

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 #3

Received and published: 30 October 2010

Data on trace gas emissions from tropical forests are of particular value at the moment with global attention on these ecosystems and particularly with current emphasis on forests of SE Asia. Thus the data in this paper are extremely useful and the authors should endeavour to publish them. However, there several points that the authors should consider as they revise this paper.

1. The literature on spatial and temporal variability of gas emissions and other biogeo-

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chemical processes is not well represented in the introduction. Thus, the study does not have a sound theoretical underpinning and this carries through to the discussion. We have sound and tested conceptual models of both spatial and temporal variability of soil gas fluxes. This paper should use these models to construct appropriate hypotheses and then test them. Correlation does not equal cause and effect. This point needs to be brought out in the discussion more clearly and statistical tests should be designed around known relationships.

2. There are large amounts of data presented that have no bearing on the question that the paper is addressing. For example water retention data to varying depths are never used to explain spatial or temporal variation in gas fluxes. Likewise, pH, total C and total N at depth is never used in the discussion of data. The paper needs to focus on clear presentation of data that answer questions and test hypotheses. Thus, I recommend that the authors rework the presentation of data and focus on biogeochemical processes.

3. What is the primary objective of the paper? The authors spend considerable effort to explain what they are observing in a 2 ha parcel of land without considering what could best contribute to increase biogeochemical understanding of the sources of within-site spatial variation of gas fluxes. For example, there is considerable effort expended on finding the appropriate way of parameterizing a linear model of gas flux based on antecedent rainfall. The authors conclude that an index based on rainfall over the previous 30 days (API30). This relationship is compared with one based on VSWC, not matric potential or % WFPS, which are more biologically meaningful. There is no consideration of the mechanisms that regulate each gas and as one would expect the authors find some statistically significant, but weak correlations. But what do we learn if antecedent rainfall over 30 days produces this type of a relationship. It is unlikely to be robust; if sand content were 20% higher, we would not expect the relationship to hold up. This type of relationship does not link with known sources of variation in microbial processes. What does it mean biologically if API30 correlates, but soil water

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content does not?

The authors need to be less concerned with explaining what they see on a small plot of land and more concerned with advancing understanding of mechanisms. My suggestion is to consider several factors more rigorously and based on mechanistic knowledge of the processes that are responsible for production of these gases. For example, CH₄ and CO₂ fluxes depend on soil aeration, available C, C:N ratios, diffusion rates, root density, etc. N₂O depends on available C and NO₃, aerobic status of the soil, pH, etc. None of these factors acts alone. So rather than focusing solely on the soil water relationship, the authors could explore some of these other factors, alone and in combination.

Specific comments

Abstract: The term water “condition” is ambiguous Bipolar is the wrong term. The spatial and temporal relationships were simply different.

Introduction: The introduction should develop the theoretical framework that will underpin this paper. There is much know on spatial variability of gas fluxes and biogeochemical processes. This is largely ignored. The introduction should also state the hypotheses that the authors will test in this paper.

P6850, Para 1: It is not true that SE Asian rainforests are not seasonal, even the site studied here has a distinct dry season. Likewise there are areas of Amazonia without seasonality. Most of the trace gas work in Amazonia focuses on the seasonal part of the basin because that is where land-use change is occurring.

P6850, Line 27: It is not clear what is meant by suppressing the depletion of soil CO₂ flux.

P6850, Line 28: I don't believe Davidson used irrigation in this experiment.

Methods: P6856, lines 25-28: It is a bit odd that the sampling depths vary from point to point. What is the rationale here? I would drop all but the surface sample data for this;

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the variation in the values with depth will likely be correlated with the surface variation. Plus, the surface layer is the most important for gas phase transport mechanisms.

P6857–8, eq. 1 – 4: If you have the water retention curves, it is not clear why so much of the analysis is based on VSWC. Expressing water content as matric potential or %WFPS, has much greater biological significance. If you are not going to use Eq 4, why present it?

P6858: The paper should have a section for statistical treatment of the data. I see a hotspot analysis, so I presume the data are not normally distributed, as is often the case with trace gas data. There is no analysis of normality of the data or logarithmic transformation of the data to homogenize variances. This needs to be looked at and much of the analysis is likely to need to be repeated.

Results: At several points of the presentation of results the findings in this study are contrasted with Kosugi et al. These should be moved to the discussion.

At several points (e.g. 6861, line 10; 6862, line 3; 6863, line 15; 6864, line 2) the authors find that there is no statistically significant relationship and then assert that a relationship exists. If the slope of a relationship is not significantly different from 0, then you cannot do this. There is no clear relationship in these cases. It appears that the authors are overinvested in obtaining a particular result and are ready to disregard the data and statistics to find that result.

The authors work with temporal averages in assessing flux emissions. Since this site has seasonal rainfall, you are losing information by doing this. Why not use an average annual CO₂ emissions or a total estimated CO₂ emission for the study period and account for seasonality?

Discussion: Why are alternative hypotheses not considered? The authors go to great lengths to analyze the effects of soil water on gas emissions. Why are factors like temperature, C:N ratio and total C not looked at with the same level of scrutiny? These

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factors are looked at only through correlation analyses that are not properly conceived. Most of these correlation analyses are trying to predict fast variables (gas fluxes) using slow variables (total C or N). It is not surprising that this approach does not give good results.

I do not like the analyses that include and exclude hotspots very much. It would be better to log-transform your data and run these analyses with all data. For the moment, you are just guessing that hotspots are not part of the population.

Table 1: % is not an SI unit Tables 3 – 4: Include units Figure 5: VSWC does not have units of %

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

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