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Discussions

Interactive comment on "Soil nitrogen oxide fluxes from lowland forests converted smallholder rubber and oil palm plantations in Sumatra, Indonesia" *by* Evelyn Hassler et al.

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We thank Dr. Yit Arn Teh for the time he invested to give his thoughtful and constructive comments. For clarity, we have copied his comments and placed our answers below each comment.

GENERAL COMMENTS

Referee: While I am strongly supportive of this work overall, I do have a few concerns. First, I believe that the authors need to reconsider the structure of the Methods and Results sections to improve the clarity of the text. For the Methods section, I was sometimes confused as to which ecosystems/land-use were sampled at what times,

and I think the authors should revise the sections describing the experimental design to better clarify the chronology of the measurements. From my reading of the text, it appears that there were 2 parts to this study; the first phase, where gas fluxes were compared among forest, jungle rubber, and small holder plantations. During the second phase, fluxes were compared among small holder and large holder plantations. It would be useful if the text could be edited to make this sampling design a bit clearer.

Answer: We addressed this concern by introducing earlier on (in the abstract, in section 1 "Introduction", 2.1 "Study area and experimental design" and in section 2.2 "Soil N-oxide fluxes and supporting soil factors") the study coverage, as suggested by Dr. Teh. We now stated in the revised manuscript that there are two parts of the study; the first was on quantifying soil N-oxide fluxes from the four different land-uses (page 6, lines 24-25; page 8, line 22; page 9, line 10), and the second, as a follow-on study, was on the comparison between smallholder and a large-scale oil palm plantations in the loam Acrisol soil (page 2, lines 10-12; page 5, lines 16-19, page 7, lines 9-12; page 9, lines 5-9).

Referee: In addition, measurements were discussed in the Results and Discussion which were not described in the Methods – for example, potential nitrification measurements were performed, but not described in the Methods. By inference, I had assumed that potential denitrification measurements had been conducted too, as the authors later conclude on Page 18, section 3.4 that nitrification was the dominant N-oxide producing process (which implies that other pathways such as denitrification or DNRA were not closely correlated with N-oxide fluxes). I had wondered if these potential nitrification measurements had been conducted as part of another study; if so, then this needs to be acknowledged.

Answer: We mentioned in section 2.3 "Statistical analysis" (page 14, lines 13-15) that we assessed the spatial control of soil biochemical characteristics on annual soil N2O fluxes, using the soil biochemical data reported in Appendix Table A2; in this table we reported the source of these data. To improve clarity in our present manuscript, we now mentioned in section 2.2 “Soil N-oxide fluxes and supporting soil factors” (page 13, lines 4-8) and section 3.4 “Spatial controls of annual soil N2O fluxes” (page 18, lines 20-22) the source of these soil biochemical data. We mentioned that these data were reported earlier by Allen et al. (2015) and in our present manuscript we only put in Appendix Table A2 the parameters that showed significant relationships with the annual soil N2O fluxes. Furthermore, the entire internal soil-N cycling was quantified in situ (except for denitrification) by Allen et al. (2015), and we used all the parameters of the soil-N cycling to correlate with the annual soil N2O fluxes. Only the gross nitrification rates showed significantly correlation with annual N2O fluxes from the reference land uses across the two landscapes. We did not interpret this correlation as the responsible process for N2O emission/production in the soil. Instead, we interpreted this as the control of soil N availability on soil N2O emission/production and gross nitrification as an index of soil N availability. Quantifying the relative importance of nitrification and denitrification on soil N2O fluxes from these land uses (which cannot be drawn from our data via mere correlation test) is the focus of a follow-on study by our group during the 2nd phase of this project, which has just started this year, 2017.

Referee: Second, I thought that the structure of the Results section could be improved. I felt that the way in which the Results were organised did not convey information clearly about how fluxes varied among land-uses and soil types. In my opinion, I think it would be clearer if the first part of the Results compared trends among land-uses (e.g. forest, jungle rubber, small holders; small holders versus large holders, etc.). The authors could then go on to explore differences among soil types. The second part of the results section could discuss temporal trends in N-oxide fluxes, such as intraannual trends in N-oxide fluxes (if any exist) as well as the pattern in N-oxide fluxes after fertilisation. The last part of the Results could discuss the role of environmental variables and N cycling processes (e.g. nitrification) in regulating flux rates. This could all be achieved without altering the text too much, but simply re-organising how the information is presented.

