

Interactive comment on “Stable soil organic carbon is positively linked to microbial-derived compounds in four plantations of subtropical China” by H. Wang et al.

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This paper has dealt with relationships between soil organic matter composition and microbial composition. This is within the scope of Biogeoscience. Major discussions are based on the results of PLFAs and NMR. There are following three serious problems in this paper.

1. Absence of quantitative data and discussion including total soil organic C
2. Overestimation of chloroform-fumigation extracted microbial biomass-C
3. Low fungi/bacteria ratio, or failure to extract fungal PLFAs

Regarding the above point (1), without quantitative analyses (discussion including

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SOC, SON data), the authors cannot discuss the source of SOM or accumulation of stable SOC. Even if there is no correlation between microbial parameters and SOC NMR index, microorganisms are always important precursors and producers of stable SOM, as mentioned by the authors. In addition, necromass of microorganisms are more or less decomposed during humification processes. Therefore, similarity or correlation between microbial parameters and SOC NMR index cannot support their direct cause-and-effect link.

The above point (2) can be a fatal flaw of this study. Soil sampling should be conducted to meet preconditions for analytical methods. Chloroform fumigation technique destructs cell walls of microorganisms, however, it also destructs cell walls of plant materials. This generally leads to overestimation of microbial biomass when samples contain the large amounts of organic materials. Thus, this method is not applicable to soil samples including organic horizons. The high microbial biomass in this study may be related to this problem. Please calculate the percentages of microbial biomass-C relative to SOC to confirm validity of the data.

Regarding the above point (3), F/B ratio is very low in this study. The data are very unique for results from the forest soils. Please compare with the previous data and add explanation. I am afraid that the authors failed to extract or determine fungal PLFAs. In this case, the following discussions of this paper would be meaningless.

Specific comments:

The title is ambiguous. What aspects of “Stable soil organic carbon” are linked to what of “microbial-derived compounds”? Based on the manuscript, chemical composition of stable soil organic carbon is linked to microbial composition.

P18095 L22-23 Please clarify the definition of “labile”. Some of polysaccharides are easily solubilized to monomers (labile to microorganisms), but the others form complexes with aromatic compounds (recalcitrant).

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L26 1998 is not recent. 15 years ago. Stable soil organic carbon is not composed only of “aliphatic” compounds.

P18096 L25-27 There appears to be misconceptions regarding the roles of microorganisms in SOM formation. As the authors mentioned, most of SOM is microbially-processed compounds. However, this does not always mean that necromass of microorganisms are simply sequestered and that this process accounts for 80% of SOM. The ultimate source of SOM is plant materials and SOM is formed through microbial processing. To state that necromass of microorganisms account for 80% of SOM accumulation, please confirm whether the origin of aromatic compounds in SOM is microorganisms or not.

P18098 L5 Revise “controlling factors driving” to “factors controlling”.

P18100 L26-27 “The organic horizons were not separately analyzed. . . .” This is a fatal flaw of this study. Soil sampling should be conducted to meet preconditions for analytical methods. Chloroform fumigation technique destructs cell walls of microorganisms, however, it also destructs cell walls of plant materials. This generally leads to overestimation of microbial biomass when samples contain plant materials. Thus, this method is not applicable to soil samples including organic horizons.

P18100 L20-21 The authors specified “Fe³⁺” and “Mn²⁺”. Did you confirm that the Fe and Mn status of the soil are “Fe³⁺” and “Mn²⁺”, not Fe²⁺ or Mn⁴⁺?

P18103 L23 What is “stable SOC content”? Please define.

P18104 L9-10 There is no discussion on concentrations of total SOC, although accumulation of stable SOC was discussed. This is one of major defects of this paper. L12 “from environmental changes” is ambiguous for scientific paper.

P18106 L10 “contribution of fungi” to what? Please clarify.

L11-12 This study has dealt with “fungal abundance” using PLFAs, but not “fungi-derived compounds”. There is a great leap of logic. Is it possible to use PLFA as

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an index of “fungi-derived compounds” as well as “fungal abundance”? For this, PLFAs should not be degraded during humification process.

P18107 L12 “determining the stable SOC pool” was not dealt with in this study. There is a great leap of logic. The authors investigated the composition of SOC, not pool size of stable SOC.

I hope these comments will be helpful for improvement of the manuscript.

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