First of all, we would like to thank the Anonymous Referee #1 for the detailed and very instructive comments. In the revised paper, all these comments will be carefully taken into account.

The comments made by the Anonymous Referee #1 are in italic, our answers are in bold.

General recommendation:

The paper describes how X-ray microtomography data can be used as auxiliary information to improve the estimation of hydraulic properties near saturation. The approach is promising as it is generally accepted that near-saturated hydraulic measurements are afflicted with the highest uncertainty. The paper is well written and fulfills style criteria almost completely. However, the paper cannot be published in its present state. The authors claim that the results are preliminary and the focus is rather on methodological aspects. Yet, for a method paper the authors failed to provide enough information on how they processed their image data. I reckon that their findings strongly depend on image segmentation, as well as image enhancement and postprocessing, given that a Luvisol usually exhibits a lot of porosity close to image resolution (34 µm). The authors should at least run a sensitivity analysis to demonstrate the effect.

We agree that methodological choices are of great importance in X-ray microtomography. It is a fact that results directly depend on them. As the Anonymous Referee #1 notices it below, as well as Baveye et al. (2010), there is a lack of user independency in regard with microtomography processing results. These aspects need to be considered, and as a matter of facts, a manuscript about this - in the context of our study - is being prepared. It gives all details concerning preprocessing, segmentation and postprocessing choices. It compares results depending on these choices. However, the central point of the present paper is to show that microtomography is a competitive tool for routine soil characterization. We think that segmentation choices do not impact the principle of the methodology itself.

Specific comments:

2. The references for the proposed image processing tool chain are not appropriate. Plougonven (2009) is a dissertation in French and Beckers et al. (2012) is an abstract for a conference. My suggestion would be to use this paper as an occasion to properly introduce your methodology.

The methodology we used is based on the methodology described in the following references (in English):

> **Plougonven, E.: Link between the microstructure of porous materials and their permeability. PhD Thesis, Université Bordeaux 1, 2009.**
>
> **Plougonven, E., Bernard, D.: Optimal removal of topological artefacts in microtomographic images of porous materials, Advances in Water Resources, 34, 731–736, 2011.**

Some modifications have been made four our application, these modifications being included in the present draft.

3. You've cited Baveye et al. [1] which is a good source to highlight the user dependency of image processing results. The recent draft missed the chance to convince the reader that image processing results might in fact be not too user-dependent. I assume, that the histograms of your image are barely bimodal due to severe partial volume effects, i.e. silt loams usually exhibit a lot of porosity in the size range close to image resolution (34 µm) and therefore have a high volume proportion of voxels in the gray value range between pores and solid. In this case, simple thresholding will lead to a fair amount of misclassification errors and should be replaced by a locally-adaptive method, e.g. hysteresis segmentation, indicator Kriging, Bayesian Markov randomfield segmentation [2-5]. Also, Otsu's method is known to be biased if the volume proportion and variance of the two classes are imbalanced which is very likely for soil images like that [6]. There are three things that you should do:

a. Create a new figure with some representative histograms. Maybe I'm wrong and the histograms are clearly bimodal.

In fact, uni- or bimodality of our histograms depends on the soil. Some of them are clearly bimodal, others being unimodal. As a result, segmentation is not obvious, indeed. As a reference, we use the paper of Wang et al. (2011) which compares notably the global method of Otsu (1979) and the Indicator Kriging method of Oh and Lindquist (1999). This paper states that considering unimodal histograms, the Otsu's method is the least worse. Consequently, we chose to use this method, but carefully, since all the samples were controlled and validated. As it was said in the paper, we used a single threshold value for all the samples. This value matches quite well with the middle of the histograms peaks when bimodal. Furthermore, this value supplies mean porosity values agreeing with the measured porosities.

b. Replace simple thresholding by a locally-adaptive method. I'm sure Avizo provides a couple of solutions.

Review papers or research articles comparing segmentation methods (for example Iassonov et al., 2009; Wang et al., 2011) show that there is no ideal segmentation method in the context of soil analyses. We think that the method has to be chosen considering the context of the study. Following Iassonov et al. (2009), factors to be taken into account are principally "the reliability and consistency of the method, as well as the computational efficiency and the automated character of it". Local vs. global methods show different advantages considering these factors. A global method can be chosen because of its rapidity and because it is less demanding for computational power. Even though local methods are very promising, they are still not efficient for every context. We also note that Iassonov et al. (2009) indicate that local methods seem to be more user dependent than the global method of Otsu (1979) since they demand expertise. However, we agree that it could be very interesting to test a local method on our samples – for example the new method of Houston et al. (2013) which seems very promising - and check the effect on the results. But we think that these investigations are beyond the scope of this paper.

c. Do a sensitivity analysis, i.e. plot porosity, specific surface and connectivity as a function of threshold for a suitable threshold range. I reckon, porosity won't change all that much, but specific area and connectivity definitely will.

Like the Anonymous Referee #1 notices, our soil pore size distribution shows a superimposition with current resolution that can lead to severe partial volume effect. As a consequence, our results could be considered highly dependent in regard with the processings. However, the studied objects are soils with structural differences only. Our approach is comparative and our choices impact can be supposed equivalent for the different studied objects. This is a fair hypothesis if the threshold value applied here is not too far from the optimal value. Considering precautions we took at the different steps of the images processings on the one hand and the good match between derived and measured hydraulic properties on the other hand, this statement seems to be fair enough. Furthermore, as it was said in the introduction, we tested this statement in the context of our study. This test showed that this impact is in fact not too important, and that the methodology is not questioned. However, detailed results of this test deserve to be presented properly in another manuscript, and are beyond the scope of this present draft.

