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Interactive comment on "Accumulation of soil organic C and N in planted forests fostered by tree species mixture" *by* Yan Liu et al.

Yan Liu et al.

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Dear editor and anonymous referee:

Thanks for your interest and critical comments on our MS. Please find responses below to respective comments. We hope the clarification below will get some positive feedbacks from you and editor. This is the final version because there are flaws of formation in the previous one. This is more convinient to read.

regards,

Pifeng Lei

On behalf of co-authors

Responses to critical comments:

(1) The experimental design compares stand composition effects with one site being 200 km away from the other two and provides not justification how we can assume that the main difference in SOC and n can be attributed to stand age and not the difference in site conditions. Mean climate conditions may be the same but soils are never the same as well as aspect, slope etc. Moreover I did not find any information about previous land use in these sites. The authors discuss that there are significant trends in SOC development with time depending on former land use, so this is very pertinent information to include and discuss. We would need much more detail about all these site conditions to be convinced that the two sites are "similar".

RE: You are right about this experimental design that it would be nice to do this experiment in one site. However, we used two sites here due to the scarcity of young plantations in Hunan Botanic Garden. We also need to point out that our purpose in this study is to evaluate the admixing effects on soil OC and N stocks by comparing mixed forests and corresponding monocultures over time, not to compare the SOC and N stock in each forest types along forest development. Therefore, we always selected three paired plantations, consisting of pure Pinus, Cinamonum plantation and Pinus-Cinamonum mixed plantation within the same age in one site. The principle of selection field site we used was to put three plantation types at the same age in one site. We stick to this principle as well during our analysis. Accordingly, we compared the magnitude of the admixing effects on the SOC and N over time by using the relative values (e.g. percentages of over-performance in mixtures over monocultures, (observed-expected)/expected) at different stand ages, instead to comparing the absolute SOC and N stocks in one specific forest type with stand development. Therefore, the assumption of "the main difference in SOC and n can be attributed to stand age and not the difference in site conditions" would not happen here. In this point, we should justify it in our MS and clarify by adding this principle of site selection into our experimental design sections. For that we appreciate. And we also need to be more careful when discuss the SOC and N along chorosequence in discussion section.

(2) I have problems to understand the experimental design at each of the three sites, but it is relatively clear to me that there cannot be any proper replication of stand age as there is just one site per stand age. It is also unclear whether there were three subplots in each stand which were used as replicates in the statistical analysis – or if the individual cores were used as replicates in the analysis? In any event the statistical analyses must be flawed as we cannot separate the stand age effect from the site effect. Even within the site these subsamples within stands of a certain age are not real replicates for a statistical analysis.

RE: In this point, we must apologize for the confusion. As we have one previous publication (Wen et al. 2014) as reference, we did not make it clear in this MS. We selected three stands, including pure Pinus, Cinamonum and Pinus-Cinamonum mixed plantations in each development stage (10, 24 and 45 years old). And three plots were established in each forest types at each development stage. Thereby our study included 27 plots consisting of Pinus-Cinnamomum mixed plantations and corresponding monocultures at age of 10, 24 and 45 years old. In each plot, 4 soil cores were sampled, sliced into three layers and treated as individual samples for SOC and N analysis. I hope this explanation is clearer and sound to you.

(3) Lastly the authors performed a three-way analysis of variance with all possible interactions. This indicates use of pseudoreplicates, but apart from this they also include "depth" as a factor. This is highly problematic as the three layers are not independent. For instance, if a 0-10 cm layer has a high C concentration then the 10-20 cm layer and 20-30 cm layers underneath are highly likely to also have higher C concentrations. If depth is included in the statistical model, the authors need to account for this correlative structure in their data. This is similar to accounting for e.g. repeated measures analysis when analyzing a time series of data.

RE: The referee is right that we treated all the measurement as pseudoreplicates here

simply to detect the effect of forest types, age and depth on SOC and N.

(4) The most recent literature is not well referenced and addressed in the Introduction and the Discussion. For instance, Guckland et al. (2009) studied beech dilution gradients (J. Plant Nutr. Soil Sci. 172: 500-511) and more recently, Dawud et al. (2016, 2017) studied effects of tree species diversity gradients on soil C and N stocks in mature European stands (Dawud et al. 2016, Ecosystems 19: 645–660 ; Dawud et al. 2017, Funct. Ecol. 31: 1153-1162). RE: They are very good references, we would cite and combine them in our later revised version.

(5) The data basis is a bit thin (only C and N concentrations and stocks in mineral soil), and the authors could leave out C and N concentration from the main manuscript with no major loss of information. Instead, the paper would have been much stronger with inclusion of forest floor C and N stocks. Recent literature has shown that there are also clear dynamics in forest floor C and N stocks as a result of tree species mixtures. In addition the authors also mention a previous study of litterfall in the same sites, and unpublished root biomass data. I strongly suggest these data be included and discussed for a more coherent and strong publication (if the manuscript can be reworked for publication based on revision of the above-mentioned flaws).

RE: It would be more informative to have more data, for example, the forest floor C and N stocks, although the forest floor is quite thin in these site we used here. It is also typical in our area in subtropical area. Regarding the litterfall data, it was measured for other experiment and the data is not complete if we use it for our purpose as the lifferfall was monitored in these three forests at age of 24 year old, instead of all the three age classes. That is why they are not included in our MS. For the fine root data, we recently developed one MS and submitted it Biogeosciences as a companion paper recently as encouraged by the previous anonymous reviewer. Hereby you could find it as supplement attachment. I hope you and the editor find it interesting.

(6) The language of the manuscript needs substantial linguistic checking by a native

English speaker. The wording is not correct in many places and several sentences are hard to understand.

RE: This can be done. We will improve it by correcting or sending it for proof-reading.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2017-62/bg-2017-62-AC3-supplement.pdf

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