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Biogeosciences
Discussions

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.

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Dear referee #5,

We appreciate for your comments and suggestions which help us further improve the quality of this manuscript. Generally, we agreed with your comments and have followed your suggestions. Below are our point-by-point responses. All the revised portions are marked in red in the revised manuscript, and the page and line numbers of the revised manuscript are also provided. Thank you very much.

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.

Answer: Thank you very much for these positive comments.

Specific comments

1) 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?

Answer: Thank you for this good suggestion. The reviewer suggested that we need to explain how and in what respect the tropical forests have shown an increase in soil N2O emissions compared to temperate and boreal forest soils, and also pointed out whether this may be due to increased atmospheric N deposition? −We agreed with this suggestion and have added this information in the text. Specifically, we have replaced this sentence: "Compared with temperate and boreal forests, tropical forests have shown a great increase in soil N2O emissions (Matson and Vitousek, 1990)" with "Because tropical forest soils are often rich in N but poor in P, they are less able to retain external N input (Hall and Matson, 1999). With the greatest increases of atmospheric N deposition occurred in tropical regions (Galloway et al., 2008), tropical forests have shown a great increase in soil N2O emissions, compared with temperate and boreal forests (Matson and Vitousek, 1990)." (Please also see Page 2 Line 25 and Page 3

Lines 1-4 in the revised manuscript)

Reference:

Galloway, J. N., Townsend, A. R., Erisman, J. W., Bekunda, M., Cai, Z., Freney, J. R., Martinelli, L. A., Seitzinger, S. P., and Sutton, M. A.: Transformation of the nitrogen cycle: recent trends, questions, and potential solutions, Science, 320, 889-892, 2008.

Hall, S. J., and Matson, P. A.: Nitrogen oxide emissions after nitrogen additions in tropical forests, Nature, 400, 152-155, 1999.

Matson, P. A., and Vitousek, P. M.: Ecosystem approach to a global nitrous oxide budget, Bioscience, 40, 667-671, 1990.

2) 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.

Answer: Thank you, and we agreed with your comment. Accordingly, we have added this information in the text: "This is not only because…, but also because only a small number of studies in tropical forests is available (Dalal and Allen, 2008; Liu and Greaver, 2009)." (Please also see Page 3 Lines 8-9 in the revised manuscript)

Reference:

Dalal, R. C., and Allen, D. E.: Greenhouse gas fluxes from natural ecosystems, Australian Journal of Botany, 56, 369-407, 2008.

Liu, L., and Greaver, T. L.: A review of nitrogen enrichment effects on three biogenic GHGs: the CO2 sink may be largely offset by stimulated N2O and CH4 emission, Ecol. Lett., 12, 1103-1117, 2009.

3) 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.

Answer: Thank you, and we agreed with this comment. Following your suggestion, we have replaced this sentence: "….leading to rapid N losses via N2O emission." with "….leading to rapid N losses via liquid leaching and gases emission (such as N2, N2O, NO, NH3, and HONO)." (Please also see Page 3 Lines 16-17 in the revised manuscript)

4) 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).

Answer: Thank you for these good suggestions.

First, the reviewer suggested that we need to add more description of where the N is retained (soil, above ground biomass, below ground biomass, microbial biomass). −−We have followed this suggestion and replaced "….net retention of 22−28 kg N ha-1yr-1 in the two younger forests…." with "….22−28 kg N ha-1yr-1 were retained in the upper 20cm soil and the plant biomass (including canopy trees, understory plants and forest litter) in the two younger forests," (Please also see Page 6 Lines 6-7 in the revised manuscript)

Second, the reviewer also suggested that we need to add the forms of N losses from the soil (leaching, gaseous losses, what gas species etc). −We have also followed this suggestion and replaced "….net loss of 8−16 kg N ha-1yr-1 from the soil in the old-growth forest (Fang et al., 2008)." with "and that a net loss of 8−16 kg N ha-1yr-1 mainly via dissolve inorganic N (NH4+ and NO3-) leaching and soil N2O emission occurred in the old-growth forest (Fang et al., 2008)." (Please also see Page 6 Lines 7-9 in the revised manuscript)

Reference:

Fang, Y. T., Gundersen, P., Mo, J. M., and Zhu, W. X.: Input and output of dissolved organic and inorganic nitrogen in subtropical forests of South China under high air pollution, Biogeosciences, 5, 339-352, 2008.

