

# ***Interactive comment on “Conversion of tropical forests to smallholder rubber and oil palm plantations impacts nutrient leaching losses and nutrient retention efficiency in highly weathered soils” by Syahrul Kurniawan et al.***

**Syahrul Kurniawan et al.**

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Author's response:

First, we greatly appreciate the very detailed comments and suggestions of Dr. Teh. These help improve the clarity and broaden the perspective of our Discussion. All the comments of Dr. Teh are incorporated into our revisions. We describe below how we have addressed his comments.

General remarks and suggestions:

1. First, I think it may be worthwhile re-organizing the information in the discussion around the major findings, listing the top-level or most important findings first. The current structure of the discussion generally follows the order in which the results are reported, but there could be some value in arranging information according to the most ground-breaking or high impact results, in order to maximise the impact of the most important findings on the reader. Structuring a discussion in this way can be especially effective for data-rich papers like this one, because the discussion sections for data-rich papers can sometimes become quite large and extensive, and it is possible for key messages to get lost due to the volume of information covered.

Author's response: We agree to this suggestion.

Author's changes in the manuscript: We restructured the Discussion in the following:

Section 4.2 – focuses first on the reference land uses. We put topic sentence in the beginning of the paragraph to put first the high-impact results (previous L428-432 is condensed into a topic sentence; previous L447-448 is revised into a topic sentence). We minimized referencing back to the tables or figures with specific parameters, unless necessary. We streamlined every sentence to limit to the most convincing Results (previous L443-446 is deleted). At the end of the paragraph, we give a take-home message (previous L458-460 is moved up to this place).

Section 4.3 – now titled land-use change effects. The first paragraph focuses on the unfertilized rubber plantations and the second paragraph on the fertilized oil palm plantations, instead of separating the latter in the previous section 4.4. Although we emphasized in the previous manuscript version that the smallholder rubber plantations are not fertilized (the common practice of the smallholders in the area, at least during our study years of 2012-2013), we now highlighted this main difference in the soil management of the rubber and oil palm plantations. We also emphasized that the decreases in leaching losses in this rubber plantations and the increases in leaching losses in fertilized area of the oil palm plantations, as compared to the reference land uses, were

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mainly due to their management practices (i.e. without and with soil amendments). Again in each paragraph, we put a topic sentence (previous L465-470 is condensed into a topic sentence) to highlight the most important Results, and at the end of the paragraph a condensed take-home message.

2. Second, another topic that is theoretically interesting and also policy-relevant is whether or not the investigators believe that over-fertilization is occurring for the rubber and oil palm systems? To phrase this another way, are the higher nutrient losses for rubber and oil palm because fertilizer inputs exceed plant/ecosystem demand, or because of the transport-reaction properties of the different soil types (e.g. do the exchange properties and rate of physical transport through the soil mean that the soil exchange complex cannot retain some of the added nutrients)? If the answer is the former, then this suggests that growers could be reducing their inputs of some elements. If the answer is the latter, then mitigation options become more complex, because they may require new means of introducing fertilizers to the soil (e.g. slow release fertilizers, organic fertilizers, soil conditioners to enhance CEC, etc.). It would be interesting if the investigators could expand upon this topic further in the discussion.

Author's response:

These are very good suggestions. We take into considerations all these points. Our discussion was geared both on the regulation of soil texture and fertilization rates. We focused on the absence of fertilization (rubber) and low (clay Acrisol, oil palm plantations) versus relatively high fertilization rates (loam Acrisol, oil palm plantations). We did not hone our discussion on exceedance of the nutrient retention capacity of the soils. The reason is because we had 2-5 times lower fertilization rates at our studied smallholder oil palm plantations than the nearby large-scale oil palm plantations, such that it will be very speculative to say that the soil exchange complex are saturated or increasingly unable to retain the added nutrients.

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We emphasized in the revised section 4.3 that the smallholder rubber plantations were not fertilized (at least during our study period of 2012-2013), and this was in part because the price of rubber had gone down at that time (as shown in the Supplementary Fig. 9 of Clough et al. 2016).

In the second paragraph of section 4.3, we also emphasized that the fertilizer application in oil palm plantations, despite at very low rates, resulted in increased nutrient leaching losses compared to the reference land uses, particularly in the loam Acrisol soil.

We stressed in the 3rd paragraph of section 4.3 that the higher rates of fertilizer application in large-scale plantations than in smallholders imply for a need to optimize fertilization rate in order to minimize environmental effect while maintaining production level.

We also revised the previous L554-556 in Conclusion to this:

Management practices to regulate leaching losses are possibly more pressing for large-scale oil palm plantations, which have 2-5 times higher fertilization rates and may have a larger impact on ground water quality than the smallholders. Process-based models, used to predict yield and associated environmental footprint of these tree cash crop plantations, should reflect the differences in soil management (e.g., absence or low vs. high fertilization rates, weed control) between smallholder and large-scale plantations.

