Biogeosciences
Discussions

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Interactive comment on "Soil microbial community structure and diversity are largely influenced by soil pH and nutrient quality in 78-year-old tree plantations" *by* Xiaoqi Zhou et al.

Xiaoqi Zhou et al.

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Dear Editor Prof. Yakov Kuzyakov,

We have carefully revised the manuscript according to reviewers' suggestions. The changes we made have been highlighted in yellow throughout the manuscript as shown at the attached file. We hope that it can meet the standard required by your journal Biogeosciences.

Best regards,

Xiaoqi Zhou on behalf of all authors

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Reviewer #2 First of all please receive all my apologies for being late to deliver my comments on the new version of the manuscript. Overall, I think this manuscript is of very good quality: it is well written and it addresses good scientific questions. Therefore I recommend to the editors to accept this manuscript after some minor revisions. You will find below my main comments. R: Thanks.

The authors propose to study the long-term selection of soil microbial communities in different tree plantations developed on the same soil parent material. 1. In the introduction the general context is well explained. The authors proposed to go further by investigating the soil microbial communities but also their associated crucial function in the context of climate change mitigation (i.e. their CH4-oxidation activity). However, it could be interesting in this perspective to look also to the potential denitrification activity of these communities. Indeed, it is now admitted that NOx can be powerful greenhouse gas specifically in arable lands and where we can observe large NOx emissions. The dataset shows for example that Kauri Pine plots have both the highest EON and the highest relative abundance of the Nitrospirae phylum (known to be implicated in the nitrogen cycle and into the denitrification process). I do not recommend that the authors delay the publication of the manuscript to investigate this question my purpose is to suggest to go further in linking microbial community structure and their functions in future studies. By the way, as I said in the first evaluation of this manuscript, I would recommend to the authors to include in their future studies a plot maintained as arable land. This improvement would allow the authors to distinguish the "afforestation effect" and the "tree species effect" on microbial communities. R: Thanks for your kind comments.

2. Mat&Met: I would ask to the authors why did they include 15N data? They do not really use it in the manuscript (by the way, I did not see the 15N arrows in Fig.4). Moreover, the highest values (indicting a processed/old soil organic matter) are found in Slash Pine and Eucalyptus plots where C/N ratios are also the highest (indicating a

fresh status of organic matter but it is maybe a tree species effect). Briefly, it is more confusing than informative. Therefore I would suggest to explain in more details the 15N patter n observed or not to mention it at all. R: Thanks. Soil δ15N has been added in Fig. 4. Lines 31-32 Page 21.

3. Results. In Table 1 I would recommend to the authors to precise the units for soil moisture: what is represented with this % ? the relative volume of water-filled pore space? the relative volume compared to water holding capacity? I would suggest to give more information on it or to express the data as grams of water per grams of dry soil. R: This has been revised. Line 11 Page 14.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-552/bg-2016-552-AC2-supplement.pdf

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