5) 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 CO_2 was allowed to enter the ECD, or whether it was trapped chemically (e.g. ascarite) as if N_2 is used as a carrier gas, and CO_2 is allowed to enter the ECD, this may bias the N_2O analysis and lead to overestimated N_2O fluxes as described by Zheng et al. (2008). Zheng X., et al., 2008. Quantification of N_2O fluxes from soil-plant systems may be biased by the applied gas chromatograph methodology. Plant and Soil, 311: 211-234.

Answer: Thank you for this good suggestion. Following your suggestion, we have added this information in the text: "Two stainless steel columns (pre-column and main-column was 1m and 3m in length, respectively) packed with Porapak Q were used to separate N_2O. The oven temperature and ECD temperature was 55 °C and 330 °C, respectively. To avoid the interference of CO_2 from the gas samples which can lead to overestimation of N_2O fluxes as suggested by Zheng et al. (2008), we used N_2 as the carrier gas (flow rate of 35mL min^{-1}) and introduced 10% of CO_2 in N_2 as the make-up gas (flow rate of 2mL min^{-1}) into the ECD (Wang et al., 2010). Through introducing high concentration and low flow rate of CO_2 into the ECD, the interference of CO_2 from the gas samples is negligible (Wang et al., 2010)." (Please also see Page 8 Lines 7-13 in the revised manuscript)

Reference:

Wang, Y., Wang, Y., and Ling, H.: A new carrier gas type for accurate measurement of N_2O by GC-ECD, Adv. Atmos. Sci., 27, 1322-1330, 2010.

Zheng, X., Mei, B., Wang, Y., Xie, B., Wang, Y., Dong, H., Xu, H., Chen, G., Cai, Z., and Yue, J.: Quantification of N_2O fluxes from soil–plant systems may be biased by the applied gas chromatograph methodology, Plant Soil, 311, 211-234, 2008.

6) 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.

Answer: Thank you for this comment. In fact, the data in our study have been tested for normality using the Kolmogorov-Smirnov test and for equality of variance using Levene's test. Those data that did not meet the requirements of normality and equality of variance have been log-transformed before statistical analysis. Following your suggestion, we have added this information in the text: "Data were tested for normality (Kolmogorov-Smirnov test) and equality (Levene's test) of variances, and were log-transformed for analysis if they did not meet the requirements of normality or equality of variances." (Please also see Page 10 Lines 4-6 in the revised manuscript)

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

Answer: Thank you for this careful review, and we agreed with this comment. Accordingly, we have modify this sentence "Soil temperature in all plots in the three forests showed a similar seasonal pattern, increasing from spring to summer and decreasing from fall to winter (Fig. 1)" to "Overall, soil temperature increased from spring to summer but decreased from fall to winter in all the forest plots (Fig. 1)." without changing its initial meaning. (Please also see Page 14 Lines 7-8 in the revised manuscript)

8) 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 N_2O 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 N_2O emission pattern across the forests.

Answer: Thank you, and we have followed your suggestions.

First, the reviewer pointed out whether the difference in mean soil temperature was statistically significant between the three forests? −− Yes. Repeated measures ANOVA showed the significant differences ($P < 0.001$) in mean soil temperatures between each forest (as we have mentioned in the Result section, Page10 Lines 15-16). Thus, to make it clear to the readers, we have added "(statistical difference of $P < 0.001$ between each forest)" here (Please also see Page 14 Line 11 in the revised manuscript).

Second, the reviewer pointed out whether the difference in the N2O emission rates was significantly different across different forests? −− Yes. Mean N2O emission rate was significantly higher in the old-growth forest than in the mixed ($P = 0.001$) and pine ($P = 0.005$) forests (as we have mentioned in the Result section, Page11 Lines 17-19). To make it clear to the readers, we have also added "(with being significantly higher in the old-growth forest than in the mixed ($P = 0.001$) and pine ($P = 0.005$) forests; Fig. 4)." here (Please also see Page 14 Lines 12-13 in the revised manuscript).

Third, the reviewer pointed out if the above mentioned differences were not statistically significant, it is incorrect to claim that soil temperature does not explain the N2O emission pattern across the forests. −− Thank you for this comment. Both mean soil temperature and mean N2O emission rates are statistically significant across forests (as we mentioned above), and thus, this suggests "a limited ability of soil temperature to explain the pattern in N2O emission across forests". (Please also see Page 14 Lines 13-14 in the revised manuscript).

9) 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.

Answer: Thank you for these comments and suggestions.