3. Third, two aqueous fluxes not included in this study are throughfall and stemflow. This observation is not meant as a criticism per se, as I fully recognize that this was very comprehensive and in-depth study, and resources are always limited for large-scale field experiments like this one. However, it would be useful if the investigators could comment on whether they think that differences in throughfall and stemflow among the different land-uses could have resulted in differences in nutrient dynamics and loss? Throughfall and stemflow are potentially influenced by factors such as vegetation struc-

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ture (e.g. plant density), leaf area and tissue chemistry, so it is possible that the different cover types (with different vegetation structure and properties) could have different patterns in throughfall and stemflow, with knock-on effects for soil nutrient dynamics.

Author's response:

This is a very important point. We incorporated our views on this aspect in section 4.1 last paragraph. In terms of magnitude, the highest throughfall nutrient depositions (from peat soils, influenced by land-clearing fires in Kalimantan; Ponette-Gonzales et al., 2016) are still much lower (<1-3%) than the extant soil-N cycling rates and nutrient stocks in the top 0.1-m soil at our sites. The effects of atmospheric nutrient deposition may not be on how much this has added to the soil nutrient levels but on whether or not the receiving ecosystem can serve as a sink and is able to buffer its other cascading effects (e.g. acidification).

Author's changes in the manuscript:

To keep the manuscript in the same length, previous L411-417 was shortened into one sentence. To incorporate these points raised by Dr. Teh above, we replaced the previous L418-425 with this:

From a peatland site in Kalimantan, influenced by land-clearing fires, throughfall nutrient depositions (19-22 kg N, 6-11 kg P, 25-44 kg S ha<sup>-1</sup> yr<sup>-1</sup>) are larger than those from bulk precipitation, indicating large contribution from dry deposition (Ponette-Gonzales et al., 2016). Total (dry + wet) nutrient depositions in our study region could be larger than the values from bulk precipitation. Such high atmospheric nutrient deposition may have fertilizing or polluting effect, depending on whether or not the receiving ecosystem is a sink and is able to buffer its other cascading effects (e.g., acidification). Additionally, atmospheric redistribution of nutrients in areas with widespread land-use conversion and intensification may have unforeseen effects on down-wind and down-stream ecosystems (e.g., Bragazza et al., 2016; Sundarambal et al., 2010).

Bragazza, L., Freeman, C., Jones, T., Rydin, H., Limpens, J., Fenner, N., Ellis, T., Gerdol, R., Hájek, M., Hájek, T., Iacumin, P., Kutnar, L., Tahvanainen, T., and Toberman, H.: Atmospheric nitrogen deposition promotes carbon loss from peat bogs, *Proc. Natl. Acad. Sci. U.S.A.*, 103, 19386-19389, <https://doi.org/10.1073/pnas.0606629104>, 2006.

Ponette-González, A.G., LisaMCurran, L.M., Pittman, A.M., Carlson, K.M., Steele, B.G., Ratnasari, D., Mujiman, and Weathers, K.C.: Biomass burning drives atmospheric nutrient redistribution within forested peatlands in Borneo, *Environ. Res. Lett.*, 11, 085003, <https://doi.org/10.1088/1748-9326/11/8/085003>, 2016.

Sundarambal, P., Balasubramanian, R., Tkalich, P., and He, J.: Impact of biomass burning on ocean water quality in Southeast Asia through atmospheric deposition: field observations, *Atmos. Chem. Phys.*, 10, 11323–11336, <https://doi.org/10.5194/acp-10-11323-2010>, 2010.

#### SPECIFIC COMMENTS:

1. Lines 49-51: Provide information for wider context: It is worthwhile emphasizing here that smallholdings are very common through SE Asia, and account for approximately 40% of the land under production throughout the region. Therefore, while the smallholdings in Jambi may represent a larger proportion of land area than elsewhere in SE Asia, smallholdings are common and thus important to understand.

Author's response: We incorporated this suggestion.

Author's changes in the manuscript:

The expansion of rubber and oil palm plantations has increased the income of Jambi, in particular the smallholder farmers (Clough et al., 2016; Rist et al., 2010), which account 99% of rubber and 62% of oil palm landholdings in the Jambi Province. In the whole of Indonesia, 85% of rubber plantations are smallholders and 40% of oil palm landholdings are smallholder plantations (DGEC, 2017).

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DGEC (Directorate General of Estate Crops), Tree crop estate statistics of Indonesia 2015-2017: Palm oil and rubber, Indonesian Ministry of Agriculture, 2017, <http://ditjenbun.pertanian.go.id/tinymcpuk/gambar/file/statistik/2017/Kelapa-Sawit-2015-2017.pdf>, .../Karet-2015-2017.pdf.

