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Biogeosciences Discussions

Interactive comment on "

Temporal and spatial variations of soil carbon dioxide, methane, and nitrous oxide fluxes in a Southeast Asian tropical rainforest" *by* M. Itoh et al.

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Reply to the comments of the Anonymous Referee #2

General comments: (RC) This paper describes an interesting study of controls on greenhouse gas fluxes in a Southeast Asian rainforest. I had a number of comments but overall the paper was clear and the data seemed sound. I would suggest the authors develop some specific hypotheses to focus the paper.

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Interactive Discussion

Discussion Paper

(AC) To general comment Thank you for your kind and insightful comments on our manuscript. We agreed with most of your suggestion and revised a lot. We would be grateful if this revised manuscript could be considered for publication. ———————————

(RC) For instance, why do the authors expect soil N concentrations to affect soil CO2 efflux?

(AC) Some papers reported that soil N concentration affect the spatial variation of soil respiration. Thus we tested the correlation between N concentration and soil respiration. We revised Introduction part a lot and some environmental variables that were reported in previous studies were listed in Introduction. ———————————

(RC) The discussion lacked some of these explanations and often repeated the results section. Consequently I think the discussion can be shortened a bit and focus more on the processes and implications.

(AC) As you and the other referee suggested we revised the discussion and conclusions. We hope our revised manuscript meet your requirement. ———————————

(RC) The authors calculated WFPS but it was only mentioned in the last part of the discussion.

(AC) We agree with the referee #3's comment that WFPS is better parameter when we consider the biogeochemical reactions. However, we couldn't collect soil core samples (for measuring "theta s") at all the flux measurement points because of the restriction of exporting the soil sampling from our site. Also, soil physiological characteristic is highly heterogeneous in field. Especially for our sampling of long term, disturbance of soil structure by insects (ants or termites) can be expected. With these uncertainties, calculation of WFPS by using such parameter might make the difference to the actual condition. Therefore, we use the soil hydraulic properties to show general spatial pat-

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terns of soil physical character in our site. And we use data of VSWC measured at each chamber (3 positions for each chamber) that should exhibit the soil water condition at each chambers. However, in the discussion section (4.2.3), we needed to compare the values in WFPS unit that was reported in the paper showing the WFPS values that denitrification occurs dominantly. So we showed average (and the ranges) values of WFPS of the time when denitrification is expected. ———————————

(RC) Perhaps the description in the methods can be shortened.

(AC) We omitted some parts of the methods section as you and the other referees suggested. Thank you. ———————————

(RC) With regards to the Tables I would suggest that the authors use the more conventional significance levels of p<0.05, 0.01, and 0.001, i.e. skip the 0.005 level. Instead, I would encourage the authors to indicate p<0.1 significance as well. Sometimes the authors mention that in the text but it would be good to indicate that in the tables as well. If the level is <0.1 it means that there still is a 90% possibility that there are significant effects. You could also consider putting numbers in bold that are significant at the p<0.05 level without mentioning the actual levels and put p values <0.1 in brackets.

(AC) Thank you for your kind suggestion. In the revised manuscript, we showed the p<0.1 significance. ———————————

(RC) Page 6850, Line 1: Consider adding a reference.

(AC) We added the references. ———————————

(RC) Page 6850, Line 4: replace ‘global warming gases’ by ‘greenhouse gases’. (RC) Page 6850, Line 16-18: That sentence looks a bit contradictory to me. (RC) Page 6850, Line 19: add ‘when comparing Amazonian and SE Asian rainforests’ at the end of the sentence. (RC) Page 6850, Line 21: add ‘the’ before ‘net forest. . .’. (RC) Page 6850, Line 27: not sure if the word ‘depletion’ is the right word here. This whole sentence is a bit unclear.

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Interactive Comment

(AC) We revised these sentences. Thank you. ———————————

(RC) Page 6851, Line 25: Not sure if 'bipolar' is the right word. Also it is not explained what this means.

(AC) We revised this part. Sorry for the inconvenience. ———————————

(RC) Page 6851, Line 26: There are several studies that tried to link soil CO2 efflux to CO2 concentrations in the soil and these weren't always successful depending on the environment. I'd suggest looking at some of these. I think in general, biological processes are considered to be more important in regulating trace gas fluxes. Also, what chemical properties do you consider to be important in regulating CO2 fluxes? It may be good to expand on this section a little bit since it is a critical part of the paper and provides justification for measuring soil gas concentrations.

