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Interactive comment on "Three dimensional soil organic matter distribution, accessibility and microbial respiration in macro-aggregates using osmium staining and synchrotron X-ray CT" *by* B. G Rawlins et al.

Anonymous Referee #1

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This is a very interesting and innovative study on a novel and exciting topic of relationships between soil organic matter and pores in soil macro-aggregates. The authors are using a novel osmium-staining CT-scanning based technique to identify SOM and are utilizing very sophisticated tools for in-depth spatial analyses of the image data. The manuscript certainly warrants publication, however, there is a need for some revisions.

My main concern that must be addressed before the manuscript is published is the method of pore size characterization used in the study. It appears that the researchers used an object identification algorithm to identify individual pores and then used vol-

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Discussion paper

umes and a shape factor of the identified pores as one of the main tools in characterizing them. I am afraid I have to say that this approach is quite meaningless, and probably some of the lack of pore effects reported in the study is just reflecting the fact that inadequate criteria of pore characterization were used. This approach completely ignores pore diameters and tortuosity - that is, the pore characteristics that are most relevant to their functioning. Say, we identified two pores with approximately the same volumes. One of them could be a thin and long tortuous pore, the other can be a large round cavity. Their functioning in terms of water, air, microbes, SOM decomposition, anything, will be completely different, yet in the classification system of this study they will be lumped in the same size class. While the distance from the pore component of the study is valid and interesting, the components that are based on the object-based pore identification (Figs. 4 and 5) should not be included in the manuscript.

Minor items: p.3 l. 6 -the part regarding representative volume does not seem to fit with the rest of the study.

p.3 l. 10 - something is missing after "and"

The experimental part seems to be very thoroughly conducted. I am curious - what was the need in using glass beads? Not having them would simplify the authors life a lot in terms of creating aggregate masks.

Are all these details in describing how the aggregate masks were created really needed? A lot of the steps talk about in-house R codes or macros and, without those provided as part of the manuscript, this procedure description is not something that anybody from the audience can even try to reproduce.

I think it is unfortunate that the authors decided to aggregate the image data. Why not just use the subsections of the original 3 micron resolution data sets?

While I do not see it as a big problem for the current study I believe in future the authors should seriously consider the need to look not just at pores in general, but to keep in

mind that depending on their diameters and other characteristics pores can function very differently. The authors expectations regarding pore-emission-SOM relationships that are expressed in the manuscript are not reasonable for many pore types/sizes.

I agree that scaling CO2 emission by TOC makes sense, but just for "quality" check - was there a positive correlation between SHS and TOC? Because if everything worked as expected there should be one, and it would be nice to hear about it. If there was none, it is also important to report.

p.10 l.30 The discussion on differences between aggregate and bulk soil findings is a bit simplistic. It is a basic soil science knowledge that density of aggregates is typically greater than soil bulk density (simply put, soil bulk consists of aggregates and large pores among them). Much more interesting would be comparisons of porosity, density, etc. results of this study with literature data that were collected on the same spatial scale (i.e., based on aggregates).

Please take a look at the following source for assessments of soil organic matter density:

Mayer, L.M., L.L. Schick, K.R. Hardy, R. Wagai, and J. McCarthy. 2004. Organic

matter in small mesopores in sediments and soils. Geochim. Cosmochim.

Acta 68:3863–3872. doi:10.1016/j.gca.2004.03.019

In Table 1 and in other places that mention porosity it should be noted that here we are looking at image-based porosity that reflects volume of pores above certain threshold.

I am very excited about lower OP probabilities results of this study. To me it is an indirect indication of pore presence to be conducive to OM decomposition.

Fig.6 - maybe do not show PP, OO, and MM values? They are not informative and without them the differences in other transition groups will be more visible.

I have to admit that what is shown on Fig.7 and its relationship to what is shown on

Fig.6 eludes me.

Figs. 8 and 10 - even though the relationships are not significant, adding regression line, p-value and r2 would be good.

I understand the driving for reporting the probabilities as the main outcome of this study from the modeling perspective, but can this probability information be somehow presented in units of actual distances? I believe it would be of interest to greater audience.

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