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Interactive comment on "Effects of nitrogen and phosphorus additions on nitrous oxide emission in a nitrogen-rich and two nitrogen-limited tropical forests" *by* M. H. Zheng et al.

Anonymous Referee #5

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General comments

This study presents a field experiment studying the effects of nitrogen and phosphorus additions on soil N2O emissions from nutrient rich and nutrient poor tropical forests. In general, the paper is well written and is highly relevant, and it provides valuable new information about the combined and individual effects of N and P fertilization on soil N2O emissions. Based on three referees and the response of the authors to them, the authors have already addressed several issues related to e.g. the high N fertilization rates, effects of P on alleviation of N2O emissions, which has greatly improved the quality of the paper. However, I have few additional comments that are mainly related to the gas analysis of N2O, presentation and interpretation of the results. I consider

this work important and worth publishing after addressing the points below.

Specific comments

Page 2, lines 24-25: Could you explain how and in what respect the tropical forests have shown an increase in soil N2O emissions compared to temperate and boreal forest soils? Is this due to increased atmospheric N deposition, or do you refer merely to a pure comparison of the N2O emission rates from these ecosystems?

Page 3, lines 2-5: I consider that in addition to the mentioned factors, the poor knowledge in factors controlling N2O emissions in tropical forests is also due to the rather small number of studies from these ecosystems.

Page 3, lines 11-13: I would mention here also other losses of N, such as leaching losses, emissions of N2, NO, NH3, and HONO, which all are signs of N saturation.

Page 5, lines 18-20: I would like to see here more description of where the N is retained (soil, above ground biomass, below ground biomass, microbial biomass), and in what forms are the N losses from the soil (leaching, gaseous losses, what gas species etc).

Page 7, lines 5-7: could you give more details of the gas chromatographic analysis. I'm missing information of the used columns, oven temperature, flow rates, carrier and make-up gases. Especially, I'm interested and slightly concerned whether CO2 was allowed to enter the ECD, or whether it was trapped chemically (e.g. ascarite) as if N2 is used as a carrier gas, and CO2 is allowed to enter the ECD, this may bias the N2O analysis and lead to overestimated N2O fluxes as described by Zheng et al. (2008).

Zheng X., et al., 2008. Quantification of N2O fluxes from soil-plant systems may be biased by the applied gas chromatograph methodology. Plant and Soil, 311: 211-234.

Page 8, lines 13-19: I'm missing information whether you tested the data for normality and equality of variances. Naturally, if these criteria were met, the use of parametric tests are justified, otherwise non-parametric tests should be used. Please, clarify this.

Page 12, lines 13-14: This line is almost identical to the sentence from page 8, lines 23-24. Please, modify.

Page 12, lines 16-18: was the difference in mean soil temperature statistically significant between the three forests? If yes, please give the p-value. Also, were the N2O emission rates across different forests significantly different? If yes, please give the p-value here. In other words, if the above mentioned differences were not statistically significant, you cannot claim that soil temperature does not explain the N2O emission pattern across the forests.

Page 12-13, chapter 4.2: You present simple correlation analysis of N2O emissions against soil temperature or soil moisture, and use robust linear regression to explain the N2O emissions (Fig. 5). Based on the scatter plots, it seems that there is an exponential relationship between at least N2O fluxes and soil temperature. Did you try to fit also non-linear models to the data? Also, as the correlation between both N2O flux and soil temperature, and N2O flux and soil moisture are highly significant, did you try to build a regression model including both soil temperature and soil moisture as parameters? This might be worth the effort.

Page 13, lines 1-7: Based on only two soil sampling occasions (Feb 2007 and Aug 2009) it is very uncertain to conclude how the soil inorganic N concentrations developed during the different seasons. For instance, a soil sampling in February 2007 does not support that the soil was enriched with inorganic N, and also a soil sampling in August 2009 does not support that the inorganic N had decreased during the growing season, as there were no measurements during the growing season. Please, discuss these uncertainties, and if possible bring in material and references to support your conclusions.

Table 2 and e.g. page 16, lines 18-20: The values in soil pH, inorganic N, organic C, microbial biomass and P in Table 2 are only from one sampling occasion, approximately two years from the start of the experiment. Also, the comparison between the

fertilization treatments is conducted with data from one time sampling only, while the fertilization was conducted every second week over a two-year period. I see here a problem when comparing the effects of the fertilization. Firstly, I think it would be best to compare the soil N (and other measures) status before and after the treatments. But in this comparison, the timing of the sampling is important as the soil N (and other) have strong seasonality, which may be larger than the treatment effect. As the soil sampling before the experiment was in the spring (February 2007), and the soil sampling after the experiment was during summer (August 2009), it is very difficult to know whether the differences result from the treatments or the seasonal variation in soil N. My other concern is that the different plots may have differed between each other already before the experiment. Did you test this? Overall, I think it is very difficult to conclude that the fertilization did or did not influence the soil N status in the experiment. Please, discuss these uncertainties or be more careful in interpreting the results, unless there is more data to support these findings.

Fig. 5: Is this data from the control plots only? Please, specify which data was used.

Technical corrections

Page 2, line 12: add “atmospheric lifetime” inside the parenthesis. Page 4, line 15, and line 19: change a N-rich to “an N-rich” Page 6, line 7: I assume that you mean wet N deposition. If so, please add the word “wet” to the “Inorganic N deposition…”. Or if this is a sum of wet and dry deposition, please clarify it. Page 8, line 25; page 9, line 9; page 10, line 2 and elsewhere in the paper: I would harmonize the use of decimal places, preferably round them to one decimal place. At least with N2O fluxes, I don’t think the precision of the measurement is high enough to give the emissions with the accuracy of two decimal places. Page 12, line 22: add “WFPS” and “the” to the sentence: “highest WFPS in the old-growth forest and the lowest in the pine forest” Tables 1 and 2. Please, give the numbers with one decimal place.

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