**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.

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Comment 1: I am not one of the assigned reviewers. Therefore I will keep my feedback rather short. In contrast to the two referees having already given their opinion (until 19th December 2015), my views on this manuscript are more positive. I think that the manuscript does contain some new, interesting data, but suffers a lot from the lack of conciseness and modesty of this version of this text. A more humble approach is advisable because the basic ideas in this manuscript are indeed all but new (see for example Anderson, S.H. 2014 Tomography-measured macropore parameters to estimate hydraulic properties of porous media. Complex Adaptive Systems 36: 649-

654. And the references therein).

Reply: We agree and will revise the manuscript incorporating all suggestions.

Comment 2: In my opinion, this manuscript needs a better focus on what is new. What is already known needs to be pointed out in a better way.

Reply: We will thoroughly revise the manuscript to clearly discern previous from new knowledge contributed by the presented study.

Comment 3: Furthermore, the authors need to explain all morphologic measures they are using to quantify the macropore network features. It appears to me that for the majority of them an explanation is entirely missing.

Reply: We will add a new figure (figure 1 in the revised manuscript) that illustrates and explains all X-ray CT derived morphological measures in detail.

Comment 4: Things that are new to me: The distinction between biopore-flow and matrix-flow dominated columns when discussing the physical soil properties (albeit I must say that I have either missed the explanation of what the authors mean by this or it really is not at all explained in the material and methods. In any event it needs to be better explained. At the moment I am assuming I am guessing correctly). - Figure 7. Well it basically boils down to introducing the distinction between biopore and matrix-flow dominated columns. If the other reviewers do not agree that this is novel, I would be very much interested in learning about the respective publications.

Reply: Thanks for your remark that nicely captures the new knowledge contributed by our study. 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. We reported that different relationships exist between macropore flow and macropore network charac-

teristics 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 characteristic interrelations 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 5: What I would moreover find interesting: Why not add a map of macropore network properties to Figure 1? I am not aware of that this has ever been published.

Reply: We will add a new figure (figure 1 in the revised manuscript) that illustrates and explains all X-ray CT derived morphological measures in detail.

Comment 6: A quantitative comparison between spatial patterns of soil properties, air and hydraulic properties and macropore morphologies.

Reply: We have provided spatial patterns of soil texture and hydraulic properties in Figure 1. Another figure (possibly Figure 6 in the revised manuscript) will be prepared to show the spatial patterns of macropore morphological properties. Then a quantitative comparison will be carried out between spatial patterns of soil properties, air and hydraulic properties, and macropore morphologies using Moran's I.

Comment 7: Which is not new? 12094, L23; 12106, L5: The facts that there is still no well performing PTF for saturated hydraulic conductivity (Ks). See Weynants, M., H. Vereecken and M. Javaux. 2009. Revisiting Vereecken pedotransfer functions: Introducing a closed-form hydraulic model. Vadose Zone J. 8: 86-95.; Vereecken, H., M. Weynants, M. Javaux, Y. Pachepsky, M.G. Schaap and M.T.v. Genuchten. 2010. Using pedotransfer functions to estimate the van Genuchten–Mualem soil hydraulic properties: A review. Vadose Zone J. 9: 795-820.). I have recently been involved in investigating if things become better if one uses tension disk infiltrometer data but they do not. see Jorda, H., M. Bechtold, N. Jarvis and J. Koestel. 2015. Using boosted regression trees to explore key factors controlling saturated and near-saturated hydraulic

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conductivity. Eur. J. Soil Sci. 66: 744-756.

Reply: We acknowledge 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.

Comment 8: 12106, L2: That the spatial CV of saturated hydraulic conductivity at the field scale is very much larger than the respective one for the texture (starting from Nielsen, D.R., J.W. Biggar and K.T. Erh. 1973. Spatial variability of field-measured soil water properties. Hilgardia 42: 215-259. (if not earlier). By the way, it is not surprising that it is like this since the sat. hydraulic conductivity may vary over several orders of magnitude but the texture at most over two. You may want to logarithmize your hydraulic conductivities. Then also the CV would decrease.

Reply: Yes this is not the novel. We provided it in the manuscript as a base for the second and novel part of the manuscript. We will calculate CV for logarithmic permeabilities as these are not normally distributed.

Comment 9: What is wrong? 12094, L26; 12106, L18: Be careful with using the term "prediction". You are claiming to predict things but are not predicting anything. You simply are fitting a regression function to your data. Using the training data for validation may lead to massive over estimations of your predictive performance (Hastie, T., R. Tibshirani and J.H. Friedman. 2009. The elements of statistical learning: Data mining, inference, and prediction. 2nd edition ed. Springer-Verlag, New York.; see also Jorda, H., M. Bechtold, N. Jarvis and J. Koestel. 2015. Using boosted regression trees to ex-

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plore key factors controlling saturated and near-saturated hydraulic conductivity. Eur. J. Soil Sci. 66: 744-756.) For this reason, the comparison between e.g. ROSETTA's prediction and your regression is highly unfair.

Reply: We agree that currently we are not predicting, instead we are fitting regressions between macropore network characteristics and macropore flow parameters. We have carried out this to show the presence of different flow systems i.e. biopore flow and matrix flow. In the revised manuscript, we will carryout multiple linear regression analysis to find out the most significant macropore network characteristics for predicting macropore flow parameters. Multiple linear regression models will also be provided in the revised manuscript.

Comment 10: 12094, L21: You are claiming that you are correlating the "spatial variability" of water and air flow to the spatial variability of other soil properties at "the field scale". But you do not correlate spatial variabilities. You are comparing the respective values since you only have one variability for each property.

Reply: Yes this is true. We will correlate the spatial variability of different variables in the revised manuscript instead of just comparing their respective values. We will correlate covariance or Moran's I of different variables with each other in the revised manuscript.

Comment 11: What I would skip: The comparison of the effect of the different segmentation approaches. It has nothing to do with the main theme of the manuscript.

Reply: We agree and will remove the comparison of different segmentation methods from the revised manuscript and focus on the method developed by Kulkarni et al. (2012).