

Referee report on “Seasonal variability in methane and nitrous oxide fluxes from tropical peatlands in the Western Amazon basin” by Y. A. Teh et al.

The revised manuscript by Teh et al. has included the most important comments and suggestions by the two referees. Thus, the quality of the manuscript was substantially improved and it should be appropriate for publication in *Biogeosciences* from my point of view.

However, I still have one concern regarding the process of “ebullition-driven CH₄ uptake”. You should not treat this as an independent process from ebullition as in l. 433 – 434 where you state that there was no evidence of ebullition but you measured ebullition-driven uptake. Logically, this doesn’t make sense as there can only be ebullition-driven uptake if there is ebullition. For the forested vegetation, no ebullition without a subsequent CH₄ uptake was measured. Doesn’t this simply mean that the CH₄ oxidation potential was very high at that site? This might be explained by the water levels, which were lowest for the forested site during the dry season, and the generally high CH₄ oxidation potentials of forest soils. These points could be pointed out more precisely.