Answers to the interactive comment by Anonymous Referee #2

Before we address the concerns of referee #2, we would like to thank the referee for the time she/he invested to write her/his thoughtful and constructive comments. For clarity we have copied the review in italics and address the concerns of the referee, using normal fonts

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.

Following this good suggestion we have now included the mean CO2 and CH4 fluxes (or range of fluxes) in the abstract. We chose to report the mean fluxes instead of the annual fluxes, since the mean fluxes are purely based on measurements, while the annual fluxes are based on trapezoidal extrapolation.

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'.

We agree with this suggestion and moved the description of the fertilization experiment to section 2.1

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.

In page 10 L3-L12, we addressed this question in the revised manuscript. Only in very few measurements of CO_2 fluxes, the last concentration measurement at the last sampling time (at 31 minutes after chamber closure) was not linear compared to the first 3 sampling times (at 1, 11 and 21 minutes after chamber closure). In such few cases, we excluded the last data point and calculated the fluxes based on the linear increase in concentrations during the first 3 sampling times. For the large majority of CH_4 flux measurements, the measured CH_4 concentrations were strongly linear with time (during 31 minutes after chamber closure). Only in a few cases when CH_4 uptake was low, CH_4 concentration change with time of chamber closure showed low R^2 in linear regression. In these few cases, however, the corresponding CO_2 concentrations (from the same gas sample as the gas sample was analyzed consecutively for CH_4 and CO_2) were linear, indicating no mistake in sampling. Thus, we still estimated fluxes from these low changes in CH_4 concentration with time using linear regression because, even if there was low linearity exhibited, this flux was a real manifestation of the balance between CH_4 uptake in and emission from the soil. All CH_4 flux measurements were included in all statistical analysis. This approach was also followed in the study by (Verchot et al., 2000), that was referred to by reviewer #2.

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.

Apparently our description how we handle methane fluxes that were not significantly different from zero was not clear. We recognized that some of the fluxes were not significantly different from zero; however these fluxes were not omitted from the dataset. Therefore, we did not use any criteria to exclude zero fluxes and accordingly we do not report these criteria. We have now changed the description in the methods section as follows (page 10, L8):

'There were a few measurements when CH_4 concentrations changed only minutely with time of chamber closure, mostly when net CH_4 uptake was low; in such cases, the calculated CH_4 flux using linear regression was not significantly different from zero. These fluxes were however retained in the statistical analyses to avoid bias by excluding low CH_4 fluxes or by assuming that these fluxes were zero.'

5) Which variables were transformed and where these transformations were subsequently 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.

We now specified (page 11, L 17-18) which transformations were used for the measured parameters, whenever necessary for the comparisons among land-use types within each landscape of between landscapes within each land-use type.

'. . . and if necessary a logarithmic (for CO_2, CH_4, and mineral N) or square root (for WFPS) transformation was used.'

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).

The statistical results are essentially the same either using LME with mean centering of the data set or using the mean values of replicate plots for each treatment (land use) on each sampling day and conducting Pearson correlation across the measurement period (i.e. in our study n=12 months) in order to assess the temporal controls on soil CO_2 and CH_4 fluxes (section 3.3). We straightforwardly used Pearson correlation as this is more commonly reported; also, as the correlation coefficients using Pearson or LME with mean centering of the data are the same anyway. The reason why we used the mean values of the 4 replicate plots for each land use type on each sampling day is in order to represent the center of the spatial structure per land use and focus the analysis mainly on the temporal pattern for such land use. We added this reason in page 12 L6-7.

For the spatial controls on soil CO_2 and CH_4 fluxes (section 3.4), we used the annual fluxes from each replicate plot within each landscape (n=16 for 4 land-use types x 4 plots) and the average soil physical and biochemical characteristics per replicate plot (Table A1), which were only measured once in 2013 since these soil variables are not going to vary within a year. In the same analogy as that with the temporal control, we used the annual values to represent the temporal pattern across the monthly measurement and focus this analysis on spatial pattern. Since we used the individual plot value (not the average of the 4 plots per land use), we used the Spearman rank correlation so as not to conduct any data transformation of the soil variables which have non-normal distribution. We added this reason in page 12 L13-14.

