Biogeosciences Discuss., 7, C3506–C3510, 2010
www.biogeosciences-discuss.net/7/C3506/2010/

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.

Anonymous Referee #1

Received and published: 27 October 2010

In this manuscript the authors investigated the environmental variables regulating soil trace gas emissions (CO2, CH4, and N2O) at both temporal and spatial scales in a tropical rainforest in Peninsular Malaysia over a 2.5 year period. The topic is presented well and the experiment is carefully described. This paper builds upon results presented by Kosugi et al. (2007) of a similar topic. This study however focuses on a larger geographical area (two hectares) adjacent to the Kosugi et al. (2007) study, additionally they also investigate CH4 and N2O emission and look at how different en-

vironmental parameters relate to the trace gas emissions. This paper presents very interesting results which will contribute to a better understanding of trace gas emissions from tropical forests in Southeast Asia. With minor revision, I would deem this article suitable for submission in Biogeosciences.

Given my limited experience working with methane and nitrous oxide fluxes, I will limit my comments to the sections pertaining to CO2 analyzes.

I will begin with some more fundamental issues I have with the paper:
1. Scientific merit:
The authors need to take care when making the link between CO2 concentrations and CO2 production. CO2 concentrations are highly sensitive to changes in soil moisture but this is largely because of the effect water in the soil profile has on gas diffusion. When soils get wet, the water in the soil profile acts as a diffusion block. Although the authors do calculate effective porosity, they do not explicitly calculate CO2 production. One accepted approach worth considering is described by de Jong and Schappert (1972). In my opinion there is not a lot of value in presenting soil CO2 concentrations alone, as it does not reflect belowground carbon dynamics, beyond highlighting the importance of soils as an important CO2 storage medium.

2. Structure of the paper:
a. Although I really like the way the introduction builds up, I think the authors should finish the introduction by explicitly outlining the objectives of their study.
b. Additionally, I would to like to see the discussion and conclusions link back to the overall objectives of the paper more frequently. By relating what was found in the study back to objectives will help improve the readability and the quality of the paper substantially.
c. Some methods have been incorporated into the results section and some results have been incorporated into the discussion. This should be fixed by moving relevant text into the appropriate sections (see specific details below).
d. Data analysis section should be elaborated on.

BGD
7, C3506–C3510, 2010

3. Specific comments:
Introduction:
P.6850 L.5: Change “to be determined” with “further investigation”
P.6851 L.6-10: Rephrase and incorporate into your next sentence. As it is now these sentences do not say very much.
P.6851 L.18: Change “belowground gas production below the ground surface” with “belowground gas production”
P.6851 L.19: Delete “the” before soil water
P.6851 L.27-29: I suggest adjusting the sentence slightly: “This suggests that not only the restriction of gas diffusivity due to increasing soil water, but also a degree of biological or chemical influence must be considered.”

Materials and Methods:
P.6852 L.14: Please include elevation above sea level.
P.6852 L.24: I suggest adjusting part of the sentence to “(. . .2003) which is less than in most other regions of Peninsular Malaysia”
P.6853 L.10-14: To make this easier to read, what about saying the number of sampling visits made and in brackets put the exact dates.
P.6853 L.10: There is a slight discrepancy between the dates listed here and the dates listed on Table 2 and 4. There is also one date missing here for CO2 flux (14 September 2009).
P.6853 L.26: What was your standard gas for spanning the IRGA?
P.6855 L2-6.: Rephrase. Is it possible to state this more clearly?
P.6855 L.14-16: I suggest adjusting the sentence slightly: “For gas samples obtained between 9 Jun2 2008 and 9 March 200, the CO2 concentration was analyzed using an automated gas chromatography system detailed by Sudo (2006).
P.6855 L.25: Add the word “Here” at the beginning of the sentence: “Here, soil temperature. . .”
P.6855 L.26: Merge the two sentences: by adding “and” before soil water content. . .
P.6856 L.4: To help clarify to the reader perhaps immediately state the that pH mea-

BGD
7, C3506–C3510, 2010

surements were made on all sampling dates.
P.6856 L.6: Switch order of words: “Soil mineral” to “Mineral soil”
P.6856 L.12: add “were” before then homogenized
P.6856 L.15: I suggest adjusting the sentence slightly: “Root biomass samples were collected at four periods during the study (March, June and October 2008 and September 2009) at the 39. . .”
P.6856 L.18: Combine the two sentences accordingly: “. . .5.1 cm), while in October 2008. . .”
P.6856 L.24: Please add a sentence here to introduce the paragraph / analysis. It is not immediately clear why you are taking undisturbed soil samples.
P.6856 L.24: Is it worth making this a new section?
P.6856 L.24 to P.6858 L.10: Is it an idea to condense this section? It is very detailed and in retrospect these soil water retention data are not frequently used for your flux explanations.
P.6858 L.11: Elaborate on the statistical analysis performed. What program was used?

Results:
P.6859 L.11-14: Perhaps more relevant for the site description
P.6859 L.22-26: These are methods. Please move to the methods section.
P.6859 L.26: New paragraph for “Spatially averaged coarse root biomass. . .”
P.6859 L.26: Does this refer to root biomass in the top 5 cm or the full profile?
P.6860 L.6-10: These are methods. Please move to the methods section.
P.6860 L.24-25: Add the standard error values for the spatially averaged CO2 flux

Discussion
P.6863 L.8-11: Rephrase, to better integrate it into your study. Or move this to the introduction, as a reason for continuing with this research (i.e. building upon a previous study)
P.6863 L.23-24: Please elaborate / explain how you can suggest that there was an increase in CO2 efflux in deeper layers (see comment 1a)

BGD
7, C3506–C3510, 2010

P.6865 L. 11-14: Please rephrase or move to results, this is not discussion.
P.6863 L.16-17: I suggest adjusting the sentence slightly: "...at each chamber for the 3 measurement dates (3 March, 7 March, and 16 December, 2007) (Table 4)".
P.6864 L.2: I suggest adding the following: "...p<0.01), for each respective measurement date..."

Conclusions
P6869 L.16-27: Relate back to the objectives of the study.
P.6869 L.16-17: Remove the first sentence.

Tables:
Table 1: Why different sampling depth intervals for the three points?
Table 2: Dates differ slightly from those reported in methods section.
Table 4: delete "day" after each gas and measurement date section. Change to "10 obs. average".

Figures:
Figure 2: For soil gas concentrations measurements (CO2, CH4, and N2O) the points are often quite cluttered and it is difficult to distinguish them.
Figure 3 and 6: Make the maps slightly larger to maximize use of space. This will help the reader see the maps better
Figure 5: Why do you use API for spatially averaged fluxes and VSWC for temporally averaged fluxes?

de Jong E, Schappert HJV (1972) Calculation of soil respiration and activity from CO2 profiles in the soil. Soil Science, 119, 328-333. Kosugi Y, Mitani T, Itoh M, Noguchi S, Tani M, Matsuo N, Takanashi S, Ohkubo S, Nik AR (2007) Spatial and temporal variation in soil respiration in a Southeast Asian tropical rainforest. Agricultural and Forest Meteorology, 147, 35-47.

Interactive comment on Biogeosciences Discuss., 7, 6847, 2010.