(AC) As you and the referee #3 suggested, we revised introduction part a lot. And we cited many previous papers on gas emissions in forest sites. ———————————

(RC) Page 6852, Line 27: What is the average seasonal/annual temperature?

(AC) We added the information on mean annual air temperature and it was stable during the sampling period. ———————————

(RC) Page 6853, Line 15-16: What was the temperature variation during this period? Were the collars always measured in the same order? If so, there could be potentially confounding factors of having certain collars being measured only when it is cool in the mornings whereas others are measured when it is warmer. It probably does not affect the analysis too much except for introducing extra variability. Perhaps the authors can see if the collars measured in the morning show consistently different values compared to those measured in the afternoon.

(AC) We have data of soil temperature at an interval of 30 min. In our site, most part of the soil was shaded by the canopy of the trees. Therefore, change in soil temperature

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during a few hours was usually small, especially in the morning. So, we think that the effect of temperature change was small. ———————————

(RC) Page 6854, Line 6-12: What was the accumulation time for CH4 and N2O? Was it also 90s similar to CO2? Later you mention that samples were taken within 30 minutes. If so, to measure all collars must have taken at least 14 hours or so. This section is a bit unclear.

(AC) Even though we collected gas samples from 0 to 30 min for CH4 and N2O, we simultaneously collected samples at most 10 chambers by shifting sampling time. Also, 4 or 5 people simultaneously did gas sampling with glass vials for CH4 and N2O measurements while the other person conducted CO2 flux measurement with IRGA at the chamber that was not used for CH4 and N2O measurements at that time. Therefore, we could collect many samples during a few hours. ———————————

(RC) Page 6855, Line 22: What do you mean by 'very close'? Be more specific.

(AC) We added the words "within 10 cm" after "very close to each chamber". ———————————

(RC) Page 6856, Line 3: Why did you measure soil pH? It didn't look like it was being used in the rest of the paper. Consider deleting this.

(AC) We consider that soil pH is important information of basic soil characteristics in our site. And we think this information will be useful for the researchers who compare the gas flux data between sites or regions. Also, this information may be useful for people who use conceptual models on gas dynamics. ———————————

(RC) Page 6856, Line 18-21: Why did you use this procedure? By moving the collars you probably introduced more variability. Of course you can't change this but it is not ideal in my mind.

(AC) Of course, we knew the risk for moving collars. At that time, we assumed that spatial variation of soil CO2 flux was controlled by difference in the amount of root

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respiration just below the flux chambers. So, we moved the collars and collected the root sample below the chamber. ———————————

(RC) Page 6857, Line 10: Replace 'obeys' by 'displays'.

(AC) Thank you for your suggestion. We shortened the methods section and omitted this sentence. ———————————

(RC) Page 6858, Line 10: Why did you calculate WFPS? It didn't seem to be used anywhere else in the paper.

(AC) Please see the response to your general comments. ———————————

(RC) Page 6858, Line 13: It would be good to add a section on how the correlations between gas fluxes and the various variables were calculated. (RC) Page 6861-6862: I would consider the present the correlation sin a separate section rather than present them within section dealing with the individual gases.

(AC) Thank you for your suggestion. We tried to add the section on the correlations, but it seemed that the change made the results more complicated. Results and Discussion sections were simplified by creating the statistical analysis section. ———————————

(RC) Page 6859, Line 10: Change title to 'Environmental conditions' or something similar.

