

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.

Anonymous Referee #2

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Dear authors and editors,

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. Sincerely yours,

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

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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 NO_x can be powerful greenhouse gas specifically in arable lands and where we can observe large NO_x 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.

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 (indicating a processed/old soil organic matter) are found in Slash Pine 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 pattern observed or not to mention it at all.

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 informations on it or to express the data as grams of water per grams of dry soil.

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