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Interactive comment on "Seasonal variability in methane and nitrous oxide fluxes from tropical peatlands in the Western Amazon basin" *by* Yit Arn Teh et al.

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10. The investigations by Teh et al. on CH4 and N2O emissions from tropical peatlands are recently of major interest. Particularly for the study region in the Amazon basin, knowledge on magnitude, pattern and controls of greenhouse gas fluxes is scarce. As this region is a potentially huge source of CH4, it is important to close this knowledge gap. This study could contribute to this process.

Authors' response: The authors would like to thank the referee for his/her kind and very supportive remarks.

11. The findings of large CH4 and negligible N2O emissions might have been antic-

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ipated while the asynchronous CH4 fluxes with higher fluxes during the dry season for two of the four investigated vegetation types might not. Potential explanations for this result are discussed. However, there is plenty of literature on mechanisms and controls of peatland CH4 and N2O fluxes but appropriate references are missing in the introduction as well as in the discussion part. For example, it has been reported that CH4 fluxes do not increase or even decline when sites are inundating and that highest emissions occur for water levels close to the surface (e.g. Couwenberg et al., 2011, Hydrobiologia 674, 67-89).

Authors' response: As discussed in our response to the first referee (see point 2), we will revise the text to include a more through discussion of the underlying controls on CH4 and N2O flux. We also thank the referee for the suggested reference, and will incorporate the findings from this work into the new version of the manuscript.

12. Furthermore, I strongly recommend to revise the abstract and the presentation of results. The abstract mainly lists the results but doesn't tell anything about motivation, objective and main conclusions of the study. The two figures are of poor quality and Fig. 2 is not very helpful for interpretation of results due to the huge amounts of outliers. I would recommend to rather show columns with standard errors or maxima/minima. And why was the data shown in the figure grouped for vegetation type but not for different seasons? Also, figures that visualize the relationships between GHG fluxes and the measured environmental variables would be interesting. Although the relationships might be very weak, this would give the reader a better idea of the dataset.

Authors' response: Thank you for these suggestions. We will revise the abstract and figure 2 along the lines suggested here. With respect to seasonal trends, we made the decision to show this information in Table 2 rather than as a figure showing seasonal differences. Finally, with respect to the relationship between GHG fluxes and environmental variables (e.g. scatterplots of environmental variables versus gas flux), we can incorporate some of this information into appendices or as online supplementary materials for the revised version of the text.

13. P4, L60: Peatlands are not necessarily peat-forming. In contrast, many peatlands have been drained for utilization purposes which turns them into significant C sources and in regions like Central Europe, only a few percent of peatlands are still in a natural condition and thus peat-forming. Please be more specific about the state of peatlands in the study region.

Authors' response: The peatlands in the PMFB are unmanaged and have not been affected by human disturbance, unlike their counterparts in SE Asia.

14. P4, L61-65: Are you talking about peatlands in general or about peatlands in the Amazon basin? If you mean peatlands in general, your statements are not correct as there are several studies on peatland GHG emissions but most of them were conducted in the boreal or temperate zone.

Authors' response: We were referring to peatlands in the Amazon basin; the focus of past research in the region has been on mineral soil wetlands.

15. P5, L90: I wouldn't classify a peat depth of 3.9 m as shallow. Following the international definition, peatlands are defined by a minimum of 20 cm of peat deposit, which could be classified as shallow. It seems that peat deposits in the study region are all very deep.

Authors' response: In the revised version of this text, we will correct the language so as to reflect this point.

16. P7, L127-128: You do not mention the months of September and October. Are they neither wet season nor dry season?

Authors' response: September and October represent a transitional period between the wet and dry seasons.

17. P7, L135: "Pure peat" is not a soil classification! Please use World reference base (2015) to classify your sites. Qualifiers should be used to characterize the soils more precisely.

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Authors' response: The referee's concern is duly noted and the revised version of the text will be changed according to the referee's suggestion.

18. P7, L136: The pH values given are not in line with Table 2. Are these values from preliminary measurements?

Authors' response: The values reported on line 136 represent the range of values observed in prior studies, whereas the values reported in Table 2 reflect more specific data from our study plots. We will make this clear in the revised manuscript.

