

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.

Anonymous Referee #1

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This manuscript by Drewer et al. examines how soil N₂O, CO₂, and CH₄ emissions differ between tropical forests and oil palm plantations. The authors show that mineral soil N₂O 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 previously 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.

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General comments:

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

Specific Comments:

Line 104-124: The authors do a nice job explaining how land use 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 N₂O 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.

Line 125 – 128: It would be useful if the authors presented hypotheses to accompany these objectives. This would help explain why they expect N₂O emissions to vary based on land use and what soil properties they expect to drive these differences.

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?

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

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?

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Line 213 – 219: The authors did a nice job of describing how they calculated fluxes. However, if CO₂ or N₂O 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.

Line 420: “CH₄ oxidation” should be changed to “net CH₄ oxidation”

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

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

Figure2: Here, and for all the figures, it would be useful if the OP sites were ordered from youngest to oldest.

Figure 3: The y-axis scale makes it difficult to see any patterns in the NH₄ 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.

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.

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