

# ***Interactive comment on “Comparison of greenhouse gas fluxes and microbial communities from tropical forest and adjacent oil palm plantations on mineral soil” by Julia Drewer et al.***

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Anonymous Referee #1

This manuscript by Drewer et al. examines how soil N<sub>2</sub>O, CO<sub>2</sub>, and CH<sub>4</sub> emissions differ between tropical forests and oil palm plantations. The authors show that mineral soil N<sub>2</sub>O emissions are higher from Oil Palm plantations compared to Forest or Riparian areas. Furthermore, the authors demonstrate that microbial communities differ between these land use types. The methods and data presented in this manuscript appear to be appropriate and support the author’s main conclusions. To improve this manuscript, I suggest the authors better integrate this study in the context of their pre-

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viously published study on the same system (Drewer et al. 2020) and improve their justification for measuring microbial community composition. My specific comments are detailed below.

Response: We thank the reviewer for taking the time to read and comment on our manuscript. We reply to individual comments in turn below.

General comments: My main comment is that the authors do not adequately justify how total bacterial and fungal diversity will explain soil trace emissions.

Response: We will add additional references to the introduction to clarify this point. We measured both N<sub>2</sub>O and CH<sub>4</sub> - fluxes which are the result of the activities of many different microbes – consumers and producers, therefore we just wish to examine if broad microbial metrics can help further explain flux variance.

I suggest the authors use their previously published manuscript (Drewer et al. 2020) as justification for measuring microbial community composition in the introduction. Additionally, the authors only present total bacterial and fungal community composition data, and do not discuss which taxa contributed to the difference in microbial communities based on land use. By examining the known function of taxa that differ between land uses, the authors may be able to better link differences in microbial community composition to soil trace gas emissions. These data should be available based on the sequencing analyses described in the methods section.

Response: We used the opportunity to investigate if there are changes in microbial community structure related to land-use change. We never tried to go beyond this (linking processes with specific taxa) as was done in the lab study. A detailed examination of specific microbial responses is beyond the scope of the current manuscript; and we prefer to present conclusions based on broad community metrics in the current MS.

Specific Comments: Line 104-124: The authors do a nice job explaining how land use

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change might alter soil microbial communities in this paragraph. However, they do not justify how changes in bacterial/fungal diversity and richness might directly affect soil N<sub>2</sub>O emissions. To do this, the authors might consider presenting some of the main findings from their prior study (Drewer et al. 2020) in this paragraph.

Response: Fluxes are driven by many different microbes – producers and consumers and activities may be the result of multiple organismal interactions – this is why we focus on broad community metrics.

Line 125 – 128: It would be useful if the authors presented hypotheses to accompany these objectives. This would help explain why they expect N<sub>2</sub>O emissions to vary based on land use and what soil properties they expect to drive these differences.

Response: We will add following hypothesis to the MS: N<sub>2</sub>O fluxes will be larger from OP due to N fertiliser addition compared to tropical forest

Line 155-156: I found the description of the study sites a little confusing in this paragraph. Here, what do “LF, B, and E” stand for?

Response: We followed the design of the SAFE project and used their site names for consistency with other SAFE publications. We will add the land-use type as an extra column to table 1 and clarify in all tables and figures.

Line 168 – 169: The authors should justify why they did not install equal number of chambers in each site.

Response: We did install approximately equal numbers of chambers at each site, though not exact. To clarify the SAFE design, there were replicate sites for each land-use, with chambers installed at random locations within these sites. We had 3 forest sites each with 8 chambers randomly installed, 2 OP also with 8 randomly installed. The only deviation was OP7, where we tried to capture topography so had 12 chambers. The riparian was the outlier with only 4 but we don't focus on that in the land-use comparison. Our analysis does not assume a balanced design, and can cope with the

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slight difference in the number of samples between sites.

Line 171-173: The authors should explain how they decided when to sample. Did they account for antecedent conditions such as time since last rain event?

Response: We tried to capture a 2 year period and had to take into account accessibility of the site so every two months was a pragmatic time scale that was achievable within the budget and allowing sample processing in between.

Line 213 – 219: The authors did a nice job of describing how they calculated fluxes. However, if CO<sub>2</sub> or N<sub>2</sub>O concentrations became saturated within the chamber headspace then linear regression could underestimate emissions – see Matthias et al. (1978) “A numerical evaluation of chamber methods for determining gas fluxes”. The authors should discuss how they addressed this here.

Response: Fluxes have been quality checked and checked for linearity and no saturation occurred during the time sampled (2 min for CO<sub>2</sub> and 45 min for N<sub>2</sub>O), so linear was the best fit for all fluxes presented here. We will add an extra sentence to clarify this.

Line 420: “CH<sub>4</sub> oxidation” should be changed to “net CH<sub>4</sub> oxidation”

Response: We will change the text accordingly.

Line 454-456: The authors should include these data in their ordination figure or as a table.

Response: We will add this to the supplementary information as it won't add much more to what is already in the text.

Line 603-608: I think this info could be incorporated into the previous paragraph.

Response: We will amend the text accordingly.

Figure2: Here, and for all the figures, it would be useful if the OP sites were ordered

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from youngest to oldest.

Response: We will change the plots accordingly.

Figure 3: The y-axis scale makes it difficult to see any patterns in the NH<sub>4</sub> concentrations in the forest and riparian sites. I understand that this is to keep the axis scale consistent, but the authors should consider using a log scale or a broken y-axis so that the figures are easier to interpret. This is also true for figure 4.

Response: As there are no real patterns, we decided to use this format but can replot the figures using a broken axis.

Figure 5: It would be useful to see these data over time so we can see the presence or lack of any temporal patterns. This is also true for Figure 6.

Response: As there are no temporal patterns, we decided to use this format but for consistency can replot these figures the same way as figure 4a.

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