Answer: Although we understand why the reviewer is suggesting this sequence of flow in the Results, we ask for Dr. Teh’s consideration of the basis of our decision on how we had organized the present structure of our results. The main reason of organizing the results this way (N-oxide fluxes 1st from the reference land uses with comparison between the two landscapes and followed by the converted land uses within each landscape, including the smallholder and large-scale oil palm plantations within one landscape, then fertilization effects as the most important management in oil palm plantations, and finally the temporal and spatial controls) is because we need first to establish if from the reference land uses (with no to minimal human disturbance) there are differences between the two landscapes, as baseline data for soil N-oxide fluxes, before going onto the land-use change effect and further onto the controlling factors. This flow of the result presentation also supports the sequence of logic in the discussion section:

a) how our measured fluxes from the baseline reference land uses are comparing with the other findings in the tropics and, with that, establishing how the soil factors control the temporal and spatial patterns of soil N-oxide fluxes.

b) how land-use conversion affected these fluxes and changed the controlling factors, and hence why significant change in N-oxide fluxes was not detected among land uses.

c) finally, the effects of fertilization, and that its importance for improved estimates of annual N-oxide fluxes at a scale larger than our present study lies on the inclusion of large-scale, more intensively fertilized oil palm plantations.

Referee: I had no major concerns about the Introduction and Discussion, as I felt that the authors did an excellent job of framing their research within a wider theoretical and applied context, and linking their findings back to bigger picture questions about the generic controls on N biogeochemistry in tropical soils.

Specific comments on individual portions of the text are provided in the section below.

SPECIFIC COMMENTS

1. Referee: Page 5, line 16-page 6, line 9: Generally, I think that this section describing the hypotheses and overall experimental goals is well-written. However, my concern here is how to introduce the second part of the study comparing N gas fluxes in small versus large holder systems in a more intuitive way. The current structure of this section makes the study on small versus large holder systems seem a bit disconnected from the first phase of the work. One possibility might be to introduce this study earlier on in the paragraph, close to the section where the authors pose their hypotheses (which implicitly refer to N availability and the HIP model), as this would then implicitly link-up to ideas about N control on N fluxes, e.g. (my suggestions in the underlined section below): "We covered four different land uses within two landscapes on highly weathered soils that mainly differed in soil texture (clay and loam Acrisols): forest, rubber trees inter spersed in secondary forest (hereafter called jungle rubber) as the reference land uses, and smallholder rubber and oil palm plantations as the converted land uses. In addi tion, we conducted a follow-on study comparing N gas fluxes across a gradient of N input that encompassed small holder plantations (lower N input rates) a large-scale oil palm plantations (higher N input rates) to try and evaluate the effect of N input rate on N gas fluxes..."

Answer: We greatly appreciate this referee's suggestion and we also see that we should bring out early on the part on the comparison of soil N2O fluxes between smallholder and large-scale oil palm plantations. We take this suggestion which is now incorporated in page 5, lines 13-19 of the revised manuscript.

2. Referee: Page 7, lines 12-17: In the comparison study between small holder versus large holder systems, were measurements from the small holder systems collected at the same time (i.e. were fluxes from the two types of oil plantations collected concomitantly)? If so, then this should be made clearer in this paragraph.

Answer: No, the measurements between the smallholder and large-scale plantations

were not measured concomitantly, mainly because of logistical limitation. The measurement periods were also clearly stated in the original manuscript (pages 6-7, lines 24-6; page 7, lines 9-12; page 8, lines 22-24; page 9, lines 5-9), and now is clearly shown in Appendix Table A1 (see comment below). This was because our permission to work in the large-scale plantation was settled later than our agreement with the smallholders. However, this time difference in measurement period between these systems is accounted for in the statistical analysis - the linear mixed effect models include measurement period and replicate plot as random effects and only the oil palm plantation type as the fixed effect (Appendix A; page 47, lines 7-14).

3. Referee: Page 9, lines 7-17: It would be useful at the start of this paragraph to remind readers which land-uses were sampled in 2013, 2014 and 2015. Perhaps the authors could put together a table or something similar to represent this information?

Answer: We agree to these suggestions and put the measurement periods in an appendix table (now as Table A1 and the previous Table A1 now Table A2) for quick reference for the readers. We retain in the Methods (as referred to this page by the reviewer) all the description of these measurement periods and only give reference to Table A1 for summary.

4. Referee: Page 9, lines 18-20: Were the authors able to determine if N2O fluxes varied with distance from palms? Given the spatial structure in oil palm plantations, and the potential effects of roots and fertiliser application, it would be useful to know if the data could be corrected for spatial effects (if they exist) caused by proximity to palms.

