Answers to the interactive comment by Anonymous Referee #1

Before we address the concerns of referee #1, 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

GENERAL COMMENTS Oil palm is one of the most rapidly expanding and financially important cropping systems in the tropics, yet little is known or understood about the biogeochemistry of these ecosystems, or their ability to release or sequester carbon. The work presented here is therefore interesting, important and novel because it provides much-needed, high quality empirical data on the fluxes of carbon from representative systems in the Southeast Asia region, over an annual cycle. This manuscript distinguishes itself from many of the other papers that have preceded it, because the sampling is more spatially extensive and over a longer time period than other work, enabling the investigators to make more confident assertions about the annual fluxes of CO_2 and CH_4 from these systems.

The findings from this work will provide an excellent basis for understanding the mechanistic controls on CO_2 and CH_4 fluxes from oil palm systems planted on mineral soils, with the potential for future modelling or up-scaling. The focus on smallholder systems is also noteworthy, because smallholder cultivation typically accounts for 40-60 % of all plantations in Southeast Asia, and therefore represents a sizeable proportion of land cover under this cropping system. Studies on smallholder systems are also less common, and therefore this study serves as a useful point of comparison compared to measurements from large, corporate plantations. However, one of the obvious scientific challenges for this study is the fact that smallholder management tends to be more heterogeneous than that of large agribusinesses (e.g. see page 7 line 22 – page 8 line 9). Yet despite this constraint, I believe that this dataset is still easily interpretable, given the selection of adequate controls (i.e. forest, jungle rubber) and the concomitant measurement of continuous independent variables (e.g. available N, P, soil texture, temperature, moisture, etc.). In addition, the use of small-scale manipulative field experiments, such as the simulated fertiliser addition study, helped to more unambiguously establish the mechanistic links between the fluxes and measured environmental variables.

My only other comment/minor concern is with respect to the content and organization of the Discussion; while the authors do a good job of comparing their findings against the existing literature, I felt that the paper could be made more impactful if the authors made it clearer in the Discussion which of their findings was novel or interesting. As it is written, it is sometimes difficult for the reader to identify the most exciting results from this study. The Discussion could also be slightly streamlined in terms of length, or slightly re-packaged/revised to better highlight the most important findings.

We have addressed this concern by making the following changes: throughout our discussion we now start each paragraph with an introductory sentence in which the most important and novel interpretation discussed in this paragraph is presented, followed by the reasoning behind this. In the previous version, in some parts of the discussion this was already the case, but in other parts, the most exciting findings were 'hidden' in the text.

In the previous discussion, we were not able to cite a source showing reliably that under oil palm the soil carbon stocks strongly decrease with time. The source that we referred to (Allen et al., 2015) did not show a decrease, which was caused by the large spatial variability of soil carbon stocks among

our sites. However, we were now able to 'streamline' the discussion by citing a study that was recently published by our group, which show strong reductions of soil C stocks in oil palm plantations (van Straaten et al., 2015). This study, which was also conducted in Jambi province, had many more site replicates, and showed that the observed decreases in soil C stocks were highly significant.

We think that by making these changes we have adequately addressed this concern of reviewer #1.

Specific comments on individual portions of the text are provided in the section below.

SPECIFIC COMMENTS 1. Page 9, line 14: Were any of the CO2 or CH4 data nonlinear? If so, how were these data dealt with?

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.

2. Page 13, lines 25: Please revise this sentence, as the structure is a bit awkward and the sentence does not read smoothly.

We changed this sentence into (page 11 L27):

'We extended the LME model to include either 1) a variance function that allows different variances of the fixed effect, and/or 2) a first-order temporal autoregressive process, which assumes that the correlation between measurements decreases with increasing time difference, if this improved the relative goodness of the model fit based on the Akaike information criterion.'

3. Page 22, lines 19-29: As the authors allude, one cause for the reduced soil respiration fluxes in oil palm system may be because of lower root respiration in the oil palm system relative to the forest due to lower overall root biomass. Do the authors know if the oil palm systems had lower root biomass than their other ecosystems? Could they use data from spatially explicit sampling (e.g. sampling in gradients away from palms/trees; see page 13 lines 19-28) to estimate root versus heterotrophic (saprotrophic) respiration? I realise that this would be a back-of-the-envelope calculation, but it may be a useful point of discussion, given that the spaces between palms in many plantations have very sparse plant cover (and therefore could be used to estimate the root-free rate of soil respiration).

As we point out in the discussion, fine and coarse root production was lower in the oil palm plantation compared to the forest (Kotowska et al., 2015). We did not conduct spatially explicit sampling of the root biomass and unfortunately cannot make the back-of-the-envelope calculation as suggested by reviewer #1.

References

Allen, K., Corre, M. D., Tjoa, A., and Veldkamp, E.: Soil nitrogen-cycling responses to conversion of lowland forests to oil palm and rubber plantations in Sumatra, Indonesia, PloS one, 10, e0133325, 2015.

Kotowska, M. M., Leuschner, C., Triadiati, T., Meriem, S., and Hertel, D.: Quantifying above- and belowground biomass carbon loss with forest conversion in tropical lowlands of Sumatra (Indonesia), Glob Chang Biol, doi: 10.1111/gcb.12979, 2015. 2015.

van Straaten, O., Corre, M. D., Wolf, K., Tchienkoua, M., Cuellar, E., Matthews, R. B., and Veldkamp, E.: Conversion of lowland tropical forests to tree cash crop plantations loses up to one-half of stored soil organic carbon, Proc Natl Acad Sci U S A, 112, 9956-9960, 2015.