First, the reviewer pointed out that an exponential relationship may occur at least between N2O fluxes and soil temperature, based on the scatter plots, and asked whether we have tried to fit also non-linear models to the data? −− Thank you for this comment. In fact, we have used some suitable exponential regression models to build the relationships between N2O flux and soil WFPS, or between N2O flux and soil temperature, but the coefficients of determination (R2) of the exponential regression models were lower than those of the linear regression model used in this study. For example, we chose some suitable exponential regression models (i.e. y=aïĆťexp(bïĆťx), y=aïĆťexp(x/b)+c, y=aïĆťexp(xïĆťb)+c), and the R2 of the models are 0.102−0.110 between N2O and soil WFPS, 0.166−0.167 between N2O and soil temperature in the old-growth forest; 0.176−0.180 between N2O and soil WFPS, and 0.098−0.099 between N2O and soil temperature in the mixed forest; 0.225−0.226 between N2O and soil WFPS, and 0.071−0.082 between N2O and soil temperature in the pine forest. All these coefficients were lower than those of the linear regression model used in this study (Please see Table 4). In addition, some other non-linear models (such as power regression models) were also tried, but the coefficients of determination were also lower than those of the linear regression regression. Therefore, based on above reasons, we used the linear regression model in this study.

Second, the reviewer suggested a regression model including both soil temperature and soil moisture as parameters.−− Thank you for this good suggestion, and we have followed it. Because liner regression model had the better fitting effect in our study as we mentioned above, we added the liner regression models including both soil temperature and soil moisture as parameters in "Table 4". Based on this added model, we have also added more information in the text: "In the control plots, soil temperature and WFPS showed a significant positive linear relationship with soil N2O emission (Fig. 5), and explained 9−17% and 12−23% of N2O fluxes variation across the forests (Table 4). The models that included soil temperature and WFPS as parameters showed the higher R^2 values (22−28%; Table 4)" (Please also see Page 11 Lines 9-12 in the revised manuscript), and also added "Compared to the models with soil temperature and N2O fluxes as parameters, the R2 values of the models with soil WFPS and N2O fluxes as parameters were not much higher (Table 4). However, mean soil WFPS showed comparable dynamics to mean N2O emission, with the highest in the old-growth forest and lowest in the pine forest (Fig. 2)" in the Discussion section (Please also see Page 14 Lines 16-19 in the revised manuscript). The added information above helps us further improve the manuscript.

10) 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.

Answer: Thank you very much for these constructive comments and suggestions, and we agreed with your comments. Although seasonal variances of soil inorganic N concentrations were not measured in this study, they were measured by our previous study in the same forests (Mo et al., 2003). Using ion exchange resin method, our previous study found that soil inorganic N concentrations (NH4+ plus NO3-) showed significant seasonal variations in the three forests, with the following order: spring (total mean value: 47.64 ± 14.67 μg per day g-1 dry resin) > fall (23.51 ± 2.30 μg per day g-1 dry resin) > winter (18.76 ± 2.06 μg per day g-1 dry resin) > summer (16.81 ± 3.29 μg per day g-1 dry resin) (Mo et al., 2003). Thus, this pattern supported our discussion in the text: "In spring, forest soil was enriched with inorganic N. . .." (Page 14 Lines 22-23 in the revised manuscript) and "In fall and winter, both the lower soil inorganic N (decreased after growing seasons). . .." (Page 15 Lines 3-4 in the revised manuscript).

Accordingly, following your suggestion, we have added this reference (Mo et al., 2003) to support our conclusion in the text: "In spring, forest soil was enriched with inorganic N. . .. (Mo et al., 2003)" (Please also see Page 14 Lines 22-24 in the revised manuscript) and "In fall and winter, both the lower soil inorganic N (decreased after growing seasons) (Mo et al., 2003)" (Please also see Page 15 Lines 3-4 in the revised manuscript).

Reference:

Mo, J. M., Brown, S., Peng, S. L., and Kong, G. H.: Nitrogen availability in disturbed, rehabilitated and mature forests of tropical China, For. Ecol. Manage., 175, 573-583, 2003.

11) 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.

Answer: Thank you very much for these constructive comments, and we would like to response to these comments point by point below.