Answer: The spatial structures of oil palms that are commonly seen in large-scale plantations are not consistent in smallholder plantations. The smallholder farmers also don't have regular spots for fertilizer applications, as we have explained in the manuscript based on our results. The deployment of the 4 permanently installed chamber bases per replicate plot is described in detail in section 2.1 "experimental design", page 6-7, lines 24-5. These chambers had random spatial locations in order to represent each replicate plot. These randomly placed chambers happened to be within 1.8 – 5-m distance to the palms and we conducted a Spearman's rank correlation test between N-oxide fluxes and distance to palms of the four replicate plots (sites) within each landscape. There were no significant correlations ($P = 0.84–0.94$). Thus, there was no basis for correction for any spatial effect as there exist no relationship with distance to the palms.

The best way to quantify any possible effects of fertilization is the way we described in our study, assuming that the random placement of chambers and the monthly sampling may have missed the fertilized spots and short-term effects of N application. Hence, we did the more intensive measurements following fertilization in the same smallholder plantations (using the same rate and application methods the smallholders claimed to employ) in order to quantify the contribution of fertilization, both in terms of space and duration, on our annual estimates.

5. Referee: Page 15, lines 16-25: I wonder if the large variation in the mean fluxes is driven by a high degree of within-plot spatial variability, which might linked to where fertiliser is applied, the distribution of palms, or surface residues (e.g. palm fronds or planted understory plants)? Is it possible to determine to what extent micro-scale variability, linked to spatial structure in the plantation, was causing variance in the measurements? This could help in interpreting the data, and understanding differences linked to management differences in small holder vs larger holder systems.

Answer: This question is related to our answer in #4 above. Fig. 1 shows the mean and SE from the 4 sites per land use on each sampling day. This variation, i.e. SE, on each sampling day reflected the variability among the 4 sites. The SE did not reflect within-plot variation because in the stat analysis (LME) the mean of the 4 chambers per plot (as subsamples nested within plot) on each sampling day is the value used in the LME analysis, which is conducted across all sampling days with land use as fixed effect and plot and sampling day as random effects (Appendix A, page 47, lines 2-3 and

6-7). Similarly, the statistically undetectable difference between the large-scale and smallholder oil palm plantations was also due to the large spatial variability among the 4 replicate plots and not by micro-scale variability within each plot. We have ascertained this in this large-scale plantation by statistical analysis since we have placed the 3 chambers/plot systematically to characterize any possible micro-scale variability within plots. In this large-scale plantation, we placed the 3 chambers/replicate plot such that the 1st chamber was on the fertilized band (at 0.8-1-m distance to the palm base) and the next 2 chambers were placed at a succeeding 2-m distance from each other. These 3 chamber locations, however, did not differ ($P = 0.70$) in soil N2O fluxes across the measurement period. This was due to the fact that the management practices were not consistently done as claimed by the plantation managers and smallholders. The field workers had sometimes broadcasted the fertilizers, sometimes also just applied in a band around the palm (page 8, lines 11-18), and sometimes piled the cut fronds in one row and sometimes in another row or sometimes not at all. The area and duration of effects of 1-2 times/yr fertilizer application in smallholders (which had 2-4 times lower application rates than in large-scale plantations) were small and only lasted for a few days (Figs. 2 & 3). We allocated full subsections of these effects in the Results and Discussion.

Even if we do a variance component analysis of the soil N2O fluxes, to partition the scales' contributions to the overall variance, this will not answer whether the within-plot variability is related to spatial structure of the management practices within plots. Variance component analysis will only quantify how much of the overall variability is accounted by within-plot variation. We think that the spatial structure of the management practices will only be detectable if there was a consistent management practices, e.g. when experimental plots are controlled by researchers. As we all know, smallholders as well as the large-scale plantations in reality do not have a uniform management practices in all years in terms of where fertilizers and residues are exactly placed, and hence we were unable to detect any statistically significant relationship of within-plot pattern of soil N2O fluxes with what is supposed-to-be the spatial structure management practices. That is however what was occurring in our actual field conditions. As we also did not find in the large-scale plantation correlation or differences in soil N2O fluxes between chamber locations and distance to palms, we also cannot relate within-plot variability to the spatial structure of this plantation. This is the main reason why we focused instead on our more frequent measurement following our own fertilization (mimicking farmers' claimed practice) to quantify the spatial and temporal contributions of fertilization on soil N2O fluxes (Fig. 2 & 3).

