Referee-1

Comment 1:

Technically the work is solid. I am not sure about the novelty of the work. What is new? It was amply demonstrated that macropore flow cannot be predicted from soil basic properties. The authors are far from being first to demonstrate this. Macroporosity, macropore mean diameter, minimum connected macroporosity and macropore local connectivity are the properties derived from network analysis that correlate with conductivities and diffusivities. Such correlation is expected to exist as macropores are major conduits of water and gases in soils. What is new?

Reply:

We acknowledge the reviewers notion that macropore flow was previously related to basic soil properties. Though this is true for fluid permeabilities (saturated hydraulic conductivity and air permeability, Fig. 4 and Fig. 5A and 5B), 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 (Fig. 5C and 5D).

The second part of the manuscript (Figs. 6, 7, 8 and Table 2) 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 (Figs. 7, 8 and Table 2). 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 now developed best subset regression models (Table 2) for macropore flow for each category of soil columns i.e. all soil column, matrix-dominated flow columns, and biopore-dominated flow columns. **Page 17 Lines 7-25 and Page 18 lines 1-2.**

Novelty of the study is now clearly pointed out in the introduction, **Page 5 Lines 14-26**.

Comment 2:

The last sentence of the abstract is "This could pave the way for the digital soil physics laboratory in the future." Nothing is said in the manuscript on this matter. Is this the novelty? If yes then the authors should elaborate on this and explain what they mean. Do they suggest the need to run CTs on soil samples in addition to conductivity and diffusivity measurements?

Reply:

Here we expressed our vision of future soil characterization. We reported strong correlations between macropore flow and X-ray CT derived macropore network characteristics for both biopore- and matrix-flow dominated cases. The governing macropore network characteristics for each case are revealed by means of best subset regression models (**Table 2 & Page 17 Lines 7-25 and Page 18 lines 1-2**). The next logical step is the application of fluid dynamics simulations (e.g., lattice Boltzmann model) to predict conductivity and diffusivity from segmented X-ray CT data. In the future, this could replace laborious standard laboratory soil characterization (**Page 19 Lines 23-26 and Page 20 Lines 1-7**).

Novelty of the study is now clearly pointed out in the introduction, **Page 5 Lines 14-26**.

Comment 3:

Overall, the new take-home message of the work should be distilled and expressed.

Reply:

Done. The take home messages are better distilled now in the revised manuscript particularly at the following places; abstract (**Page 2, Line 13-24**), results and discussion (**Page 17 Lines 7-25 and Page 18 lines 1-2**), and conclusions (**Page 19, Line 2-19**).

Referee-2

General comments

Comment 1:

The manuscript describes an interesting data set, and shows that macropore effects cannot be neglected at fine scale; in other words, pedotransfer functions and phenomenological models are not yet sufficiently developed to predict the real hydraulic properties of soils, because they do not properly account for macropore characteristics. This is evident from figure 3, which renders the rest of the paper less interesting and relevant. Sorry, but my opinion is that the paper is not suited for publication on HESS. In fact, the data set is very interesting, but data processing and interpretation are performed with standard tools and do not have any innovative content.

Reply:

The first part of the manuscript (Figs. 4 and 5) illustrates that macropore flow cannot be predicted with common pedotransfer functions and empirical models that are based on basic soil properties. So what other options are left to predict macropore flow? With our study we illustrate the utility of X-Ray CT derived macropore network characteristics for prediction of fluid and gas transport properties and the necessity to discern biopore-flow and matrix-flow dominated systems (Figs. 7, 8 and Table 2). The major reason for failure of empirical models is that macropore flow is only weakly correlated with basic soil properties, but strongly influenced by soil structure (i.e. macropore network characteristics). This has been illustrated in the current manuscript via observed strong correlations between macropore flow and macropore network characteristics (Figs. 7 and 8). This is not entirely novel, as a few previous studies (e.g., Katuwal et al., 2015; Larsbo et al., 2014; Naveed et al., 2013; Luo et al., 2010) also showed such correlations. The novel part of this study is the distinction between biopore-flow and matrix-flow dominated soil systems. As a result of this distinction,

the correlations between macropore network characteristics and macropore flow were significantly improved (Figs. 7, 8, and Table 2).

