

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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(RC) 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 endeavor to publish them. However, there several points that the authors should consider as they revise this paper.

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To general comment (AC) We thank you for many comments. All comments contributed to improve our manuscript. We agree with most of you and the other referee's suggestion and revised much part of the manuscript. We would be grateful if this revised manuscript could be considered for publication. Thank you very much again. —————

(RC) 1. The literature on spatial and temporal variability of gas emissions and other biogeochemical 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.

(AC) We agreed your comments for the introduction part. The referee #1 also recommended revising the introduction part. We revised introduction part a lot. As for the use of conceptual models, we think that we don't have enough data to use the models at the present time. To use conceptual models will be our future work because we think that more data with high time resolution will be needed. And we are now planning to measure these gas fluxes with the equipment that allows us continuous measurement (such as CH₄ analyzer using a tunable diode laser spectroscopy (TDLS)). —————

(RC) 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.

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(AC) We are sorry for the lack of explanation on our data and statistical tests. As the referee #1 suggested, we calculated the gas diffusion coefficient (D_s) and consider the CO_2 production rates in each depth. In the calculation of D_s , we used the data on water retention. We measured pH once to show the basic information of 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 or who use conceptual models on gas dynamics. _____

(RC) 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 content does not?

(AC) We agree with your 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, as you know, 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 uncertain-

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ties, calculation of WFPS by using such parameter might make lead the difference to the actual condition. Therefore, we use the soil hydraulic properties to show general spatial patterns 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. As for the API, we want to explain the importance of considering the history of water condition before the sampling by using this parameter. VSWC or WFPS show the condition at the sampling time. However, as you know, there must be a time lag between environmental change and change in microbial activity. Therefore, we assumed that not VSWC or WFPS but the API or something that shows the history of water condition affects the microbial activities. As you pointed out, of course, antecedent rainfall over 30 days is specific for our site and the other number of the days should be used for other sites. So we mentioned it in our manuscript "Our results suggest that API30 is the best parameter when predicting N_2O 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) 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, CH_4 and CO_2 fluxes depend on soil aeration, available C, C:N ratios, diffusion rates, root density, etc. N_2O depends on available C and NO_3 , 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.

(AC) Thank you for your critical comment. We agree with your comment that many factors affect simultaneously and we are sorry that we didn't analyze the NO_3 concentration in soil. As you pointed out, we have already tested the multiple regressions by

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selecting each 2 variables for each gas flux data. We are sorry for not mentioning this in the manuscript. And we found significant relation only between the spatial patterns of VSWC and soil N concentration and those of gas fluxes as shown in Table. 4. By these analyses, change in soil water condition (not just VSWC or WFPS but rainfall history) was found to be main control factor on temporal and spatial patterns of gas fluxes in our site where temperature change was small. _____

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

(AC) Thank you for your comment. We revised these parts. _____

(RC) 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.

(AC) We agreed your and referee #1’s comments that recommend to revise the introduction part. We revised introduction part a lot. _____

(RC) 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. _____

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

(AC) We revised this part as you and the other referee suggested. _____

(RC) P6850, Line 28: I don’t believe Davidson used irrigation in this experiment.

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(AC) Thank you for your pointing out our error. It was drought experiment and we corrected the sentence. _____

(RC) 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; 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.

(AC) This is because of the difficulty of taking undisturbed soil cores in deeper soil layer which was with many lateritic gravels. Also, at the point 1, we used the hand auger to collect the soil. Therefore, it is difficult to uniform the sampling depths. _____

(RC) 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?

(AC) As for the use of VWSC, please see the response to the major comments. 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) 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.

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(AC) As you and the other referee suggested, we made a new section on statistical analysis.

_____ Results: (RC) 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.

(AC) We moved some parts of the results section to the discussion as you and the referee #1 suggested. Thank you very much. _____

(RC) 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.

(AC) Thank you for your critical comment. For the significant level of the results, the referee #2 encouraged us to indicate $p < 0.1$ significance as well. He also said "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." We agree with your comment, however, we think that to show the data with $p < 0.1$ significance (of course we must clearly stipulate the significant levels) will be good indication for the readers. We ask for your kind understanding.

_____ (RC) 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?

(AC) We also think that we should estimate the average annual CO₂ emission; however, we only have the data of one to three sampling times per year at the present

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time. We might be able to estimate the annual CO₂ emission with these data, but it must cause a significant error. Now, we are trying to collect the soil gas flux data continuously. We think that the estimation of annual emission is our future issue.

_____ Discussion: (RC) 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 factors are looked at only through correlation analyses that are not properly conceived. (RC) 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. (RC) It would be better to log-transform your data and run these analyses with all data.

(AC) We are sorry for the lack of explanation on our data and statistical tests. We had conducted the single and binary regression analysis for all the parameters that we measured. Also, we conducted the regression analyses with log transform data of N₂O emission. However, no significant relationship was found other than the data that we showed in our manuscript. We added the information of variation of temperature in the manuscript. Diurnal or annual variation of temperature was small during the observation period. _____

(RC) For the moment, you are just guessing that hotspots are not part of the population. (RC) I do not like the analyses that include and exclude hotspots very much. (AC) In our site where many termites and ants are alive, outlier emission of CO₂ or CH₄ is observed with some frequency. In this paper, we showed our data by two ways that including and excluding the hotspots. By doing this, we may show some relationships between environmental variable and gas emission that is hidden by the existence of hotspots. Of course we stipulated that whether or not hotspots are included, and by doing so, it would be convenient for the readers to make new hypothesis in future work.

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(RC) Table 1: % is not an SI unit (RC) Figure 5: VSWC does not have units of % (AC)
Thank you very much. We revised these parts.

(RC) Tables 3 – 4: Include units (AC) These tables show the person correlation coefficient, therefore, we think that we don't need to show the unit.

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