

# ***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 #1**

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Although the number of studies on trace gas fluxes from lowland tropical peatlands are steadily increasing, measurements are completely absent from some important large peatland areas. The peats in the foreland basin of Pastaza Marañon (PMFB) in Peru is an example of an important tropical peatland area from which we have no empirical studies on CH<sub>4</sub> and N<sub>2</sub>O fluxes. As such, this study is timely and novel. The authors conducted chamber based CH<sub>4</sub> and N<sub>2</sub>O fluxes stratified according to the four dominant vegetation types ranging from nutrient rich floodplains to nutrient poor bogs. While the spatial resolution of the sampling is good (see Table 1) the temporal resolution is restricted to four time sampling over a period of two years (sampling twice during the wet season and twice during a dry season). However, given the difficult

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accessibility and the fact that this is the first report on fluxes from this important peat area this is acceptable for this initial study. Furthermore, we do learn important controls since supporting variables were measured as well. Nevertheless if have several point that should be addressed before the manuscript can be accepted for publication.

-In your introduction, I miss a section on the known controls of CH<sub>4</sub> and N<sub>2</sub>O fluxes in peatlands. I think it is important to include this, since it is otherwise unclear why you measured the environmental variables that you did. Are there some potentially important factors that you did not measure and if so, why was that the case?

-In l.142 and l.147 you mention soil Ca, it remains a mystery in which form this Ca occurs. Please provide more details, is Ca an important environmental control on CH<sub>4</sub> or N<sub>2</sub>O fluxes?

-You describe your chambers as floating static chambers (l. 179). You furthermore write that chamber were placed on the soil surface from a distance of no closer than 2m to reduce the risk of ebullition (l. 186). For me it is hard to believe that ebullition was completely excluded and I also cannot understand how you can place these chambers at a distance of 2 m? Nobody has arms that long (at least I don't), so how was this done in detail? And, especially, how did you take the gas samples from your chambers using syringes without causing ebullition?

-Ebullition is relatively easy to detect e.g. if you start with very high concentrations or if you detect abrupt increases in CH<sub>4</sub> concentration. Can you give us information on how you dealt with such data and how many of your chamber measurements were potentially affected by ebullition? Is there a correlation of flux strength and the potential occurrence of ebullition? You also mention in your discussion that you measured occasionally high CH<sub>4</sub> uptake values (l. 477). Is it possible that this was related to high CH<sub>4</sub> concentrations at the beginning of chamber closure, potentially caused by ebullition? Did you find a correlation between initial CH<sub>4</sub> concentration and calculated CH<sub>4</sub> uptake values? I was also wondering whether the contrasting seasonal emission

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patterns that you discuss (l. 417 and further) could be related to ebullition. As you see, I think you don't give use sufficient information about the potential occurrence of ebullition and you should clearly improve this.

-I have no problem with the fact that you measured negative N<sub>2</sub>O fluxes, since we see more and more evidence that globally this is quite an important process. However, some of the negative fluxes seem to be quite high in your figure 2. How do your N<sub>2</sub>O uptake rates compare to other published values and is it possible that this may also somehow related to ebullition? Also, here I would be interested if your negative values correlate with the initially measured concentration in your chambers. If ebullition played a role for negative CH<sub>4</sub> and N<sub>2</sub>O fluxes, you may expect that both strong CH<sub>4</sub> uptake and N<sub>2</sub>O uptake would occur in the same chamber measurement. Did you check this?

-You probably measured your fluxes at different time of the day. Did you find any diurnal pattern in GHG fluxes? If yes, what could explain these observed differences and how would this affect your estimate of emission strength?

-The potential role of CH<sub>4</sub> oxidation is remarkable absent from your discussion. Why is that the case? Do you think this is not important for the total measured CH<sub>4</sub> fluxes?

-The version of the map in Fig. 1 that I saw did not have a very good quality. Can you provide a map where the four vegetation types that you used for stratification are included? The impression of Fig. 1 is that the total peat area is larger than what can be seen in the map. Can you adjust Fig. 1 in such a way that we see all of (or most of) the PMFB peatland area?

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