6. Referee: Page 16, lines 1-10: There is a potential confounding effect here due to the presence of roots which needs to be acknowledged. Granted, it is likely that the effect of fertiliser application will overwhelm the effect of roots in the immediate to short-term after fertilisation. However, it is worthwhile knowing whether or not the presence of roots ameliorates the effects of fertiliser (e.g. plant competition with nitrifiers/denitrifiers for inorganic N may reduce the relative gases loss of N in areas with high root densities). For example, do the authors have data on N gas fluxes from root-free and rhizosphere soil in the large holder systems to compare against? My thought here is that if the N application rate is higher in the large holder systems it may be possible to compare N fluxes from rhizosphere soil with different N application rates to evaluate the effect of N input rate on gas fluxes (i.e. making a like-for-like comparison).

Answer: From another study (conducted by another group in this collaborative project) that measured root distribution in the same smallholder oil palm plantations, there were no significant correlations between root mass distribution with distance to palms. This was attributed to the facts that these are mature plantations (12-16 yrs old, except one site that was 9 yrs old) and the weeding practices in smallholder plantations were not intensive (1-2 times per year only; Hassler et al., 2015) and hence the ground was almost always covered with undergrowth. It is impossible to see a root-free area. We don't think root can ameliorate the pulse effects of N fertilization on soil N2O fluxes (the total flux was also only 0.2-0.7% of the added N; page 17, lines 14-16), because we would have not seen a similar effect in tropical forest soils all covered with roots (e.g.

Koehler et al. 2009). In this latter study, soil N2O emissions clearly increase following N fertilization (at comparable application rate as we have in the present study) and went back to the background levels after about 6 weeks (during which the added N are already recycling within the soil N cycle). In the large-scale oil palm plantation PTPN VI (which was 12 yrs old), we don't have root data. Following Dr. Teh's suggestion of like-for-like comparisons, we conducted statistical analysis between the large-scale and smallholder plantations considering only the chambers on fertilized spots (the supposed-to-be spots which the smallholders and PNPT VI manager claimed where fertilizer was banded or broadcasted) and the sampling days within 6 weeks following fertilization; and a separate analysis for sampling days after six weeks of fertilization for chambers on locations which were not supposed-to-be fertilized. There was still no detectable significant difference between the large-scale and smallholder plantations ($P = 0.50$-0.67). Thus, the argument on confounding effect of roots was not convincing, at least from our dataset. The short-term effects of fertilizer application clearly showed the overwhelming effects (Figs. 2 & 3), although the emission percentage to amount of N added was actually only small; thus, presumably a large part of the added N must have been incorporated into the soil N cycle and eventually into the plant-soil cycling.

7. Referee: Page 16, lines 1-17: Regarding the use of locations a, b and c to refer to different distances to the palm; perhaps it may be possible to use identifiers that are a bit more descriptive, as this would make it easier for the readers to pick-up on the information quickly? e.g. 0.3 m = "inner root ball", 0.8 m = "outer root ball", 4-4.5 m = "inter-palm space" (or something similar)? Use of letters is a bit more abstract and (while clear) forces the reader to refer back to the tables or legends to remind themselves of the meaning of these abbreviations. Also – where trends are statistically significant, the authors could list the P-values from the multiple comparisons tests in parentheses to highlight where significant trends existed (I see that this has been done for the table, but would be useful for the reader if this was stated in the text, too).

Answer: We agree with the suggestion to use meaningful identifiers rather than a, b and c. To address this concern, we followed the suggestion of referee 1 and introduced the following clearer abbreviations: F1 = chamber location with incidental fertilization (0.3 m from the tree base), F2 = fertilized chamber location (0.8 m from the tree base), NF = non-fertilized chamber location (4Âň-4.5 m from the tree base) (page 10, lines 2-6 and Tables 2 & 4).

Furthermore, we indeed gave consistently the P values of all comparisons in the text and not just in the Table) (i.e. 1st and 3rd paragraphs in section 3.2 for comparisons among chambers).

8. Referee: Page 16, lines 18-22: Are these estimates derived from the trapezoidal extrapolations or some form of area-weighted upscaling?

Answer: The calculations for these estimates were explained in the Methods, pages 11-12, lines 18-4. From this equation, the total N-oxide emissions following fertilization (chambers F1 & F2) and the background fluxes (from the unfertilized chamber NF) are the trapezoidal calculations of the fluxes shown in Figs. 2 & 3 for each site/replicate plot. Since fertilizer-induced fluxes were limited in space and time, we also considered the fertilized area (multiplied by the tree density/ha) and the frequency of fertilizer application.

9. Referee: Page 18, section heading 3.3 Temporal controls of soil N-oxide fluxes: This section appears to discuss the relationship between environmental variables/drivers and N gas fluxes. Perhaps it may be more appropriate to re-name this section as "Role of abiotic variables in controlling N-oxide fluxes"? Or, if the authors may wish to more explicitly discuss how temporal variability in these environmental drivers contribute to fluctuations in N-oxide fluxes?