(AC) Thank you for your suggestion. We changed the title to "Temporal variations of environmental conditions". ———————————

(RC) Page 6859, Line 22-26: I'd suggest moving this part to the methods section. (RC) Page 6860, Line 6-10: Move to the methods section. (RC) Page 6861, Line 13-14: Move to the methods section. (AC) We moved these parts to the methods section. Thank you. ———————————

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(RC) Page 6860, Line 3: Change title to ‘Spatial patterns’. (AC) We changed the title, thank you. ———————————

(RC) Page 6860, Line 13-14: This was unclear to me. (RC) Page 6860, Line 16: This does not appear to be the case for point 9 according to Fig 3e. (AC) The residual water content is the water content where a large suction change is required to remove additional water from soil. In other words, the soil can not remove the residual water even in very dry condition. The amount of residual water content is decided by such as soil particle size or soil porosity. Though the VSVC value at point 9 was as the same levels as the other points, high residual water content (low effective porosity) at point 9 indicate that smaller air-filled space (low gas diffusivity) was assumed at point 9. I hope our explanation make sense. ———————————

(RC) Page 6860, Line 21-22: Would you expect N concentrations to vary temporally? Also it appears you only measured N concentrations once so the term ‘temporally averaged’ is unnecessary.

(AC) As we wrote in methods section, we measured surface soil N concentration on all sampling days. We measured soil N concentration vertically, which was conducted once. Sorry for the inconvenience. ———————————

(RC) Page 6861, Line 5: It is rather unusual to see CO2 concentrations decrease with increasing depth. This would imply that CO2 would diffuse into the subsoil. This did not make sense to me. It may be worth checking to see if concentrations at 30 and 50 cm are really different.

(AC) Thank you for your pointing out our error. We checked our data and found our error. We corrected all parts that relate the soil gas concentrations.. ———————————

(RC) Page 6863, Line 23-25: I would be careful with this statement since the higher soil CO2 concentrations may have been caused by limited diffusion, while higher CO2

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efflux may have been caused by higher microbial activity at the soil surface. Page 6866, Line 12-13: See comment Page 6863, Line23-25.

(AC) Thank you for very important comments. As referee #1 suggested, we estimated CO2 production in soil layer by using the method of Hashimoto et al. (2007) that is similar to the method of de Jong and Schappert (1972). Although we could estimate the production rates for only two sampling points where we collected data on hydraulic properties, we admitted our mistake in interpretation of our CO2 profile data. Results of the estimation are shown in Fig. 5. As you indicated, CO2 production in surface layer (0-15cm) was much higher than deeper layer and was comparable to the CO2 emission from soil surface. This suggests that high CO2 concentrations in deeper layers were attributed by limited gas diffusion. Increase in CO2 concentrations with soil water content also supports this. Therefore, discussion section (4.1.1) was changed a lot. ———————————

(RC) Page 6864, Line 3-4: What was the P value? If the P value was around 0.1 I could buy this but if it was higher this may be a dangerous statement.

(AC) We added the P value into this sentence. Thank you. ———————————
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(RC) Page 6864, Line 6-7: Why do you make this statement? Please expand.

(AC) We rewrite this sentence as “However, hot spots probably because of termite activity measured in our site may obscure the relation between CH4 flux and environmental factors.” ———————————

(RC) Page 6864, Line 15: Replace ‘Reiner’ by ‘Reiners’. (RC) Page 6865, Line 18: I assume you mean Fig 5b.

(AC) Thank you for your pointing out our error. We correct these. ———————————
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(RC) Page 6865, Line 1-2: See comment (RC) Page 6863, Line 23-25. Page 6865,

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Line 4-8: This is a bit vague. I'm not sure what you are trying to say here. Based on Table 3 API30 is a very good predictor of N2O flux. Unless you can expand on this I would eliminate this section.

(AC) Thank you for your suggestion. We rewrite the sentence as "Our results suggest that API30 is the best parameter when predicting N2O emissions at our site, although a different number of days for the API value may be appropriate for the other site because water retention characteristics also differ." ———————————

(RC) Page 6866, Line 13-14: Not sure I understand this sentence. Perhaps rewrite this sentence to 'Spatial patterns were dominated by the presence of hotspots.' (AC) We revised the sentences. Thank you for suggestion. ———————————

(RC) Page 6868, Line 2: Can you provide a reference for this statement? (AC) We omitted this sentence. Thank you. ———————————

References de Jong E, Schappert HJV (1972) Calculation of soil respiration and activity from CO2 profiles in the soil. Soil Science, 119, 328-333.

Hashimoto, S., Tanaka, N., Kume, T., Yoshifuji, N., Hotta, N., Tanaka, K., and Suzuki, M.: Seasonality of vertically partitioned soil CO2 production in temperate and tropical forest, J For Res., 12, 209–221, 2007.

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