**Hydrology and
Earth System
Sciences**

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Discussions

Interactive comment on "Macropore flow at the field scale: predictive performance of empirical models and X-ray CT analyzed macropore characteristics" *by* M. Naveed et al.

M. Naveed et al.

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Comment 1: The manuscript presents a study in which X-ray CT-based image information and existing prediction functions are used to deduce/predict macropore flow "at the field scale", based on topsoil samples from a 15x15m area. I find the study interesting and justified, and reasonably well presented, although I don't see much novelty in checking some exotic PTFs that are not designed to estimate macropore flow, especially not to the foreign locality. I have relatively minor and technical suggestions towards finalizing the manuscript, other than I would really encourage the authors to make the relevant data available to others if possible through some data repository

(which would hopefully find followers).

Reply: We acknowledge the reviewer's notion that macropore flow was previously related to basic soil properties. Though this is true for fluid permeabilities (saturated hydraulic conductivity and air permeability), there is not a lot of published work related to gas diffusivity. While it was previously documented that gas diffusivity is a concentration-driven gas transport parameter that can be predicted from basic soil properties (e.g. Moldrup et al., 1998 & 2000, Deepagoda et al., 2011 & 2014), we demonstrated in the current manuscript that this does not hold for -30 cm matric potential. Only for matric potentials of -100 cm and lower empirical models for prediction of gas diffusivity from soil properties performed reasonably well. The second part of the manuscript (Figs. 6 and 7) is novel. Although a few recent studies (e.g. Katuwal et al., 2015; Larsbo et al., 2014; Naveed et al., 2013; Luo et al., 2010) reported quantitative relationships between macropore flow and X-ray CT analyzed macropore network characteristics, this is to the best of our knowledge the first study that distinguishes biopore- and matrix-flow. This was also pointed out by J. K. Koestel in his short comment. We reported that different relationships exist between macropore flow and macropore network characteristics for biopore-flow and matrix-flow dominated columns for permeabilities (air and water) as well as for diffusivity at -30 cm matric potential, but not for diffusivity at -100 cm matric potential. We propose to develop and add multiple linear regression models to reveal significant macropore network characteristics for predicting macropore flow for biopore- as well as matrix-flow dominated cases. In the revised manuscript, we will attempt to clearly point out the novelty and implications of findings. All minor and technical suggestions will be addressed and corrected in the revised manuscript. We will provide a web link to a public data repository in the revised manuscript.

Comment 2: I wonder if sample storage at -2C (P120955 L21) introduces freeze-thaw effects? Was the actual moisture content controlled – which could introduce differences in the behaviour of samples when frozen and thawed?

Reply: This was erroneously reported. The actual temperature was 2C. We will correct this in the revised manuscript.

Comment 3: I suggest introducing – early in the manuscript – the corresponding pore diameters that are expected to drain at the examined pressures, and relate that to the resolution of the images.

Reply: Done. This will be introduced earlier in the revised manuscript in Materials and Methods section.

Comment 4: It would be great to introduce each of the CT-derived metrics, or refer to a source if one exists for all of the used metrics.

Reply: Done. A new figure will be provided in the revised manuscript including all CT-derived macropore network characteristics with detailed schematic explanation for each.

Comment 5: On fitting power functions to the data in Fig 7: Were power functions better than simple linear regression? After describing that they were fitted on an either-or basis, there was no discussion of how they performed relative to each other, but only power functions were mentioned. If there is a physical basis why power relationship can be expected, explain it briefly.

Reply: Done. The model that best described the data was fitted in figure 7. This will be better explained in the Materials and Methods section.

Comment 6: Define how the samples with biopore flow were separated from those with only matrix flow.

Reply: 3-D pore visualization was carried out for each soil column. Based on the visual judgement, the samples with apparent biopores connected from top to bottom of soil columns (referred as biopore-flow dominated columns) were differentiated from matrix-flow dominated columns. It will be better explained now in the revised manuscript.

Comment 7: P12101 L13-19: It is understood that those PTFs were developed based on small core samples (mainly from horizons), so the scale difference in not really real. (To this end, I wonder if this is really a "field scale" study – hence the quotes in my intro sentence. Second, I think the situation in terms of over and under prediction is not that simple, given the huge range difference between predicted and measured data (Figure 3). First, find out and discuss why there is a large range of measured kw but a much smaller range of PTF predicted ones - I guess this comes from the limitations of the PTFs. To my understanding existing PTFs are not really expected to perform well to predict macropore flow. And third, as I deduct, the study evaluates its own prediction (fitting?) on exactly the same data (i.e. no independent evaluation), while the data set is an independent set to any of the PTFs involved. That is not exactly good methodology. Is the PTF part really needed?