19. P8, L150: The number of plots remains unclear to me. You mention 229 plots but the numbers below summarize to 148 plots and in Table 1 you list 161 plots. Please clarify. Authors' response: The total number of plots should be 161, in accordance with Table 1. The text will be corrected accordingly.

20. P9, L177: Please clarify the procedure of the measurement campaigns. How long were these campaigns, did you sample each plot only once, several times per day or also on different days?

Authors' response: The duration of the campaign for each study site varied depending on its size. Each study site was generally sampled only once for each campaign.

21. P9, L178: What about the wet season in 2013? Why didn't you measure the fluxes during that period?

Authors' response: Due to personal circumstances, we were unable to collect data during the 2013 wet season.

22. P9, L186: How is it possible to place the chamber in a distance of 2 m? I cannot imagine how this practically works. And what about the sampling procedure? You have to get quite close to the chambers for that. Please clarify.

Authors' response: As discussed in our response to the first referee (see point 4), this was achieved by using a 2-m long pole to lower the flux chambers onto the water or

saturated soil. Gas samples were collected with syringes using >2 m lengths of Tygon® tubing, after thoroughly purging the dead volumes in the sample lines. If this paper is accepted for publication, we will revise the methods to provide these additional details on chamber placement and sampling technique.

23. P10, L199: Does it mean that fluxes were calculated from linear or non-linear regressions depending on the individual concentration trend against time? It is important to clarify this as linear regression can lead to substantial underestimation of fluxes as a consequence of decreasing concentration gradients over time. And which quality criteria have been used to ensure the reliability of computed fluxes?

Authors' response: The referee is correct; the manuscript will be revised to clarify this point.

24. P13, L268: The paragraphs for the results of different variables always have the same wording, which gives a quite uninspired impression.

Authors' response: We strove for clarity of expression in this section of the text, and believe that this reporting format achieves this goal.

25. P14, L281: I don't find it very meaningful to do statistics on measurements of air temperature. Also, you would have to compare exactly the same periods, otherwise the results could be misleading.

Authors' response: Air temperature measurements can be useful because they provide an indication of overall climatic variability, not only between seasons but among ecosystems, which may have different amounts of canopy closure. We have therefore provided this information to provide the reader a sense of overall patterns in climate variability among study sites. However, if the referee strongly objects to incluson of these data, we can remove it from the revised version of the manuscript.

26. P16, L325: Several different statistical tests were applied but not mentioned in the statistical analyses section.

Authors' response: We did not specifically mention the Wilcoxon signed-rank test in the statistics section; however, we did indicate that non-parametric tests were used under certain circumstances (see line 222). The text will be revised to provide specific detail on what non-parametric tests were employed.

27. P19, L398: It has to be considered that conclusions on that can only be drawn when annual CH4 budgets can be estimated from regular or automatic measurements in high temporal resolution.

Authors' response: The sentence referred to by the referee includes a qualifier (i.e. "may be") to denote that we believe that it is highly likely that this region is an important regional contributor to CH4 flux, but do not necessarily claim that this is entirely proven. Although we agree with the referee that regular or higher frequency measurements would be required to reach a more definitive conclusion, we believe that the weight of evidence supports our qualitative claim, including findings not only from this paper but from regional atmospheric sampling studies (Wilson et al., 2016).

28. P20, L 416: The water tables of the studied sites do in my opinion not allow the definition of oxic conditions as the water tables reported were quite high even in the dry season. Particularly non-degraded peat has a high water holding capacity, thus also when the water table drops below the soil surface, water filled pore space remains high in the top soil, still preventing CH4 oxidation.

Authors' response: The sentence referred to by the referee includes a qualifier (i.e. "more") to indicate that we are comparing oxygen availability during the wet and dry season, rather than making a statement about absolute oxygen content. The data provided in Table 2 demonstrate that most of the vegetation types, with the exception of Mixed Palm Swamp, show elevated dissolved oxygen levels during the dry season, supporting the idea that the soils contained more oxygen than during the wet season.

29. P21, L 438: No references are given in this section. The weak relationship is probably a result of the overall high water levels as the general assumption that CH4

emissions increase with water level is based on measurements from sites with huge drainage gradients. Generally, CH4 emissions increase exponentially when the water level passes a threshold of roughly 20 – 30 cm below ground. For water levels close to or above the surface, CH4 fluxes are often extremely variable. Some references would be very helpful here for the interpretation of your results. Also, methodological issues should be discussed as CH4 emissions mainly occur in terms of erratic ebullition when water tables are above the soil surface. This might be difficult to detect with small chambers during a short period of enclosure.

