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Interactive comment on "Soil fertility controls soil–atmosphere carbon dioxide and methane fluxes in a tropical landscape converted from lowland forest to rubber and oil palm plantations" *by* E. Hassler et al.

Anonymous Referee #2

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The presented study aims to document the magnitude and investigate the drivers of soil-atmosphere carbon dioxide and methane exchange in Sumatra, Indonesia. To this end, two land-use change gradients on soils of differing texture were used to investigate the influence of soil type and forest conversion. Given considerable economic pressure for forest conversion, the reported data represents a timely and comprehensive contribution to the relatively sparse literature addressing the dynamics of soil-atmosphere greenhouse gas exchange in the region. The experimental design reflects considerable dedication in the field and manuscript is generally well written and reasoned. With this in mind I have a few queries and suggestions that I feel should be considered to improve the clarity of the work for future readers.

1) Very minor but I'd quite like to see annual fluxes reported in the abstract to provide a point of reference for the key findings described.

2) I think the methods section would be clearer if the text (Page 9174, line 15 – Page 9175, line 7) describing the experimental set-up of the fertilisation manipulation is moved from '2.2. CO2 and CH4 flux measurement' and included in the previous section '2.1. Study area and experimental design'.

3) It is stated that carbon dioxide fluxes were calculated using a linear model fit, however, the text does not make it clear if this approach was also applied to methane fluxes (Page 9174, lines 5 –9). Evidence of non-linearity in the change of headspace concentration (a common observation in static chamber data that is acknowledged by the authors on Page 9173, line 1) might imply that a different approach to flux calculation e.g. Pedersen et al., 2010 may be more appropriate. Clarification of the approach used is required.

4) Similarly, the authors indicate that net zero methane fluxes were retained in their dataset (Page 9174, line 10) but do not state the criteria used to define measurements as such e.g. Verchot et al., 2000, Pedersen et al., 2010, Parkin et al., 2012. I think it is important to indicate the lines along which measurements are defined as zero or omitted from the dataset (i.e. non-significant fits resulting from small flux rates vs. those caused by errors in sampling, storage or analysis) and the number of 'zeros' retained. The treatment of zero fluxes in the literature can be somewhat patchy, despite the fact their inclusion / exclusion inherently introduces biases, so this sort of information is very useful to future readers when comparing reported flux rates across studies. Again, clarification is required.

5) Which variables were transformed and where these transformations were subse-

quently used in statistical tests should be indicated (Page 9176, line 13). Full assessment of the reported relationships is not really possible in the absence of this information.

6) Mixed effect models are used to account for spatial and temporal structures in the experimental design when testing the effect of categorical variables (i.e. landscape and land-use) on mean fluxes at the level of plot/palm and sampling date. However, this approach is not extended to testing relationships between fluxes and other continuous variables. Instead the dataset is reduced and Pearson's or Spearman's correlation tests are applied to investigate temporal and spatial variability. This seems fine to me but I think it might be useful for the authors to (v. briefly) explain the reasoning behind their approach (Page 9176, line 14 – Page 9177, line 15).

7) It's not always clear which statistical tests have been applied throughout the results section. Maybe include a reminder in sections 3.3 and 3.4 that the reported correlation coefficients are Pearson's r and Spearman's rho, respectively.

8) The results of the fertilisation manipulation seem to receive relatively limited discussion beyond consideration of spatial footprints. For example, smaller carbon dioxide fluxes where identified at the furthest chamber location from a palm (Page 9179, line 7). It's not clear whether this is a result of the application of the fertilizer or whether it results from a more general pattern related to distance from palms e.g. driven by differences in root biomass and respiration. Is a relationship present between flux and distance from palms in the main dataset (i.e. when considering chambers positioned $\sim$ 1.8 – 5 m from palms and measured monthly throughout the year; Page 9179, line 13)? Or did the flux return to pre-fertilization levels as reported for methane (Page 9179, line 27- 28)?

9) Support for the main thrust of the manuscript in relating carbon dioxide and methane fluxes to soil fertility is heavily reliant on the results reported in section '3.4 Spatial controls of annual CO2 and CH4 fluxes across land-use types within each landscape' i.e.

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the relationships presented in the abstract relating carbon dioxide flux positively with SOC and negatively with 15N, extractable P and base saturation (Page 9165, line 14 – 16) and methane uptake negatively with N availability and positively with Al availability (Page 9165, line 20 – 23) are presented here. However, I find this section a little hard to follow as a correlation matrix (like Table 3 for temporal relationships) isn't shown. I realise there are a large number of variable pairs and multiple scales considered but I think some sort of table could be very useful given how central these results are to the manuscript. Particularly, I'm unclear as to whether relationships between annual means of fluxes and environmental variables were considered as drivers of spatial variability. Given the highlighted importance of WFPS in driving temporal variability in methane flux (Table 3) from these soils I would like to see the possibility that variability in WFPS could be driving spatial variability addressed. Indeed, a lack of a relationship here, as similarities in bulk density (Table A1) and unclear patterns in WFPS between land-uses (Figure 1) might suggest, would serve to strengthen the argument made for fertility as the key driver across this system.

References

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