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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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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? Also,

within the same paragraph, the results for both water potentials are treated as results 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.

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?

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