

## ***Interactive comment on “Soil greenhouse gas fluxes from different tree species on Taihang Mountain, North China” by X. P. Liu et al.***

### **Anonymous Referee #1**

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The authors investigated soil GHG fluxes (CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O) and their potential drivers among the six different tree species on Taihang Mountain of north China. After going through the whole paper, the MS reads like a scientific report. I did not give any detailed suggestions/modifications on the MS itself but presented several large concerns. Firstly, the discussion part can be regarded as a review of previous work and is full of simple listing. I am afraid that the readers can not really get any new insights from this part. There should be many interesting things that could be discussed. For example, the authors selected a good study place where plantation and nature regenerated forests coexisted but the further discussions on the potential similarity/difference of GHGs between these two type of forests and the implications (this may become a eye-catching point for this MS) were absent. Secondly, I understood that the annual fluxes for CH<sub>4</sub> and N<sub>2</sub>O can be calculated based on simple time interpolation because

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these fluxes showed poor correlations with the measured soil properties (e.g. soil temperature and soil moisture). But this method would greatly introduce the significant bias into annual CO<sub>2</sub> flux calculation because CO<sub>2</sub> fluxes were strongly correlated with the soil temperature (and also soil moisture) and their relationships should not be linear. Finally, there are numerous grammar and spelling errors. I strongly suggest that the MS should be carefully checked and revised by the native English speakers if it was resubmitted or submitted to other journals. Based on the above-mentioned issues, I suggest that the paper presented like this should not be published in Biogeosciences.

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Interactive comment on Biogeosciences Discuss., 10, 11037, 2013.

**BGD**

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