

Interactive comment on “Soil resources and climate jointly drive variations in microbial biomass carbon and nitrogen in China’s forest ecosystems” by Z. Zhou and C. Wang

Anonymous Referee #3

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This is an interesting and well rationalized study aiming to examine the patterns in microbial carbon and nitrogen by climate zones and management regimes for China’s forest ecosystems, as well as identify the factors driving its variability. Before its acceptance for publication in BG, I have some suggestions on their manuscript.

General comments

Because of the lack of confirmation of some statistical analyses, some points of the discussion are difficult to assess. I think that the discussion needs to be reviewed in some points in order to be less speculative, based on the results and avoiding big conclusions that are not supported by the present findings. The conclusions should be

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re-writing to summarize the major contributions of the manuscript.

Specific comments

P195 L7: As reviewer #2 suggested: please explain why the authors collected the studies only starting from Jan. 2000. L9-10: In the case of the data collection in the Web of Science, the authors only used the words “soil microbial” and “forest” as key words? How they limited the search to the target region? L13-14: Please explain why the authors considered enough time 7 years of no anthropogenic disturbances or management activities?

P197 L9-11: Please include Bonferroni corrections for all multiple regression analyses (e.g. those reported in Table 1).

P198 L12: Fig. 2a and b should be Fig. 2a and c, as well as L16 Fig. 2b should be Fig. 2c.

P202 L-4: The authors need more support for the assumption that the soil resources on Cmic and Nmic change with the availability and stoichiometry of Csoil and Nsoil. L13-16: These are big assumptions since Fig. 6 show very weak relationships.

Fig. 2 and 5, Please explain how is that the SE of the inserted figures that are the overall comparisons between the natural (NF) and planted forests (PF) are that small?

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