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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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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

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therein).

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. 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.

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.

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.

- A quantitative comparison between spatial patterns of soil properties, air and hydraulic properties and macropore morphologies

What is not new:

- 12094, L23; 12106, L5: The fact 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. Pachep-

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sky, M.G. Schaap and M.T.v. Genuchten. 2010. Using pedotransfer functions to esti-
mate 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 conductivity. Eur. J. Soil Sci. 66:
744-756.

- 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 prop-
erties. 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 mag-
nitude but the texture at most over two. You may want to logarithmize your hydraulic
conductivities. Then also the CV would decrease.

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 explore 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.

- 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.

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.

Well, it was difficult to keep this short. Anyway, I am looking forward to read a revised version of this manuscript,

Best regards from the North, Merry Christmas/Happy Holidays and a good start into the new year,

John Koestel

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