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Interactive comment on “Effects of multiple environmental factors on CO₂ emission and CH₄ uptake from old-growth forest soils” by H. Fang et al.

Anonymous Referee #2

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GENERAL COMMENTS

In their manuscript, Fang et al. present a comparison of soil CH₄ and CO₂ effluxes from old forests on different climate in China. They also present site specific relationships of environmental factors to effluxes. The study is based on a nice dataset of three year data on four different sites. The specific aims of the study are clearly formulated and answered. In introduction the authors claim that the dataset makes it possible to study the effects of multiple environmental factors by comparing the sites. However, they fail to provide any results of that. I think it would be an impossible task with only four sites, as too many variables change between the sites. Furthermore, they try to make con-

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clusions on effect of climate change to CH₄ and CO₂ effluxes from old forests in China overall. Unfortunately, the conclusions remain speculative and this study does not contain much new information for answering that question. However, the data is valuable and interesting, and it founds a base for GHG budget estimation and modeling. Soil nitrogen content measurements raise the value of the study considerably, especially the relationships between soil mineral N and soil C fluxes are very interesting.

There are problems with the sampling design. Firstly, only three replicates of chambers per site were used, which in general is not enough. Luckily, the standard error bars in Fig. 4 does not look too bad, probably caused by the large surface area covered by the chambers (0.25m²). Secondly, only 4 samples are taken during the closure and linear function is applied. The linear function underestimates considerably and exponential function should be used (Kutchbach et al.: Co₂ flux determination by closed-chamber methods can be seriously biased by inappropriate application of linear regression, *Biogeosciences*, 4, 1005-1025, 2007). However, four points in a closure is usually not enough for using exponential fit. Instead, at least for CH₄ the underestimation could be corrected using the Intercept method (see Kroon et al.: The importance of reducing the systematic error due to non-linearity in N₂O flux measurements by static chambers, *Nutr. Cycl. Agroecosyst.*, 82, 175–186, 2008). Please note that in fact R² is not a good indicator of goodness of fit. Thirdly, the 30 minute closure time may be far too long for CO₂ measurements, as saturation of CO₂ concentration inside the chamber is usually very strong. This means that the use of linear fit causes even stronger underestimation to the efflux. Luckily, the chambers used are relatively high, reducing the saturation effect. In case you think there is no issue, could you show an example graph of the concentration development inside chamber? Fourthly, the authors do not mention when the collars were installed to the site. The collars should be installed to at least a year before the measurements, because the installation breaks roots, which may have strong influences to the soil system before the roots grow back. The effects include reduction of root and rhizosphere respiration, increase in root growth respiration, increase in dead root decomposition and reduction of soil moisture via reduction

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of transpiration. Installation of the collars could explain the high spatial variation in CO₂ effluxes 2003. For future measurements, one alternative to prior installation of the chambers is gentle installation and sealing the edges of the collar with wet sand.

The structure of the paper is good and the figures are clear. In general, language is kept simple, which helps reading. However, I strongly recommend spell checking by a native English speaker. Also the text could be compacted, especially in results and discussion. Also I think the discussion is quite thin. However, by assuming that the assumed bias in soil CO₂ efflux measurements can be resolved, I suggest that this manuscript is accepted as publication in Biogeosciences after major revision.

SPECIFIC COMMENTS

Page 7822, line 25: “. . .forests can continue to sequester carbon. . .” I assume that this refers to assumed increased respiration due to global warming? Please mention the common hypothesis why sequestration would not continue.

Page 7823, line 24: “natural conditions”. What do you mean by natural conditions? No anthropogenic influence, eg. N deposition? Or does this refer to non-manipulation measurements? Please specify.

Page 7825, lines 16-18: How long before the measurements started were the collars inserted?

Page 7825, lines 21-22: What would the times be in sun time (approximately)?

Page 7825, line 25: Are you aware that there can be severe disturbance to the soil gas efflux at the chamber installation (0-30s after the closure)?

Page 7826, line 3: Why did you choose to measure soil temperature at 10cm? The depth of the measurement affects the apparent temperature sensitivity.

Page 7827, line 8: Please specify you nonlinear regression model here.

Page 7827, lines 14-24: Could you simplify all this to: “Soil temperature and moisture

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showed clear seasonal courses (Fig. 2a), being the highest at summer and highest at winter. Soil moisture correlated strongly with soil temperature in all sites, except in subtropical forest.”

Page 7828, lines 1-2: If you simplify lines 14-24, you could add to the end “, but no trend in soil moisture from north to south”.

Page 7828, lines 4-8: This could be shortened.

Page 7828, lines 14-19: This could be shortened.

Page 7829, lines 7-10: Consider also mentioning the flux rates, as the length of growing season is different at different forests.

Page 7829, line 25-26: Please note that Q10 can be confounded eg. by changes in substrate availability (see Gu L, Post W. M. and King A. W.: Fast labile carbon turnover obscures sensitivity of heterotrophic respiration from soil to temperature: a model analysis, *Global biogeochemical cycles*, 18, GB10222, 2004 and Kirschbaum M. U. F., The temperature dependence of organic-matter decomposition-still a topic of debate, *Soil biology and Biochemistry*, 38, 2510, 2518, 2006.)

Page 7830, lines 27-28: As mentioned in discussion, could be also other way around, so that NO₃-N is released from decomposition together with CO₂. Depends probably if forest is N-limited or N-saturated.

