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Biogeosciences Discussions

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.

Anonymous Referee #2

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General comments

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. The findings of large CH4 and negligible N2O emissions might have been anticipated 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).

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.

Specific comments

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.

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.

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.

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

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.

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

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.

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?

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

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.

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?

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

P14, L281: I don’t find it very meaningful to do statistics on measurements of air tem-

perature. Also, you would have to compare exactly the same periods, otherwise the results could be misleading.

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

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.

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.

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.

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.

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.

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

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

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

P10, L211: Please add manufacturer.

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

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

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

P18, L364: Plural: relationships

P18, L372: Results should not be interpreted in the “Results” section.

P18, L382: Add “electrical” before “conductivity”.

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

P19, L394: Please round up to 1510.

P25, L526: Replace “these data” by “our data”.

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