2. Line 147-150: Consider re-phrasing the description of the fertilization rates, as the current wording makes it a bit more difficult to understand. One option may be to breakup this sentence into two shorter sentences; one referring to the clay Acrisol and the other to the loam Acrisol.

Author's response: We take this in our revision.

Author's changes in the manuscript:

Fertilization rates were 48 kg N, 21 kg P and 40 kg K ha<sup>-1</sup> yr<sup>-1</sup> in the clay Acrisol soil, whereas these were 88 kg N, 38 kg P ha<sup>-1</sup> yr<sup>-1</sup> and 157 kg K ha<sup>-1</sup> yr<sup>-1</sup> (accompanied by CI input of 143 kg CI ha<sup>-1</sup> yr<sup>-1</sup>) in the loam Acrisol soil.

3. Line 161: Minor question or point for clarification: Do the authors have any insight as to where nutrient-acquiring roots proliferate in this system? Is it possible that sampling 1.3-1.5 m from the palm could slightly overestimate the rate of leaching loss? Oil palms tend to show the highest density of roots within 1 m of the plant stem; therefore, it is possible that by sampling outside of this region the investigators may underestimate plant uptake or overestimate leaching. Arguably, however, it is not clear if all the roots within 1 m of the palm stem are active or specialized for nutrient uptake, i.e. many of these roots may be dead or not directly involved in nutrient acquisition. Moreover, if the growers' practice is to apply fertilizer 1.3-1.5 m from the stem, then it is likely that this sampling scheme is likely to best represent actual trends in leaching. It is also possible that the roots produced 1.3-1.5 m from the stem are tracking nutrient availability and are specialized for nutrient uptake.

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From another study (conducted by another group in this collaborative research center) 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 (2 times per year only) and hence the ground was almost always covered with undergrowth. We think that we did not over-represent the leaching losses from the fertilized area as these values were averaged with the leaching losses from under the frond stacks to get a plot-scale estimate for an oil palm plantation.

4. Line 163: Did the growers plant any understory plants for erosion control? If so, did the authors sample from these areas too? Although the biomass and uptake capacity of these herbaceous plants is likely to be low relative to mature palms, leaching patterns are likely to be different from unvegetated areas.

Author's response:

In these smallholder oil palm plantations, the ground vegetation was that from naturally regrowth after the 2 times weeding per year. The ground was mostly covered with understory plants for most part of the year.

5. Lines 268-279: Do the investigators have an estimate for the nutrient input from throughfall and stemflow? If these data do not exist, is it possible to constrain these values in the model from similar systems? While rain water provides a useful end-member with which to estimate the nutrient content of "external" moisture inputs, it is possible that dry deposition of nutrients and leaching from aboveground plant parts could contribute to the nutrient input to soil. Especially if this region is near local sources of N pollution, it is possible that throughflow/stemflow could make a contribution to the overall N load to the soil.

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We did not measure stem flow and throughfall, and there are no data for stem flow and throughfall for these land uses for Jambi or Sumatra that we are aware of. It is likely that stem flow + throughfall is larger than from bulk precipitation in areas with large dry deposition from biomass burning. In terms of magnitude, those high throughfall nutrient depositions from heavily fire-impacted peatlands in Kalimantan (Ponette-González et al., 2016) are still much lower than extant N cycling rates and macronutrient stocks in the soil at our sites. As nutrients deposited into a system will eventually be incorporated into the soil-plant cycling, changes in leaching losses are ultimately reflecting how efficient the system (soil, biota and vegetation) is in retaining the nutrients from both external sources and internal cycling.

Author's changes in the manuscript: Please see our answers to reviewer's major comment 3 above.

6. Lines 317-320: What are the comparable values for ET, run-off and drainage for rubber and oil palm systems?

Author's response: We provide this information.

Author's changes in the manuscript:

In rubber and oil palm, modelled annual ET was 30-32 %, runoff was 22-31 %, and drainage was 37-47 % of annual precipitation.

7. Lines 395-534: Given that the authors introduce testable hypotheses in the introduction, I think it's important to "close the circle" by referencing these hypotheses in the discussion, and confirming if the authors' findings supported or falsified their hypotheses.

Author's response: We agree with the reviewer.

Author's changes in the manuscript:

We re-structured the Discussion according to the 1st and 2nd major suggestions above.

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In the Conclusion, we closed the circle by linking our findings to our hypotheses (stated in the 2nd-5th lines in the Conclusion).

8. Line 441: Further clarification required re: the phrase “higher rates of soil  $\text{NH}_4^+$  cycling.” For those who have not yet read Allen et al. (2015), does this phrase mean that the rate of  $\text{NH}_4^+$  mineralization is greater, gross production and uptake of  $\text{NH}_4^+$  is greater, or that the overall turnover of  $\text{NH}_4^+$  is greater?