We now developed best subset regression models (Table 2) for macropore flow for each category of soil columns i.e. all soil column, matrix-dominated flow columns, and biopore-dominated flow columns. **Page 17 Lines 7-25 and Page 18 lines 1-2.**

Novelty of the study is now clearly pointed out in the introduction, **Page 5 Lines 14-26**.

Based on findings of the present study, the next logical step is the application of fluid dynamics simulations (e.g., lattice Boltzmann model) to predict conductivity and diffusivity from segmented X-ray CT data. In the future, this could replace laborious standard laboratory soil characterization (**Page 19 Lines 23-26 and Page 20 Lines 1-7**).

Comment 2

However, the paper is well written and organised, but for few problems that are listed in the technical comments below.

Reply:

Done. We have addressed all the raised comments in the revised manuscript as discussed below.

Specific comments

Comment 3:

The term “connected macroporosity" as defined at page 12096, line 29 to page 12097, line 2 is used to describe a connected volume of macropores, which extends from one side to the opposite end of the sample. In the scientific literature, this is often referred to as percolating cluster or percolating connected volume, borrowing the definitions and notation of percolation theory. Since a lot of connectivity indicators have been defined in the scientific literature, I think that the use of percolating macropores" should be preferred.

Reply:

Done. We have replaced connected macroporosity with percolating macroporosity throughout the manuscript.

Comment 4:

Page 12101, lines 23 to 25. I think that the statement “The density-corrected...biopores." is not supported by the data.

Reply:

Done. This sentence is revised now. Page 13 Lines 1-4.

Comment 5:

I think that it would be great if the authors stored the measurement results in a public data repository, making them open to the whole scientific community, so that other researchers can profit from their excellent experimental work to improve phenomenological models and to perform further analyses that could permit to extract more information from this data set.

Reply:

Done. All the measured data (location, texture, saturated water permeability, air permeability, gas diffusivity, and X-ray CT analyzed macropore network characteristics) has been provided now in the supplement.

Comment 6:

Page 12092, line 21. Erase “-0.5".

Reply:

Done. Page 3 line 17.

Comment 7:

Page 12093, line 28. Rephrase the sentence “However...yet".

Reply:

Done. Page 4 lines 21-23.

Comment 8:

Page 12095, line 3. Replace “1.69 ha" with “1.69-hectars-wide".

Reply:

Done. Page 6 line 3.

Comment 9:

Page 12095, line 5. Geological Survey of Denmark and Greenland (1999) is not listed in the references.

Reply:

Done

Comment 10:

Page 12095, line 13. Replace “ID", possibly with “internal diameter".

Reply:

Done. Page 6 line 12.

Comment 11:

Page 12096, line 7. Substitute “Jassonge" with “Jassogne".

Reply:

Done. Page 7 line 7.

Comment 12:

Page 12101, line 9. Please rephrase “both models", by explicitly writing which models are used to compute data for Figure 3.

Reply:

Done, Page 12 Lines 11-14.

Comment 13:

Page 12101, line 23. Substitute “und" with “and".

Reply:

Done. Page 13 line 1.

Comment 14:

Page 12111, lines 17 to 19. The citation to this paper is missing in the text.

Reply:

Done, removed from the reference list.

Referee 3

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 reviewers notion that macropore flow was previously related to basic soil properties. Though this is true for fluid permeabilities (saturated hydraulic conductivity and air permeability, Fig. 4 and Figs. 5A and 5B), 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 (Figs. 5C and 5D).

The second part of the manuscript (Figs. 6, 7, 8 and Table 2) 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 (Figs 7, 8 and Table 2). 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 now developed best subset regression models (Table 2) for macropore flow for each category of soil columns i.e.

all soil column, matrix-flow dominated columns, and biopore-flow dominated columns. **Page 17 Lines 7-25 and Page 18 lines 1-2**.

Novelty of the study is now clearly pointed out in the introduction, **Page 5 Lines 14-26**.

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:

Done. We agree with the reviewer and therefore the term "field scale" was removed 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:

Done. We have revised the manuscript title now.

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:

Done. Page 9 lines 21-22.

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 L^2/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. This is changed throughout 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:

Revised. Page 6 line 25.

Comment 9:

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

Reply:

Done. Page 9 line 1.

Comment 10:

P 12098 L 10 add of after "potentials”

Reply:

Done. Page 9 line 4.