These are the approaches that we have typically employed in many of our earlier works on soil trace gas flux measurements (e.g. Koehler et al., 2009; Veldkamp et al., 2013; Corre et al., 2014) in order to present the data analysis correctly yet in a less confusing manner.

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.

We have partly followed this suggestion. Since all our figures and tables clearly state our statistical methods (including Table 3 where the Pearson's correlations are reported) we did not add this information in section 3.3. However, the first time we report the Spearman's rho correlation coefficients in section 3.4, we specify this test. Furthermore, we changed the symbol that we used for Spearman's correlation to rho (ρ) throughout the text in order to make this clearer.

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 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)?

We did not put a lot of emphasis on the fertilization manipulation since the temporal and spatial effects that we measured were relatively insignificant, as we also stated in the Results (page 14, L1-5, L19-21). There was no systematic change (i.e. significant correlation) in CO_2 and CH_4 fluxes with distance to palms when we conducted Spearman correlation using annual fluxes from all individual chambers. Since the subplots where measurements were conducted were selected randomly and the chamber bases were also randomly located in the subplot, such absence of systematic change with distance to palms indicated that the subplot measurements represented the plot. Moreover, none of the chamber bases in this dataset were closer than 1.8 m from the palm tree, whereas the strongest effects from fertilization was within 1 m from the palm trees. The effects that we reported for CO_2

fluxes in the fertilization experiment were the result of a general pattern, probably related to differences in root biomass and respiration. They did not disappear or change with time as was the case with CH_4 fluxes.

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. 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.

The main reason why we chose not to present the correlation matrices in section 3.4 is that there are simply too many variables, and instead we chose to present only the correlations of annual soil CO_2 and CH_4 fluxes with soil variables that showed significant correlations. So the correlations mentioned in the text are the only ones that were significant and hence it makes no sense to present all soil variables in a correlation matrix table. As stated in Statistical analysis (page 12, L2-7): we first conducted a Spearman correlation of annual CO_2 and CH_4 fluxes with the soil physical and biochemical characteristics (Appendix Table A1) for the reference land uses across the two studied landscapes (correlation matrix 1), and second across land-use types for each landscape separately (correlation matrices 2 & 3). We think that such correlation matrices are too much to present, even for an Appendix because many soil variables are auto-correlated with each other and only few soil variables (the ones mentioned in the text) showed significant correlations. This last point was maybe not clear and we have now added the following sentence to paragraph 3.4 (page 15, L28-29):

'Apart from the correlations reported here, there were no other significant correlations with any of the tested soil physical and biochemical characteristics.'

There was no significant correlation between annual CH_4 fluxes and WFPS, since we had to average the WFPS across the monthly measurements to correlate with one annual value. In essence, if one has to look at the spatial pattern of annual soil CH_4 fluxes across plots in a landscape, WFPS (which displayed quite significant seasonal variability; Fig. 1a, b) is not a good predictor variable but probably annual rainfall. WFPS is more appropriate variable to correlation with temporal pattern of soil CH_4 fluxes (as we have reported in Table 3 and section 3.3).

References

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Koehler, B., Corre, M. D., Veldkamp, E., Wullaert, H., and Wright, S. J.: Immediate and long-term nitrogen oxide emissions from tropical forest soils exposed to elevated nitrogen input, Global Change Biology, 15, 2049-2066, 2009.

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Verchot, L. V., Davidson, E. A., Cattanio, J. H., and Ackerman, I. L.: Land-use change and biogeochemical controls of methane fluxes in soils of eastern Amazonia, Ecosystems, 3, 41-56, 2000.