

## ***Interactive comment on “Response of microbial decomposition to spin-up explains CMIP5 soil carbon range until 2100” by J.-F. Exbrayat et al.***

### **Anonymous Referee #2**

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#### General comments

“Response of microbial decomposition to spin-up explains CMIP5 soil carbon range until 2100” could be interesting to readers in Geoscientific Model Development. This paper clarified how to differ initial global SOC stocks among ESMs in CMIP5 experiment and the initial condition considerably governed future global soil stock behaviors even under the extreme climate change scenario in ESMs. Although the results and messages of this manuscript are very simple, I think that this study can still contribute to the further improvement in ESM due to seriously lack of constrains of initial global SOC as in this study.

I agree the overall comments given by Referee 1. Additionally, there are two major questions and some individual comments on here.

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# I thought the key finding in this study is that the soil decomposition processes is more dominant process to determine the initial global SOC stocks of current ESMs than C input onto soil from photosynthesis production. So, I recommended this finding should be emphasized more by additional analysis. For example, instead of just comparing between two linear regression analyses (Fig.3&4), can you analyze the relative importance of these two explanatory variables to total SOC?

# In fact, we are not sure the actual earth system getting the equilibrium in the global SOC stock even at industrial era. In addition, SOC accumulation and soil genesis need millennial time scale in situ. So, we can also choice the non-equilibrated state for global SOC stock in simulation. It means that we can get initial states of global SOC stock to be reaching the reference global SOC stock (HWSD) in spin-up procedure before getting the equilibrium (although this method is not used for C, N, O.). If GPP are well constrained by observations, this might seem not to be too worse option. Do you have any recommendation about whether getting the equilibrium of global SOC or not in spin-up procedure?

Individual comments

P3488L8-9 If you have any literatures using such an explanation, please cite here.

P3488L29- P3489L2 I don't think these statements are meaningful. During the historical periods, it is likely that all models without N cycling scheme are parameterized under N limitation conditions. Therefore, the comparison between them doesn't give any information in this context.

P3488L26- P3489L8 You should mention the differences in the variation between SOCin and decay constant among ESMs. Especially in SOCin, are there any comparable values in previous literatures?

P3489L9-18 Are there any relationships between SOC residence time and (mean?) decay constant "k" in each ESM or between SOC residence time and the number

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of components in each ESM? This is the important information how to adjust decay constant of ESMs?

Fig. 1&2 Please re-size the aspect ratio to be 1:1 (X axis: Y axis) of all figures.

Fig. 3&4 Please line up these two figures.

All figures There are too large significant digits in regression results.

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Interactive comment on Geosci. Model Dev. Discuss., 7, 3481, 2014.

**GMDD**

7, C978–C980, 2014

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