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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.

Anonymous Referee #2

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This manuscript reports a study on SOC and N stocks in pure and mixed stands of Pinus and Cinnamomum at three different stand ages representing a chronosequence in the Hunan Province in China.

The aim and focus of the study are well justified, particular the idea to include the stand age effect in the assessment of tree species mixture effects. Several studies have previously studied mixture effects in young plantations (<20-30 years), so this is an interesting aspect of this study. I also appreciated the effort to address whether results were non-additive or just additive.

However, I have several concerns regarding this manuscript which prevents me from recommending it for publication. The experimental design, methods and the statistical analyses include several problematic assumptions and descriptions of methods are not

clear. This is a shame as the original idea of the study is relevant and justified.

The lack of information generally prevents me from being able to fully assess the work done. I have indicated that the manuscript could be reconsidered after major revisions, but this depends on whether the experimental design can support statistical analysis (see details below). If the authors can substantiate that their experimental design is appropriate for scientific evaluation or discuss this in a qualified manner, the revisions would entail a completely reworked statistical analysis as well as a description of sites and methods which fully matches the expectations of a transparent scientific paper. Only after this has been clarified can further details of the study be evaluated.

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".

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.

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.

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).

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 concetrations 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).

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.

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