triplet.

AGREE. We now write (changes in italics) "Building on the scale triplet (*spacing, extend, and support*; Blöschl and Sivapalan, 1995)"

   fig. 9 is very good and seems to portray the key message of this paper (thus being one of the main deliverables from this study) - i would suggest that the authors use this figure as a vehicle to illustrate current practice and knowledge and future direction in the context of PTFs. The figure could appear much earlier in the manuscript, and this could potentially help merge different subsection in which the message is similar - for example, problems with PTF upscaling are discussed in different parts of the MS and perhaps could just be assessed in one section only, thus helping with reducing the length of the text.

DISAGREE. Moving this figure as suggested would mean we have to completely reorganize the manuscript, which will be an unfathomable endeavor.

68. Line 1029: As a manifesto, we advocate ten points:

   Reviewer comment: a summary of this could be provided in the abstract?

AGREE. We now add "*We close with a ten-point catalogue for funders and researchers to guide review processes and research.*" at the end of the abstract to point at the very important part.

69. Figure 1:

   Reviewer comment: perhaps something on the scanning curves could be mentioned here?

DISAGREE. Revisiting the figure we think this is fine, in particular with reference to the legend, which is complete.

70. Figure 2:

   Reviewer comment: it may be worth mentioning in the main body text that this was tested as part of the study?

AGREE. We added a sentence to the end of section 2.4 "Figure 2 shows the retention curves from the laboratory with fully saturated samples and the field retention curve, analysed in this study. "

71. Figure 3: Total porosity and water content at -33 kPa for A-horizons (a, b), B-horizons (b, d) of selected soil orders, and diagnostic horizons (e, f) as defined by US Soil Taxonomy. Data are from the Pedogenic and Environmental Data Set (PEDS).

Reviewer comments:this figure is very interesting, and great to improve modelling. c? citation?

AGREE. *a, b* was changed to *a, c.*

72. Figure 6: PTF fitting of the water retention data obtained from the EU-HYDI database at soil suction of -100 cm. (a) Comparison between measured soil moisture and PTF derived soil moisture by multiple linear regression (adjusted R2: 0.64), colour is related to percentage of sand in sample, data point size is related to organic matter content, (b) same as (a) colour related to method number, data point size is related to organic matter content, (c) residuals plotted per method. Method 604: unknown; Sand/kaolin box method with undisturbed soil core, method 610: 100 cm3, 613: 222 cm3; Pressure plate method with undisturbed soil core, method 620: 100 cm3, 621: 200 cm3, 622: 250 cm3; 642: Pressure membrane method on undisturbed soil clods method 642 3-5 cm3 with estimation of soil volume on undisturbed soil core (500 cm3), 643: 3-5 cm3; Hanging water column method with undisturbed soil core, method 650: 250 cm3; Evaporation method on undisturbed soil core, method 672:630 cm3, with tensiometers at four depths (1, 3, 5 and 7 cm).

Reviewer comments: a legend for the organic matter would be useful for better interpretation.

AGREE. Added the following text to the end of the caption. *Further details on methods and data are to be found in EU-HYDI; Weynants et al., 2013)*

some of the methods are hard to follow - i would suggest to revise the text provided or present it in a different manner.

DISAGREE. It is all there. The reference to the EU-HYDI document is now included.

the residuals violin plots is not commented in the legend, some brief text would help with the interpretation

DISAGREE. It is there.

73. Figure 7:

Reviewer comment: same as for Fig. 6

AGREE. Added the following text to the end of the caption. *Further details on methods and data are to be found in EU-HYDI; Weynants et al., 2013)*

74. Figure 9:

Reviewer comment: this figure is very good

THANKS