Page 7833, lines 7-10: I don't understand this. Do you refer to CH₄ and O₂ diffusion “POTENTIALS”? Otherwise, assuming stationary conditions (small storages) and no CH₄ and O₂ production in soil, is not the CH₄ and O₂ consumption the same as the flux?

Page 7833, lines 23-24: I don't follow your reasoning. In fact, I would expect that that plants allocate resources less to roots if there is more available nutrients in the soil (see Davidson R.L.: Effects of Soil Nutrients and Moisture on Root/Shoot Ratios in *Lolium perenne* L. and *Trifolium repens* L, *Annals of Botany* 33: 571-577, 1969.). The amount

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of roots itself does not reflect allocation, because the plant biomass and the sites are different. However, ratio root:shoot or root:biomass describes allocation better. These variables are tree species dependent, and it hard to see on a study based on four very different sites.

Page 7834, lines 9-11: In page 7823 17-20 you write that addition of N to forest soil may have various effects on soil CO₂ emissions. That being the case, any findings you make are consistent with experimental findings. You may find following articles interesting: Knorr M., Frey S. D., Curtis P. S.: Nitrogen additions and litter decomposition: a meta-analysis, *Ecol.*, 86, 3252-3257. 2005 and Hyvönen R., et al.: The likely impact of elevated [CO₂], nitrogen deposition, increased temperature and management on carbon sequestration in temperate and boreal forest ecosystems: a literature review, *New Phytologist*, 173, 463-480, 2007

Page 7834, line 27-page 7835 line 5: Could you state more clearly that relationship between soil NO₃ and CH₄ flux may be not causal but purely statistical and caused by anaerobic conditions in soil? By the way, could there be causal relationship between soil water content and soil NO₃ concentration?

Page 7835, line 10: Could you say that: The sensitivity” is “The apparent sensitivity”?

Page 7835, line 20: “NH₄-N will decrease soil CH₄ uptake” is quite strong expression. Consider softening it.

TECHNICAL COMMENTS

Page 7822, line 1, "actual carbon exchanges". Where refers actual here?

Page 7823, line 9: I guess you mean to model or to predict, but not to model the prediction?

Page 7825, lines 5 and 7: Could you remove “etc.” from the species list? It is already stated that there are other species than the listed ones.

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Page 7825, lines 14-16: It is apparent that the chambers were non-transparent, but still consider mentioning it.

Page 7825, lines 18-19: It is apparent to those who are familiar with chamber measurements that the fan was installed inside the chamber, but consider mentioning it: “A fan 10 cm in diameter was installed on the top wall INSIDE each chamber to make turbulence when chamber was closed.” Also the flow-rate or power consumption of the fan could be mentioned, as the fan size itself does not tell much of its ability to create turbulence.

Page 7825, line 22: Correct 11:300 a.m. to 11:30 a.m.

Page 7826, lines 11-15: Consider removing the latter sentence and writing: “In the middle ten days of each month during GROWING SEASON, . . .”

Page 7827, line 2: Does zero here refer to base temperature of zero degrees Celcius? Replace ‘coefficients’ to ‘coefficient’.

Page 7828, line 11: Replace (2003 to 2005) with (from 2003 to 2005).

Page 7828, lines 20: what do you mean that “NO₃-N concentration tended to decrease with the time firstly. . .” Does ‘firstly’ refer to spring?

Page 7829, line 1: Carbon dioxide can be written as CO₂, as everywhere else.

Page 7829, lines 14 and 18: Replace word ‘uptake’ with word ‘sink’.

Page 7829, line 17: Replace word ‘emission’ with word ‘source’.

Page 7829, line 19: What do you mean by word ‘seasonal’ here?

Page 7829, lines 19-20: Consider replacing “forest soil showed CH₄ emission with a mean of 1.18 kg CH₄ ha⁻¹” with “forest soil was a source of CH₄ of 1.18kg CH ha⁻¹”

Page 7829, line 23: An exponential model? Eq.1? Please specify.

Page 7830, line 8: Do not say “about 7-8”, instead say about 7.5 or “7-8” or “from 7 to

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8". Specify the range similarly for tropical and subtropical forests as well (line 9).

Page 7830, line 10: "If all four forests included in the model": Please express more clearly.

Page 7830, line 12: "significant positive relationship": Statistically significant?

Page 7830, line 12-13: "Positive relationship between CH₄ flux. . ." CH₄ flux to which direction, may be unclear to some readers.

Page 7830, line 13: remove "in other forests"

Page 7830, line 14: "CH₄ variations": You mean "variation in CH₄ flux"?

Page 7830, lines 14-16: ". . .response of soil Ch₄ uptake to warming depends upon soil temperature": You mean initial temperature? Could you specify in which direction the effect is? Could you say "response to temperature" instead of "response to warming"?

Page 7831, line 2: Should be "Discussion" instead of "Discussions"?

Page 7832, lines 10-15: Could you clarify this?

Page 7832, lines 18-21: Avoid using "some studies". Could you instead write: "Additionally Xu and Qi (2002) and Rey et al. (2002) found. . .?"

Page 7832, lines 24-25: Avoid using "some studies" (see previous comment)

Page 7832, line 24 to page 7833, line 16: I am not sure whether this should be here in discussion at all.

Page 7832, lines 24 and 26: replace 'was' with 'is'

Page 7834, line 6: Replace 'microbe' with 'microbes'.

Page 7834, line 20: 'stimulated' Do you mean 'stimulates'?

Fig 2. is actually Fig. 4.

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Fig 5. There is a typo in axis “Soil moiture (%)”. Please determine soil moisture more specific, eg. Soil volumetric water content.

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