First, the reviewer suggested that it would be best to compare the soil N (and other measures) status before and after the treatments, and that the timing of the sampling is also important as the soil N (and other) have strong seasonality, which may be larger than the treatment effect. −− Thank you for this suggestion. Firstly, it is a good method to compare the soil variables before and after the treatments, as suggested by the reviewer, but this method may not be suitable for our present study using long-term and on-going fertilization treatments, because it may be a little difficult to evaluate the difference caused by treatments or seasonality if we compared the results from different sampling periods after the treatments with those from before the treatments (Please note that we only measured soil properties once in February 2007 before fertilization). However, in our study, we have set up the control plots in all the three forests, which allow us to know about the treatment effects by the comparison between the fertilization plots and the control plots in the same sampling period. This method of studying treatment effects by setting up control plots has also been widely used in many forest studies (Treseder et al., 2001; Cleveland and Townsend et al., 2003; Hall and Matson, 2003; Davidson et al., 2008; Koehler et al, 2009; etc). Accordingly, we hope that our method of comparing the treatment plots with those in the control plots is also feasible. Secondly, we agreed that the timing of the sampling is also important because the soil variables may have seasonality, as suggested by the reviewer. For this reason, we now have showed all the soil properties values measured during our study period (in August 2007, February 2008, August 2008, February 2009, and August 2009), rather than one sampling occasion (Please also see Table 2 in the revised manuscript, and Table S2-S4 in the supporting information). We measured soil properties in February and August, mainly because (1) February and August is within the dry and wet season, respectively, in our study region, and (2) our study region had typical seasonal pattern, with the wettest and warmest during wet season and the driest and coldest during dry season. (Please also see Page 6 Lines 18-22 in the revised manuscript).

Second, the reviewer pointed out that the soil sampling before the experiment was in the spring (February 2007), and the soil sampling after the experiment was during summer (August 2009), so it is very difficult to know whether the differences result from the treatments or the seasonal variation in soil N. −−Thank you very much for this constructive comment. Firstly, we now have showed all the soil sampling data (August 2007, February 2008, August 2008, February 2009, and August 2009) rather than one time of soil sampling data (August 2009). Secondly, we now have analyzed soil variables using the method of repeated measures ANOVA, and this statistical analyses method could evaluate the treatment differences based on different sampling periods. (Please also see Table 2 in the revised manuscript, and Table S2-S4 in the supporting information). Therefore, we hope that the above improvements could help us evaluate the treatment differences properly.

Third, the reviewer asked whether the different plots may have differed between each other already before the experiment. −−Thank you for this constructive comment. In fact, we have measured the soil properties in all the plots before the treatments, and we found no statistical difference of soil properties among the plots in each forest (Please also see Table S1 in the supporting information).

Fourth, the reviewer pointed out that it is difficult to conclude that the fertilization did or did not influence the soil N status in the experiment, and suggested us to discuss these uncertainties or be more careful in interpreting the results, unless there is more data to support these findings. −−Thank you very much for this comment. We have added more soil properties data to support our findings (Table 2, Table S2, Table S3 and Table S4) and used the repeated measures ANOVA to analyze the treatment effects rather than one-way ANOVA, which could rule out the interference of sampling times. Thus, we hope that these improvements will allow us to draw the conclusions more credibly.

Fifth, because we added more data of soil properties in the revised manuscript, we have made the revision on the description of the Results section (3.3 soil properties) from "Soil pH did not change after addition of fertilizers in the old-growth and pine forests, but significantly decreased after NP-addition in the mixed forest (Table 2). Soil NH4+ concentrations significantly increased after P- and NP-addition in the old-growth forest, while NP-addition significantly decreased soil NO3- and NH4+ concentrations in the old-growth and pine forests, respectively. N-addition significantly decreased soil total inorganic N (NH4+ + NO3-) concentrations in the pine forest. No treatment effect occurred on soil organic C in the old-growth and pine forests, while both P- and NP-addition significantly increased soil organic C in the mixed forest. Soil microbial biomass C significantly increased after NP-addition in the old-growth forest and after N-, P- and NP-addition in the mixed forest. Although not always statistically significant, both P- and NP-addition increased soil available P concentrations in all the forests compared to the control plots" to "Repeated measures ANOVA showed that soil pH significantly increased after P-addition in the old-growth forest (Table 2). Soil NO3- concentrations significantly decreased after P-addition in the old-growth and mixed forests, and significantly increased after N-addition in the pine forest. Soil NH4+ concentrations and total inorganic N (NH4+ + NO3-) concentrations had no response to either N- or P-addition in any forest. Soil available P concentrations significantly increased after P-addition in all the forests. Soil organic C significantly increased after N-addition in the mixed and pine forests, but not in the old-growth forest. Soil microbial biomass C significantly increased after P-addition in the old-growth forest and after N-addition in the mixed forest. Interaction of combined N and P additions occurred in soil AP concentrations and microbial biomass C in the old-growth forest, and in soil pH and NO3- concentrations in the mixed forest" (Please also see Page 11 Lines 5-13 in the revised manuscript).

