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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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GENERAL COMMENTS

Data on N-oxide fluxes from oil palm ecosystems in SE Asia or elsewhere in the tropics is scarce, making it difficult to estimate or predict how existing land-use practices influence the flux of N-oxides to the atmosphere, and regionally/globally important processes such as tropospheric ozone formation, N-deposition, or climate forcing. Improving our understanding of the role played by management, soil type, environmental factors, and other control variables in modulating N-oxide fluxes from oil palm is critical if we hope to evaluate the impact of land-use change on the regional and global N cycle. This is especially important for a major land-use like oil palm, which accounts for

>13% of the tropical land area, and is expanding rapidly across the African and Latin American tropics.

The work presented here by Hassler et al. (2016) is therefore novel and timely because it helps us to start addressing these knowledge gaps. The focus of this research in one of the most heavily oil palm-dominated areas in SE Asia is also important, given that the process-level insights derived from this work can help us understand the functioning and behaviour of similar systems elsewhere in this area. The comparison among multiple land-uses, soil types, and different fertilization regimes (e.g. lower intensity small holder vs higher intensity large holder) also helps to develop a more generic and comprehensive picture of how different management practices influences N-oxide fluxes in the region. Inclusion of NO fluxes is especially exciting, because (as the authors note) we have little or no data on NO fluxes from oil palm systems so far. Given the importance of NO in regulating local/regional atmospheric chemistry (Fowler et al., 2011, Hewitt et al., 2009) and transport of reactive N across the landscape, understanding of NO fluxes could have more wide-ranging policy and management implications.

From an experimental perspective, the spatial sampling design was robust and the experiment was well-replicated. The authors are to be commended for collecting such a complete set of flux and environmental data from a remote field site with relatively poor infrastructure. The monthly sampling frequency is also adequate for capturing major trends in N-oxide fluxes, and the relationship between N-oxide fluxes and environmental variables. The higher frequency sampling study that investigated shorter-term trends in N-oxide flux following fertilisation also provides a good picture of how fertiliser application influences N-oxide fluxes. While it would be desirable to have fluxes collected at finer temporal resolution using quasi-continuous sampling methods (e.g. automated chambers), I do not believe that these kinds of data are necessary to test the questions and hypotheses posed here as the authors do not aim to construct an ecosystem N budget.

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. 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.

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 intra-annual 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.

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 are provided in the section below.

SPECIFIC COMMENTS

1. 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 interspersed 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 addition, 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..."

2. 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 concomi-

tantly)? If so, then this should be made clearer in this paragraph.

3. 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?

4. 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.

5. 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.

6. 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).

7. 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).

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

9. 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?

10. 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.

11. 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 emissions 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.

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