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Interactive comment on "Methane paradox in tropical lakes? Sedimentary fluxes rather than water column production in oxic waters sustain methanotrophy and emissions to the atmosphere" *by* Cédric Morana et al.

Anonymous Referee #1

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In their manuscript, submitted to Biogeosciences, the authors present data collected on five African lakes with the goal to quantify methane production in oxic waters and the other relevant sources and sinks. Their conclusion is that methane produced in the anoxic sediment and released via diffusive flux or ebullitive fluxes is the primary source of methane to the surface water. They state that empirical models for pelagic methane production (PMP) do not account for tropical systems as they were developed from temperate and often meso- or oligotrophic lakes. Their work ultimately aims to assess the importance of surface water methane production in oxic conditions by determining

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the relevant sources – namely PMP via incubations/tracer experiments, diffusive and bubble methane release from the sediment, air-water transport and oxidation.

While I agree with the authors' ideas that trophic lakes need to be carefully considered and are likely not comparable to higher-latitude lakes, there are some areas I am unclear in within their manuscript. I find the paper hard to follow in several places, and the structure could be improved. I am not sure if combining the results and discussion is the best approach, as results are often buried in the text, and the logic becomes confusing to follow. Furthermore, the lakes are not always listed in the same order in the tables and figures, making it more cumbersome to compare separate results on the same lake. Finally the "mass balance" presented on Fig. 6 is strange, and hard to follow. In fact the balances do not close with the rates presented.

Finally, while the authors clearly did a vast amount of excellent work on these lakes, and presented very intriguing data, there needs to be a better assessment of the large uncertainty in the analysis and methodology, clearer presentation of the data, and locations where the samples were obtained (i.e. maps with sample locations are a must). Again, these are very interesting data, and the methane varies between the lakes in some still unexplained ways. Perhaps a closer comparison of the lakes and their properties in relation to methane concentrations, d13C signatures, etc. I list the individual (both minor and major) comments/questions below more-or-less in order they appear in the text.

- Environmental Setting: In general, please define where the samples were obtained. I think a map of each lake with the location would be extremely helpful. For example, there is no indication where profiles were obtained other than depth. Furthermore we are missing the locations of the sediment obtained for the sediment-water flux determinations. Finally, I think the oxygen profiles should be included on figures 1 and 2.

- Diffusive flux at air-water interface: This is always an area of controversy and uncer-

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tainty. I suggest utilizing several parameterizations, perhaps some more recent, to at least give a range. I could suggest e.g. (MacIntyre et al. 2010; Vachon et al. 2010), or additional/others. Furthermore, how close was the weather station to the lake sampling points.

- Ebullition flux: How were the locations selected for the bubble flux measurements with the funnels? What was the assumed %methane of the initial bubble gas as a significant portion of the bubble gas from shallow bubble release is N2 (Langenegger et al. 2019). Finally how were bubble sizes selected? I believe some literature values are available.

- CH4 flux across the sediment-water area: I like the method the authors' used here. The main issue I have here is that because the incubations were performed after the removal of oxygen, the flux rate they get may be on the very upper end. Perhaps this should be viewed as a potential maximum methane flux. By removing the oxygen, the methane oxidizing layer at the sed-water interface was removed resulting in artificially large fluxes. This has been shown in several instances – see e.g. (Damgaard et al. 1998; Liikanen and Martikainen 2003)(Liikanen Fig 2).

- CH4 oxidation rates: Did these incubations remain oxic throughout? I find the rates reported rather on the upper end of reported values. It would be useful to compare your measured oxidation rates with literature values – especially for tropical lakes.

- Sunlight inhibitory effect on methane oxidations: Here, I have the same question regarding oxygen concentrations in the bottles. Furthermore, the authors state that they "investigate the hypothetical inhibitory effect of dissolved O2 production [on methane oxidation] by phytoplankton" however this was never discussed again. Since O2 was not reported, could the effects they see with reduction of oxidation with increasing light exposure rather be related to oxygen concentrations? Finally, as I understand, serum bottles block considerable light from penetrating, how is this considered?

- Determination of pelagic methane production: Here I admit I am not an expert. The authors use DCMU to inhibit photosynthesis. However, is it not important that the

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methane oxidation is inhibited with methyl fluoride? In other words, with the high reported oxidation rates, how does the oxidation that occurs within the incubations accounted for? Finally how did you ensure that the samples remained oxic through the experiment?

- Mass balance (figure 6): I think the mass balances are slightly misleading. Firstly, L. George is missing a source of 6 mmol/m2/d to close the balance, while the oxidation rate is very to high in L. Nya to close the balance. At any rate, such a mass balance would need to be performed over the lake scale. However, given the very limited data such a mass balance would also have a large amount of uncertainty. I suggest putting this information into a table and be very detailed that these are point measurements over a very large lake and thus may not be representative of the overall conditions. Please list uncertainties in these estimates.

- As an example for the L. George on figure 6. In mmol/m2/d the sources are Sed + bubble + PMP which is 9 + ∼2 + 0.027 = ∼11. The losses are oxidation + atm = 5.5 + 0.13 = ∼5.5. If this is meant to be a mass balance, and assuming steady state, there is a missing source term of 5.5 mmol/m2/d.

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