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Hydrology and Earth System Sciences
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: Although it does not seem to have much novelty in results or methods of analysis, the paper is a solid piece of experimental work that the professional readership should be exposed too, therefore I recommend acceptance following significant changes suggested below.

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.

Comment 2: Title 1. "Macropore flow at the field scale:" lets the reader expect that observations or models of flow are at the field scale whereas all observations in the paper are made on cm-scale cores. The field scale has also nothing to do with the second part of the title on the predictive performance. Further more, the high values observed for the hydraulic properties of the cores (and the large variability) are due to samples with connected pores with a linear length of 3.5 cm. It is most probable that at the field scale the biopores will not dominant the large-scale flow and matrix properties will be more relevant. Therefore the term" field scale" should not appear in the title.

Reply: We agree with the reviewer and remove "field scale" from the manuscript title.

Comment 3: Title 2. The "empirical models" are the less exciting part of the work and if

included in the title they should be seconds to the “CT analyzed” which proved better.

Reply: We will modify the title according to the reviewer’s remark.

Comment 4: As a hydrologist the term “saturated water permeability” is a little annoying, because at saturation (i.e. single phase flow) the permeability is a characteristic of the porous medium regardless of the fluid. Perhaps the results – higher air permeability in an incompletely dry soil than water permeability in a supposedly saturated soil (means in table 1) drove the authors to use this terminology. Nevertheless, if not changed, the use of this term should be explained.

Reply: The term “saturated water permeability” will be better explained in the Materials and Methods section of the revised manuscript.

Comment 5: The use of the term “diffusivity” for the ratio between the diffusion coefficient in the porous medium and the diffusion coefficient in free air (if I understood correct), is also not the best choice I think. As far as I know the term diffusivity is given to parameters with the dimensions [L2/T] that fit the diffusion coefficient in the diffusion equation (e.g. in groundwater hydrology Transmissivity/Storativity (T/S) or K/Ss).

Reply: In soil physics literature, gas diffusivity is referred to as the ratio of gas diffusion in soil to the gas diffusion in free air. Gas diffusion has dimension L2/T and thus gas diffusivity is a unit less quantity. For example, please see Moldrup et al., 1998 & 200, Deepagoda et al., 2011 & 2014 and the references therein.

Comment 6: Use Ka (-30) rather than Ka -30, Dp/D0 (-100) rather than, Dp/D0 -100 etc.

Reply: Done in the revised manuscript

Comment 7: P 12094 L 8-9 delete “arrival time” it’s included in breakthrough.

Reply: Done in the revised manuscript

Comment 8: P 12096 L1 change “energy level” to electrical tension or electric potential

difference.

Reply: Done in the revised manuscript

Comment 9: P 12098 L 6 start a new paragraph before “After”

Reply: Done in the revised manuscript

Comment 10: P 12098 L 10 add of after "potentials”

Reply: Done in the revised manuscript

Comment 11: P 12098 L13 5hPa is pressure not a pressure gradient

Reply: Done, This sentence is revised in the manuscript.

Comment 12: P 12098 L 17 correct the dimensions of delta p to [M/LT2]

Reply: Done in the revised manuscript

Comment 13: P 12099 L 1-2 use capital K for hydraulic conductivity

Reply: Done in the revised manuscript

Comment 14: P12099 L 8 change “SD” to standard deviation (SD)

Reply: Done in the revised manuscript

Comment 15: P 12100 L 5, How can the median be dominated by extreme values? I would discard this sentence altogether.

Reply: Done in the revised manuscript

Comment 16: CV of 218% does not describe the variability as good as acknowledging the 5 orders of magnitudes spread of the saturated permeability.

Reply: Done in the revised manuscript

Comment 17: In addition to table 1, I recommend to add histograms of the hydraulic

properties (or at least of the saturated permeability) for the interested readers in the hydraulic data.

Reply: Done in the revised manuscript

Comment 18: Table 1, add a row of statistics of the saturated hydraulic conductivity in cm/hr easier for hydrologists and soil scientists 'to know where we are living'. Permeability in square microns is not intuitive to most of us.

Reply: Done in the revised manuscript

Comment 19: P 12101 L 1-2 delete the sentence "This is quite. . .. .

Reply: Done in the revised manuscript

Comment 20: 12101 L 20 change "decade" to 2 decades

Reply: Done in the revised manuscript

Comment 21: P 12103 L 26-28 It's the other way around: macropore flow is controlled by connectivity; matrix flow is controlled by pore-diameter distribution.

Reply: We understand that biopore flow is mainly controlled by the largest pore diameter whereas matrix flow is mainly controlled by the pore-diameter distribution and its connectivity based on our measurements and literature. We will revise this sentence to avoid confusion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12089, 2015.