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Interactive comment on "Simulating microbial degradation of organic matter in a simple porous system using the 3-D diffusion based model MOSAIC" *by* O. Monga et al.

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a. This is an interesting manuscript that would be a solid contribution to SOM decay modeling. However, there are two issues that ought to be addressed before the revised manuscript is considered for publication. First, I appreciate the use of data from one water content to set parameters and data from the other to evaluate the model. However, wouldn't you expect those parameters to vary as a function of water content? You state that "It is more likely that the matric potential primarily affected mineralization through its control on the substrate diffusion rate through water filled pores." But what's the justification for this statement?

In the experimental condition described in the paper, we assumed that the biological parameters of the model did not vary, with water content. As a consequence, differences in biodegradation rates between the two water contents only came from variations of fructose diffusion rate to microorganisms. One reason is that the two water contents are relatively "wet" conditions (-10 cm or 0.001 MPa and -100 cm or 0.01MPa). Hence, it is reasonable to assume no direct effect of moisture on the physiological state of microorganisms. Second, the prominent role of diffusion on biodegradation variations with moisture was recognized and discussed in several papers (e.g. Harms, 1996, Dechesne et al., 2010, Moyano et al. 2013). We justified this in page 12 : "We hypothesized that the physiology of microorganisms, i.e. their maximal growth rate, constant of half saturation or mortality rate, were not affected by the decrease in moisture, because the water potential remained too high to cause a physiological stress (Manzoni et al. 2012). The bacteria are supposed to be attached to the solid particles and are submitted to the same micro-environment at -10 or -100 cm of water. Several studies have found that diffusion limitation was the main factor explaining the decrease of respiration with soil moisture (e.g., Harms, 1996, Dechesne et al., 2010, Moyano et al. 2013). A meta-analysis by Manzoni et al. (2012) showed that moisture soil respiration curves were not affected by microbial community composition, which they interpreted by other factors than microbial physiology controlling respiration, i.e. solute diffusivity."

Also, within the same paragraph, the results for both water potentials are treated as result when, in fact, one set was used to estimate the parameters. I think it's important that the data used to separate parameters are clearly distinguished from those used to evaluate the model - these must remain independent statistically and w/in the manuscript.

We agree with the referee's comment and modified our manuscript accordingly. We reorganized the part 2.4 of the manuscript by splitting into a calibration section using one water potential and an evaluation section using the other water potential. We added errors bars obtained from replicates in figures 6 and 7.

b. Second, the paper implies that this model is an improvement over other approaches. Yet there are no data to back this claim. Is it possible that simpler models could explain this observation set as well? If so, is this really an advance? If not, where are the data to demonstrate this?

We used a simpler model using the moisture function of Roth-C and we compared the model results with Mosaic II. We added a section 2.5 Simulating microbial decomposition with a simple approach. Figure 6 was changed to show also the results of the simpler approach. The result section was also corrected.

In this paper, we have tested for the first time the ability to simulate the change in mineralization taking into account explicitly the soil structure and the change in water distribution using real data. This paper is the first step of our model test using real data of mineralization and TC images. We have changed the last sentences "Our modeling exercise gave results as good as those obtained using a simple moisture function found in the literature. We anticipate that for more complex 3-D architectures, such as those of soil compared to sand, the simulation using MOSAIC should be superior. Two directions for improvement can be identified: (i) using CT images with a better spatial resolution, as it is increasingly possible using new generation μCTs, in order to describe and model the processes within micrometer scale pores and (ii) describing better the water connectivity at low water content in the model, accounting for water films. In future studies more scenarios will be tested using more complex systems (real soil and more complex bacterial community) and for different distances between degraders and organic substrates."

Interactive comment on Biogeosciences Discuss., 10, 15613, 2013.

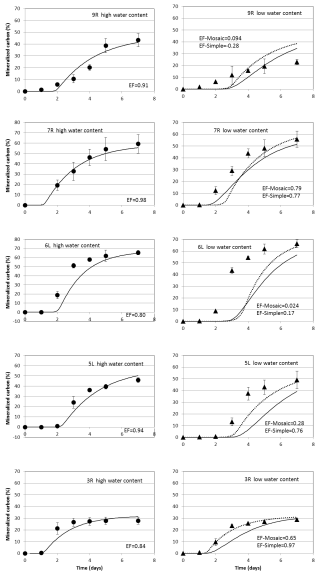

Figure 6: Experimental (symbols) and simulated (line) results for the 5 bacterial species inoculated at high and low water contents with their respective efficiency

Fig. 1. new figure 6 of the manuscript