Reply: 1. We agree with the reviewer that the tested PTFs were developed on the horizon scale and may not be able to perform well for the small soil columns as used in the present study. 2. Yes the main limitation for the pedotransfer functions is that they take into account soil textural properties but ignore soil structural properties, particularly biopores. 3. We are not proposing a model based on data fitting as shown in Fig. 7. Instead, we illustrate that two different macropore flow phenomena occur, one is biopore-flow dominated and other is matrix-flow dominated. Different correlations exist between macropore flow and macropore network characteristics for each i.e. biopore-flow dominated columns and matrix-flow dominated columns. So any future pedo-transfer function must take into account soil structural features as well as two distinct macropore flow processes (matrix and biopore). Further in the revised manuscript, we will develop multiple linear regression models to reveal significant macropore net-work characteristics for predicting macropore flow for each of the cases (i.e. biopore and matrix-flow dominated systems). Another potential future avenue for prediction of macropore flow is the application of fluid dynamics simulations (i.e. lattice Boltzmann model) with segmented X-ray CT pore networks.

Minor editorials:

Comment 8: P12091 L8: of its inherently

Reply: Done in the revised manuscript

Comment 9: L19: 'relatively failed' – I can't make sense of this. Did it fail or not? Needs to be stated based on objective criteria.

Reply: Done, This will be revised.

Comment 10: L19: potential, particularly (comma use)

Reply: Done in the revised manuscript

Comment 11: P12020 L1: I suggest replacing 'need of' with 'opportunity for'

Reply: Done in the revised manuscript

Comment 12: L2: for a digital

Reply: Done in the revised manuscript

Comment 13: P12093 L3: replace 'large presence' with 'abundance'

Reply: Done in the revised manuscript

Comment 14: L6: first by

Reply: Done in the revised manuscript

Comment 15: L21: along with the prediction

Reply: Done in the revised manuscript

Comment 16: L28: However, none of the studies have tested their application in the field scale before.

Reply: Done in the revised manuscript

Comment 17: P12094 L20-26: These are not specific objectives, but research questions. Introduce them differently, or reformulate the 3 points to present objectives.

Reply: Done. This will be reformulated in the revised manuscript.

Comment 18: P12095 L14: in the summer of 2012

Reply: Done in the revised manuscript

Comment 19: L16: move the word 'stepwise' to after 'cylinders'

Reply: Done in the revised manuscript

Comment 20: L18: move 'step by step' to the end of the sentence

Reply: Done in the revised manuscript

Comment 21: L20: from the field

Reply: Done in the revised manuscript

Comment 22: L27: using the method of Kulkarni et al

Reply: Done in the revised manuscript

Comment 23: P12097 L17: calculated as the ratio

Reply: Done in the revised manuscript

Comment 24: L20: and was defined as

Reply: Done in the revised manuscript

Comment 25: P12098 L13: for laminar flow

Reply: Done in the revised manuscript

Comment 26: P12099 L21, 'mainly': Preferably state all the texture classes

Reply: Done in the revised manuscript

Comment 27: P12100 L13: north side of the field

Reply: Done in the revised manuscript

Comment 28: L24, 'marked samples': marked for what? It should be here, or even earlier that some of the samples are highlighted – why those, etc.

Reply: This was just to show 3-D pore visualization of four samples, out of which 2 are biopore-flow dominated and 2 are matrix-flow dominated. This will be clarified in the revised manuscript.

Comment 29: P12101 L8: At least some of the referred studies predict Ksat, not Kw

Reply: Done. This will be corrected in the revised manuscript.

Comment 30: L14-15: over-predicted under-predicted

Reply: Done in the revised manuscript

Comment 31: L24: comparatively fails? Does it fail or not?

Reply: Done. It will be clarified in the revised manuscript.

Comment 32: P12102 L9: methods, whether global or locally adaptive, resulted (comma use)

Reply: Done in the revised manuscript

Comment 33: L27: between the two measures

Reply: Done in the revised manuscript

Comment 34: P12103 L1: if the image

Reply: Done in the revised manuscript

Comment 35: L2: i.e. there is a lot of noise

Reply: Done in the revised manuscript

Comment 36: P12104 L4-5: 'two-branch system data trend' and 'single' needs to be introduced. I know what is meant, but this is vague. Also cite the unfilled symbols.

Reply: Done. This will be better clarified in the revised manuscript. Unfilled symbols will be cited in the revised manuscript.

Comment 37: L10, 18 and elsewhere later: explain 'moderate and significant power regressions', modify terminology as necessary.

Reply: Done in the revised manuscript

Comment 38: L28: the performance of the regression function significantly improved. . . . . .(Btw, use significantly if tested, else use the term substantially. Significantly is a reserved term.)

Reply: Done in the revised manuscript

Comment 39: P12106 L3: despite this

Reply: Done in the revised manuscript

Comment 40: L7: for the prediction of

Reply: Done in the revised manuscript

Comment 41: L8-9: particularly for the samples that contained top-to-bottom connected biopores.

Reply: Done in the revised manuscript

Comment 42: P12107 L5: of a digital

Reply: Done in the revised manuscript

Comment 43: Figure 2: Better relate to Figure 1, and especially to the text on P12100. At the moment they are introduced quite late in the ms.

Reply: OK. This will be better described in the revised manuscript.

Comment 44: Figure 6: Define what is weak, moderate, etc. and how decided.

Reply: Done. This is defined based on correlation coefficient. It will be clarified in the revised manuscript.

Comment 45: Caption of Figure 7: if found significant.

Reply: Done in the revised manuscript