Comment 11:

P 12098 L13 5hPa is pressure not a pressure gradient

Reply:

Done. Page 9 line 6-7.

Comment 12:

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

Reply:

Done. Page 9 line 10.

Comment 13:

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

Reply:

Done. Page 9 line 19 and equation 2.

Comment 14:

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

Reply:

Done. Page 10 line 3.

Comment 15:

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

Reply:

Done. Revised in this sentence page 11 line 8.

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. Page 11 line 24.

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. We have provided now all the measured hydraulic data in the supplementary file.

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. Table 1.

Comment 19:

P 12101 L 1-2 delete the sentence "This is quite……

Reply:

Done.

Comment 20:

12101 L 20 change “decade“ to 2 decades

Reply:

Done. Page 12 line 24.

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:

Done. This sentence is revised to make it clear, Page 15 lines 1-3.

Referee 4

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 reviewers notion that macropore flow was previously related to basic soil properties. Though this is true for fluid permeabilities (saturated hydraulic conductivity and air permeability, Fig. 4 and Figs. 5A and 5B), 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 (Figs. 5C and 5D).

The second part of the manuscript (Figs. 6, 7, 8 and Table 2) 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 (Figs. 7, 8 and Table 2). 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 now have developed best subset regression models (Table 2) for macropore flow for each category of soil columns i.e. all soil columns, matrix-flow dominated columns, and biopore-flow dominated columns. **Page 17 Lines 7-25 and Page 18 Lines 1-2.**

Novelty of the study is now clearly pointed out in the introduction, **Page 5 Line 14-26**.

All minor and technical suggestions are addressed in the revised manuscript.

All the measured data (location, texture, saturated water permeability, air permeability, gas diffusivity, and X-ray CT analyzed macropore network characteristics) has been provided now in the supplement.

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 2^{o}C. This is corrected now in the revised manuscript, Page 6 line 19.

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 has been introduced at the start of the section 3.3 that is focused on the correlations between macropore flow parameters and macropore network characteristics, Page 13 lines 15-26.

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 (Figure 1) has been prepared and included in the manuscript now explaining CT-derived matrices.

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:

Power functions were only fitted if they were significantly better (R^2 value) compared to that of linear functions, page 10 lines 9-10.

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 is better explained now in the manuscript, Fig. 1D & Page 7 lines 21-24.

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 is 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:

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 (Page 19 lines 20-22). However our aim here is to test the performance of PTFs when the degree of macropore flow is very high (five orders of magnitude of saturated hydraulic conductivity). Yes the main limitation for the PTFs is that they take into account soil textural properties but ignore soil structural properties, particularly biopores. And this is the main reason of the failure of the PTFs, **Page 12 lines 8-23**.

We are illustrating 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 system and matrix-flow dominated system. So any future empirical models or PTFs must take into account soil structural features as well as two distinct macropore flow processes

(matrix and biopore). Further, we have developed best subsets regression models to reveal significant macropore network characteristics for predicting macropore flow for each of the cases (i.e. biopore and matrix-flow dominated systems), Page 17 lines 7-25 and Page 18 lines 1-2. 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, **Page 19 lines 23-26 and Page 20 lines 1-7**.

Minor editorials:

Comment 8:

P12091 L8: of its inherently

Reply:

Done.

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, Page 2 line 16-17.

Comment 10:

L19: potential, particularly (comma use)

Reply:

Done. Page 2 line 16.

Comment 11:

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

Reply:

Done, Abstract is revised now.

Comment 12:

L2: for a digital

Reply:

Done. Abstract is revised now.

Comment 13:

P12093 L3: replace ‘large presence’ with ‘abundance’

Reply:

Done. Page 3 line 25.

Comment 14:

L6: first by

Reply:

Done. Page 4 line 1.

Comment 15:

L21: along with the prediction

Reply:

Done. Page 4 line 15.

Comment 16:

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

Reply:

Done. Page 4 lines 21-23.

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. Page 5 lines 14-26.

Comment 18:

P12095 L14: in the summer of 2012

Reply:

Done. Page 6 line 13.

Comment 19:

L16: move the word ‘stepwise’ to after ‘cylinders’

Reply:

Done. Page 6 lines 15-16.

Comment 20:

L18: move ‘step by step’ to the end of the sentence

Reply:

Done. Page 6 line 16.

Comment 21:

L20: from the field

Reply:

Done. Page 6 lines 17-18.