Finally, we propose for this paper to rewrite the discussion, including these statements. We also propose to present our segmentation choice more clearly, as well as to present local segmentation methods as interesting alternatives.

5. The conclusions have to be shaped up a little. What makes the paper stand out against previous papers that combined image analysis with hydraulic properties? How can additional information beyond macroporosity, like specific surface area or specific connectivity really be used to improve model predictions? What are the limitations of this approach?

P 4815, l 13-15: How could this refinement be achieved?

Past (e.g. Vogel and Roth, 1998; Vogel et al., 2005) and more recent research in this topic (e.g. Rezanezhad et al., 2009; Elliot et al., 2010; Rezanezhad et al., 2010; Köhne et al., 2011; Schlüter and Vogel, 2011) show its interest and the lack of a unanimous answer. In fact, we can't still predict accurately soil macroscopic behaviour. Moreover, like the Anonymous Referee #1 reminds us in the introduction, near-saturated hydraulic properties estimation is prone to high uncertainty. As a consequence, the link between pore parameters and hydraulic properties still needs to be investigated.

We made the choice here to work with a real soil and, more than this, a silt loam. It is scarcely used to develop methodologies in image analysis: it is a heterogeneous soil, and its pore size distribution shows a superimposition with current resolution. Like the Anonymous Referee #1 notices, it can lead to severe partial volume effect. But this kind of soil is widely met and the need for its study is evident. As a result we chose to work with a comparative approach, the studied objects being silt loam soils with structural differences only.

The central point of this paper is to show that X-ray microtomography brings supplementary and complementary information to macroscopic measurements in this particular context. It is a bridge between micro- and macroscopic approach. Using 3D soil structural parameters measured with X-ray microtomography, previous research that compares micro- and macroscopic measurements does it on the same sample (e.g. Rezanezhad et al., 2009; Elliot et al., 2010; Rezanezhad et al., 2010). They generally use 1 or 2 samples, scarcely more, and microstructural parameters are directly linked –with more or less success-with macroscopic properties. In our study, we work with far more repetitions (i.e., 8 samples) and we show that a mean macroscopic behaviour can be relied with a mean behaviour at the microscopic scale. Furthermore, we put in evidence microtomographic parameters which could contribute to explain mean hydrodynamic behaviour of these soils.

Besides, most research works with pore modelling (e.g. Köhne et al., 2011; Schlüter and Vogel, 2011), while we show that X-ray microtomography can be used to enhance plot scale models. More specifically, the pore size distribution derived from microtomography allows us to enhance near saturation hydraulic function through parameterization of the Durner's double porosity model (1994). The next step being the improvement of these functions thanks to X-ray microtomography structural parameters, for example by including a connectivity- and/or a specific surface-based parameter in the function implementation. X-ray microtomography could thus improve water flow modelling at the macroscopic scale.

As a conclusion, our research permits to show that: i) we have an agreement between measurements at the macroscopic and the microscopic scales ii) behaviours at these different scales can be linked together iii) microscopic information can enhance macroscopic description and modelling.

Technical comments:

P 4804, l 13-20: How did you collect the samples? Silt loam samples are prone to sampling artifacts like soil compaction or cracks close to the sample container. A figure with some vertical slices or 3D cuts provide a good lead how severely your samples are affected by these problems.

We collected the samples very carefully. However, as the Anonymous Referee #1 notes it, samples are prone to cracks close to the sample container. As a result, we reduced a little the radius of the investigated regions to eliminate these effects. Final radii were the same for all our samples.

P 4806, l17-19: Taking only one sample out of eight makes the values quite arbitrary. Consider taking at least the highest three replicates out of eight.

p 4813, l 8-10: You've mentioned that you measured Ksat in two directions. To which of them do you refer here?

Both these comments refer to the same issue. We didn't make our point clear here. In fact we calculated the mean value for Ksat in each direction for all the samples, i.e. eight samples for each direction. Then we chose the highest value between the mean horizontal and the mean vertical one, considering that the 3D pore network characteristics are more correlated with this highest value.

P 4813, l 12-23: Why don't use the permeameter measurements to condition your model and fit both h(_) and K(h) simultaneously?

Since we have only 3 points in the unsaturated zone of the K(h) curve, the fitting is not improved including them in the process. As a result, we think that they are more useful as a validation of the method.

P 4814, l 21-24: Be more specific about 'present an enhancement'. How does the separation of pores improve hydraulic functions? Pore size distributions, surface area measurements or connectivity metrics can also be obtained for the entire pore space, without the requirement to label each pore cluster individually. What would be the advantage to do so?

Indeed, all these parameters can be calculated for the entire pore space. We think there are great advantages to separate the pores individually. First, it is possible to analyse the characteristics variation against the pore size for example, and thus to obtain characterisation for defined pressure head range. Second, it is then possible to derive pore characteristics depending on their orientation.

P 4815, l 7: Why should mean object position in small samples (5x3cm) be important at all with respect to effective horizon properties? This creates the impression, that you just used any parameter that was available in Avizo without thinking it over whether they make any sense.

The "mean object position" referred to the position of the solid dots in the figure 4, it is not referring to pores positions in the sample or to a structural parameter.

P 4815, l 17: Maybe I missed it, but where did you present results for the dispersion coefficient? Is it in the PCA figures or hydraulic functions? The entire paragraph seems to be barely supported by any figure or table.

The comment is right; we have to include tables to support these statements.

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