Although we added more data of soil properties, those results of soil properties supporting our findings did not change, that is (1) "In the old-growth forest, we found no increase in soil organic C, microbial biomass C (Table 2),. . ." (Please also see Page 15 Lines 19-20 in the revised manuscript); (2) "As a result, no significant increase in soil inorganic N (NH4+ and NO3-) was observed after N addition in the old-growth forest (Table 2)." (Please also see Page 15 Line 25 and Page 16 Lines 1-2 in the revised

manuscript); (3) "despite no significant increase in soil total inorganic N following N addition, a significant increase in soil microbial biomass C and soil organic C was observed in the mixed forest, as well as a significant increase in soil organic C in the pine forest (Table 2)." (Please also see Page 16 Lines 7-9 in the revised manuscript); (4) "NP addition did not significantly affect soil total inorganic N (NH4+ plus NO3-) (Table S2)," (Please also see Page 19 Lines 1-2 in the revised manuscript). However, only the sentence of "However, we found no significant change in soil total inorganic N (NH4+ plus NO3-) after approximately 2 years of P addition in all forests, despite a significant increase in NH4+ in the old-growth forest (Table 2)." should be replaced with "However, we found no significant change in soil total inorganic N (NH4+ plus NO3-) after P addition in all forests, despite a significant decrease in NO3- in the old-growth and mixed forests (Table 2)." (Please also see Page 17 Lines 16-18 in the revised manuscript), but the revision of this sentence did not affect our main finding in the Discussion.

Finally, we appreciated the reviewer for the above comments and suggestions.

Reference:

Cleveland, C. C., and Townsend, A. R.: Nutrient additions to a tropical rain forest drive substantial soil carbon dioxide losses to the atmosphere, P. Natl. Acad. Sci. USA, 103, 10316-10321, 2006.

Davidson, E. A., Nepstad, D. C., Ishida, F. Y., and Brando, P. M.: Effects of an experimental drought and recovery on soil emissions of carbon dioxide, methane, nitrous oxide, and nitric oxide in a moist tropical forest, Global Change Biology, 14, 2582-2590, 2008.

Hall, S. J., and Matson, P. A.: Nutrient status of tropical rain forests influences soil N dynamics after N additions, Ecol. Monogr., 73, 107-129, 2003.

Koehler, B., Corre, M. D., Veldkamp, E., Wullaert, H., and Wright, S. J.: Immediate and long-term nitrogen oxide emissions from tropical forest soils exposed to elevated nitrogen input, Global Change Biol., 15, 2049-2066, 2009.

Treseder, K. K., and Vitousek, P. M.: Effects of soil nutrient availability on investment in acquisition of N and P in Hawaiian rain forests, Ecology, 82, 946-954, 2001.

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

Answer: Yes. The data were from the control plots. To make it clear to the readers, we have added "....in five control plots of the study forests" in the figure legend. (Please also see the legend of Fig. 5)

Technical corrections

13) Page 2, line 12: add "atmospheric lifetime" inside the parenthesis.

Answer: Thank you for this suggestion, and we have added "atmospheric lifetime" inside the parenthesis. (Please also see Page 2 Line 13 in the revised manuscript)

14) Page 4, line 15, and line 19: change a N-rich to "an N-rich"

Answer: Thank you for this careful review. Following your suggestion, we have replaced "a N-rich" to "an N-rich". (Please also see Page 4 Line 21 and Page 5 Line 7 in the revised manuscript)

15) 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.

Answer: Thank you for this suggestion. We mean wet N deposition in this sentence, so we have added "wet" to the "Inorganic N deposition..." in the text. (Please also see Page 6 Line 22 in the revised manuscript)

16) 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.

Answer: Thank you for this suggestion. Following your suggestion, we have harmonized the use of decimal places, from two decimal places to one decimal place, throughout the text. (Please also see Page 2 Lines 2-3; Page 10 Line 14, 24; Page 11 Line 17; Page 12 Lines 1-7; Table 1 and Table 2 in the revised manuscript)

17) 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"

Answer: Thank you, and we have followed this suggestion to add "WFPS" and "the" to the sentence, and this sentence is now "the highest WFPS in the old-growth forest and the lowest WFPS in the pine forest". (Please also see Page 14 Line 18-19 in the revised manuscript)

18) Tables 1 and 2. Please, give the numbers with one decimal place.

Answer: Thank you. We have followed this suggestion to give the numbers with one decimal place in the two tables. (Please also see "Table 1" and "Table 2" in the revised manuscript)

Again, we appreciate the reviewer for the constructive comments and suggestions.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-552/bg-2015-552-AC4-supplement.zip

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