Authors' response: We will include additional references to this section in order to ensure that our statements are more clearly supported by prior research (Couwenberg et al., 2010;Couwenberg et al., 2011). Moreover, we will revise the text to include greater discussion about ebullition, in-line with the first referee's remarks.

30. P23, L479: Where negative CH4 fluxes also measured for water tables above ground? This would be rather unreliable in my opinion as one would not expect CH4 uptake in water saturated soil or even open water. Also, I assume that there should be more recent literature on CH4 exchange in tropical peatlands.

Authors' response: No negative CH4 fluxes were observed when water tables were above the soil surface, only when water tables were below the soil surface.

31. P24, L505-508: It is for me very unlikely that the different ecosystems do not differ in N availability. This conclusion cannot be drawn from equally low N2O emissions as there are probably other reasons for negligible N2O fluxes also in the nutrient-rich ecosystems. For example, there might be a higher N uptake by productive plant species at the nutrient-rich sites, competing with the potentially N2O producing microbes or N compounds are completely reduced to N2 during denitrification because of permanently anoxic conditions. Technical corrections P2, L9: The numbering of sections starts with 2.

Authors' response: We do not disagree with the referee; we simply indicated that this

may be one possible explanation for the trends in our data.

32. P5, L99: Replace “positive water tables” by “high water tables” or “water tables above ground”.

Authors’ response: This editorial suggestion will be taken in the revised version of the text.

33. P10, L197: I assume that the instrumental precision was > 95 % or the instrumental noise was < 5 %.

Authors’ response: Yes.

34. P10, L210: In which height was the air temperature measured?

Authors’ response: Approximately 1.3 m above the surface.

35. P10, L211: Please add manufacturer.

Authors’ response: This editorial suggestion will be taken in the revised version of the text.

36. P13, L270: add “different” after “significantly”.

Authors’ response: This editorial suggestion will be taken in the revised version of the text.

37. P14, L282: “Soil temperature” has to be replaced by “air temperature”

Authors’ response: This editorial suggestion will be taken in the revised version of the text.

38. P15, L314: Add “during” before “the dry season”.

Authors’ response: This editorial suggestion will be taken in the revised version of the text.

39. P18, L364: Plural: relationships

Authors' response: This editorial suggestion will be taken in the revised version of the text.

40. P18, L372: Results should not be interpreted in the "Results" section.

Authors' response: The text will be revised to take into account the referee's concern.

41. P18, L382: Add "electrical" before "conductivity".

Authors' response: This editorial suggestion will be taken in the revised version of the text.

42. P18, L383: Why do you mention CO2 here? No results on CO2 were shown.

Authors' response: Reference to CO2 will be removed in the revised version of the text.

43. P19, L394: Please round up to 1510.

Authors' response: This editorial suggestion will be taken in the revised version of the text.

44. P25, L526: Replace "these data" by "our data".

Authors' response: This editorial suggestion will be taken in the revised version of the text.

REFERENCES Couwenberg, J., Dommain, R., and Joosten, H.: Greenhouse gas fluxes from tropical peatlands in south-east Asia, Global Change Biology, 16, 1715-1732, 10.1111/j.1365-2486.2009.02016.x, 2010. Couwenberg, J., Thiele, A., Tanneberger, F., Augustin, J., Bärisch, S., Dubovik, D., Liashchynskaya, N., Michaelis, D., Minke, M., Skuratovich, A., and Joosten, H.: Assessing greenhouse gas emissions from peatlands using vegetation as a proxy, Hydrobiologia, 674, 67-89, 10.1007/s10750-011-0729-x, 2011. Wilson, C., Gloor, M., Gatti, L. V., Miller, J. B., Monks, S. A., McNorton, J., Bloom, A. A., Basso, L. S., and Chipperfield, M. P.: Contribution of regional sources to atmospheric methane over the Amazon Basin in

2010 and 2011, Global Biogeochem. Cycles, 30, 400–420, 10.1002/2015GB005300, 2016.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2017-48/bg-2017-48-AC2-supplement.pdf

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