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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.

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GENERAL COMMENTS This is an interesting and very topically-relevant paper, given the large, global extent of oil palm production and the current drive to understand not only oil palm's environmental impacts, but also to derive potential mitigation options for small growers and large agribusinesses. The contrast between different soil types (e.g. loam versus clay) is also very instructive, given that oil palm is cultivated on a mixture of soil types all over the tropics, and capturing this range of variability will help stakeholders develop a better predictive understanding, that includes knowledge

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of how soils properties (e.g. texture, etc.) play a role in modulating aqueous fluxes of nutrients. Lastly, the focus on smallholdings is also welcome, given that these systems form such a large part of the production landscape, and are often under-represented in existing projects which have focused on larger-scale industrial plantations.

Overall it is my assessment that this paper was clearly written and well-structured. The methods appeared wholly appropriate for the research questions and hypotheses tested here. The approach to data analysis and interpretation appears logical and well-reasoned. I therefore do not find that this paper requires too much modification prior to publication, since this is – in my view – a solid and rigorous piece of research, that will make a meaningful contribution to our wider understanding of the aqueous biogeochemistry of managed tropical landscapes in Southeast Asia.

However, I did have a few general remarks and suggestions for improvement. More specific comments are provided in the section which follows this one. 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.

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.

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 structure (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.

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.

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 break-up this sentence into two shorter sentences; one referring to the clay Acrisol and the other to the loam Acrisol.

3. Line 161: Minor question or point for clarification: Do the authors have any insight as

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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.

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.

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.

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

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.

8. Line 441: Further clarification required re: the phrase "higher rates of soil NH4+ cycling." For those who have not yet read Allen et al. (2015), does this phrase mean that the rate of NH4+ mineralization is greater, gross production and uptake of NH4+ is greater, or that the overall turnover of NH4+ is greater?

9. 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 « 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 « nutrient input); however, it would be useful to hear the authors thoughts on this topic given its wider importance for mitigation of nutrient pollution.

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.

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?

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