Author’s response:

Gross production and immobilization of  $\text{NH}_4^+$  were positively correlated and both rates are large, meaning this internal cycling was large and closely-coupled, and was mirrored by low TDN leaching fluxes in the clay than loam Acrisol soils.

Author’s changes in the manuscript: We changed this sentence accordingly.

9a. Lines 491-534: One question and one comment: first, given the finding that fertilization is enhancing leaching losses in these smallholder landscapes, do the authors believe that the growers are over-fertilizing? Is the high rate of leaching loss because the plant demand is lower than nutrient supply, or is it because transport factors mean that the nutrients are lost before plants are able to take-up the nutrients? The authors expert assessment directly influences policy and management decisions; if it is an over-fertilization situation (i.e. plant demand  $\hat{=}$  nutrient input), then the mitigation option would be to reduce fertilizer inputs. If it is an issue of transport (e.g. ion exchange sites are saturated or movement of soil solution is too rapid for efficient plant uptake), the different mitigation options suggest themselves (e.g. use of slower release fertilizers, or other technologies to reduce nutrient transport through the soil column). The conclusion that soil texture was the dominant influence (lines 528-529) tends to imply that the authors believe the second option is more likely (i.e. rapid transport leads to loss, rather than plant demand  $\hat{=}$  nutrient input); however, it would be useful to hear the authors thoughts on this topic given its wider importance for mitigation of nutrient pollution.

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## Author's response:

These smallholders are not over-fertilizing; please see also our answer to the 2nd major comment above. The high leaching losses from oil palm plantations occurred particularly on the fertilized area around the palm base and were higher in the loam Acrisol soil, which also happened to have a larger fertilization (e.g., 48 vs. 88 kg N/ha/yr, section 2.1), than the clay Acrisol soil. The plant demand, using simply the index of fruit harvest export (72-96 kg N/ha/yr; Kotowska et al. 2015), was certainly higher than the fertilization rates of the smallholders, whose low fertilization rates were largely determined by their resources of being able to afford the cost of fertilizers. Thus, our discussion was focused more on the regulation of soil texture and fertilization rate rather than on the exceedance of the ecosystem's capacity to retain the added nutrients from fertilizers. The leaching losses that we measured are possibly contributed by both soil texture (not only through adsorption/exchange capacity but also on solute transport) and fertilizer application. We think the role of transport occurred on pulses or time periods when high rainfall occurred following the 2 periods in a year when fertilizers were applied. Unlike, however, N<sub>2</sub>O emissions from the soil surface of which temporal pattern following fertilization are clearly manifested (Hassler et al. 2017), this is not the case for leaching losses - probably because transport of solute down to 1.5-m depth (lysimeter sampling) can take days of intermittent rainfall events.

## Author's changes in the manuscript:

As to the aspect of our findings' implications to management, we addressed this in the last paragraph of section 4.3.

'The fertilization rates in our studied smallholder oil palm plantations were only 2-5 times lower than the nearby large-scale plantations, typically with 230-260 kg N ha<sup>-1</sup> yr<sup>-1</sup>. Our findings, that leaching of TDN and base cations increased and their retention efficiency decreased particularly in the loam Acrisol despite the low fertilization rates (Tables 4 and 5), imply for a need to optimize fertilization rate in large-scale plantations,

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especially on coarse-texture soils which have low inherent nutrient retention, in order to minimize environmental effect while maintaining production.’

9b. My second point is a comment rather than a question. One of the challenges in predicting the behaviour of smallholder systems is that there is potentially a wider diversity of practices and fertilization schemes compared to large-scale industrial plantations. For instance, depending on the relative wealth or resources of individual growers, they may have better access to fertilizers than less fortunate growers. While this does not necessarily take away from the message that the authors are trying to convey here (i.e. that certain types of more “intensive” or “invasive” land-use can show enhanced leaching losses), I think it is useful to discuss this source of potential variance and uncertainty, since it means that we have to develop better process-based models so that we can adequately predict flux from smallholder systems.

Author’s response: We completely agree to this comment.

Author’s changes in the manuscript: We added this in the Conclusion.

‘Process-based models, used to predict yield and associated environmental footprint of these tree cash crop plantations, should reflect the differences in soil management (e.g., absence or low vs. high fertilization rates, weed control) between smallholder and large-scale plantations.’

10. Table and figure legends: Minor pedantic point: throughout the table and figure legends, the authors refer to the loam Acrisol and clay Acrisol as two different “landscapes.” While I do not consider this as problematic as such, I wonder if the phrase “soil orders” or “soil types” may be more intuitive for the reader, given that the reference for these two types of environments are the names of the soil orders?

Author’s response: Yes, we agree. We changed all these from landscapes to soil types.

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-221>, 2018.

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