Answer: This page is still in the Result section – we mainly present (not discuss) the controlling factors of the temporal pattern of soil N-oxide fluxes. We keep this section heading, because we explicitly want to distinguish between temporal and spatial (section 3.4.) (and both sections considered abiotic factors) controls of N-oxide fluxes.

10. Referee: Page 18, section heading 3.4 Spatial controls of annual soil N2O fluxes: Similar to my above point (9), I do not feel that this heading properly describes what is discussed in the section. In this section, the authors discuss the relationship between N cycling processes rates and N-oxide fluxes, in order to evaluate the principal source of N oxides in these soils. They conclude that nitrification is probably the dominant driver of N-oxide fluxes because of the correlation between nitrification rates and gas fluxes. Perhaps the section could be retitled "Role of different N cycling processes in regulating N-oxide fluxes"? Also – I re-read the Methods and did not see the nitrification potential experiments described. Was this work done as part of another study or was this done as part of this work? In either case, this needs to be added to the Methods to make it clear that this work was done as the reference to nitrification (although interesting and relevant) came as a but of a surprise.

Answer: We have explicitly explained in the statistical analysis (page 14, lines 13-17) that for assessing the spatial controls of soil N2O fluxes, we used the annual flux per plot (and thus excluding the temporal variation) and conducted the correlation analysis with all the measured soil factors (physical and biochemical factors as well as the soil-N cycling processes) across the landscapes, encompassing soil conditions of the plots within the reference land uses and within the converted land uses. Thus, any significant relationships we observed suggested the range of conditions across plots and hence indicated the spatial controls. We also now added in the Methods (see answer to general comment #2 above) the descriptions of the sources of these soil controlling factors that were included in these correlation analysis for section 3.4. The control of gross nitrification was not interpreted as the main source of N2O fluxes in these soils but rather as an indicator of N availability in the soil; please see our answer to this similar comment in the 2nd general comment above.

11. Referee: Page 20, lines 9-22: Fluxes of NO from these systems, particularly oil palm, is extremely novel and of wider environmental significance, given the potential role of NO in tropospheric ozone formation, N deposition, and regional atmospheric

oxidant (OH) balance. It would be useful in the discussion if the authors could bring into the discussion some of the findings from earlier atmospheric sampling campaigns by the OP3 consortium (Fowler et al., 2011, Hewitt et al., 2009), where elevated NOx concentrations were found in the troposphere near oil palm plantations? Hewitt et al. (2009) and Fowler et al. (2011) suggest that the implications of enhanced NO emission from oil palm could be potentially regionally significant, and the work here in Sumatra on ground-based NO fluxes would be an interesting counter-point to the atmospheric sampling work from Sabah.

Answer: We very much appreciated this suggestion and included this aspect in section 4.2 "Land-use change effects on soil N2O and NO fluxes from oil palm plantations" (page 23, lines 16-22).

REFERENCES:

Allen, K., Corre, M. D., Tjoa, A., and Veldkamp, E.: Soil nitrogen-cycling responses to conversion of lowland forests to oil palm and rubber plantations in Sumatra, Indonesia, PloS one, 10, e0133325, 2015.

Hassler, E., Corre, M. D., Tjoa, A., Damris, M., Utami, S. R., and Veldkamp, E.: Soil fertility controls soil–atmosphere carbon dioxide and methane fluxes in a tropical landscape converted from lowland forest to rubber and oil palm plantations, Biogeosciences, 12, 5831–5852, 2015.

Hewitt, C. N., MacKenzie, A. R., Di Carlo, P., Di Marco, C. F., Dorsey, J. R., Evans, M., Fowler, D., Gallagher, M. W., Hopkins, J. R., Jones, C. E., Langford, B., Lee, J. D., Lewis, A. C., Lim, S. F., McQuaid, J., Misztal, P., Moller, S. J., Monks, P. S., Nemitz, E., Oram, D. E., Owen, S. M., Phillips, G. J., Pugh, T. A. M., Pyle, J. A., Reeves, C. E., Ryder, J., Siong, J., Skiba, U., Stewart, D .J.: Nitrogen management is essential to prevent tropical oil palm plantations from causing ground-level ozone pollution, P. Natl. Acad. Sci. USA., 106, 18447–51, doi:10.1073/pnas.0907541106, 2009.

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, Glob. Change Biol., 15(8), 2049–2066, 2009.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-357/bg-2016-357-AC2-supplement.pdf

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