Comment 22:

L27: using the method of Kulkarni et al

Reply:

Done. Page 7 line 10-13.

Comment 23:

P12097 L17: calculated as the ratio

Reply:

Done. Page 8 line 17.

Comment 24:

L20: and was defined as

Reply:

Done. This is revised now.

Comment 25:

P12098 L13: for laminar flow

Reply:

Done. Page 9 line 6-7.

Comment 26:

P12099 L21, ‘mainly’: Preferably state all the texture classes

Reply:

Done. Page 11 line 3.

Comment 27:

P12100 L13: north side of the field

Reply:

Done. Page 11 line 15.

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. Page 11 line 26 and Page 12 line 1.

Comment 29:

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

Reply:

Here we have just converted Ksat into kw so that a comparison can be made with ka (Page 9 lines 21-22).

Comment 30:

L14-15: over-predicted under-predicted

Reply:

Done. Page 12 lines 20-21.

Comment 31:

L24: comparatively fails? Does it fail or not?

Reply:

Revised, Page 13 lines 1-4.

Comment 32:

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

Reply:

This is revised in the manuscript now.

Comment 33:

L27: between the two measures

Reply:

Done. Page 13 line 26.

Comment 34:

P12103 L1: if the image

Reply:

Done. Page 14 line 3.

Comment 35:

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

Reply:

Done. Page 14 line 3.

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:

Revised. Page 15 lines 6-12.

Comment 37:

L10, 18 and elsewhere later: explain ‘moderate and significant power regressions’, modify terminology as necessary.

Reply:

Revised in the 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. This is revised in the manuscript.

Comment 39:

P12106 L3: despite this

Reply:

Done. Page 19 line 3.

Comment 40:

L7: for the prediction of

Reply:

Done. Page 19 line 10.

Comment 41:

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

Reply:

Done. Page 19 line 12.

Comment 42:

P12107 L5: of a digital

Reply:

Done. Page 20 line 6.

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:

Done.

Comment 44:

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

Reply:

Done. This is defined based on correlation coefficient. Revised now figure 7 caption.

Comment 45:

Caption of Figure 7: if found significant.

Reply:

Done, Figure 8 caption is revised now

Short comment

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 the manuscript is revised now by following all of the 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:

Done, **Page 5 lines 14-26**.

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:

Done. A new figure was prepared and provided now in the revised manuscript as **figure 1** explaining all morphologic measures.

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, 7, 8 and Table 2) 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 (Figs 7, 8 and Table 2). 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 now developed best subset regression models (Table 2) for macropore flow for each category of soil columns i.e. all soil column, matrix-dominated flow columns, and biopore-dominated flow columns. Page 17, Line 7-25 and Page 18, Line 1-2**.**

Novelty of the study is now clearly pointed out in the introduction, Page 5, Line 14-26.

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:

Done. Figure 1.

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 2. This reflected that their spatial patterns were highly different. Autocorrelations were observed for soil textural properties but not for hydraulic properties. We have found that it is not worth to show spatial patterns of macropore morphologies. This is because macropore morphologies predicted well hydraulic properties only when soil columns were divided into two categories i.e. matrix-flow dominated and biopore-flow dominated columns.

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

Reply:

Done. We acknowledged this now in the revised manuscript. Page 5 lines 14-26.

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 and we agree on this. We have provided this data to show that measured saturated hydraulic conductivities were laid on five orders of magnitude.

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 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.

Reply:

We agree that currently we are not predicting, instead we are fitting regressions between macropore network characteristics and macropore flow parameters. In the revised manuscript, we have further carried out best subsets regression analysis to find out the most significant macropore network characteristics for predicting macropore flow parameters (Table 2). We here showed that empirical models/PTFs are not able to predict macropore flow when e.g. saturated hydraulic conductivity varied over 5 orders of magnitude as in the present study. In this scenario, X-ray CT analyzed macropore network characteristics showed the promising potential (Figs 7 and 8, Table 2). Predictions of macropore flow based on X-ray CT derived macropore network characteristics can be standardized in future depending on the resolution of the CT data.

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 was true. We have revised this section now, Page 5 lines 14-26.

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:

Done. We agree and have removed the comparison of different segmentation methods from the revised manuscript and focused on the method developed by Kulkarni et al. (2012) only.