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Interactive comment on "Sources and processes sustaining surface CO_2 and CH_4 fluxes in a tropical reservoir: the importance of water column metabolism" *by* Cynthia Soued and Yves T. Prairie

Anonymous Referee #3

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This study is novel as it explicitly parses out evasion of CO2 and CH4 (sourced horizontally, vertically, from internal metabolism, and from sediments) from different hydromorphologic parts of a reservoir and its inflows. The results are interesting as well, showing that the CO2 and CH4 fluxes fundamentally change along a "river to reservoir continuum" and that the overall reservoir budget can not be closed. I think this is all very interesting, however I have some concerns (not dissimilar from Referee #1) about the discussion not sufficiently exploring certain results and the title of the paper being misleading. Overall, this work falls under the scope of Biogeosciences and appears to be scientifically sound.

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Perhaps most importantly, the manuscripts' title and abstract arrive at (in my opinion) different conclusions than the conclusion of the manuscript. The manuscript's conclusion does not suggest that the primary finding is 'internal water metabolism remains a dominant driver' as stated in the abstract. Rather, I read the conclusion's primary finding as an "integrative portrait of the relative contribution of different sources to surface CO2 and CH4 fluxes in a permanently stratified reservoir including its transition zones (branches)." I agree with Referee #1 that the massive uncertainties of the metabolism budgets limit the authors' abilities to conclude much from the metabolism values (including its presence in the manuscript's title). I would further argue that this concluding statement is not presented as the main focus of the manuscript's results. The authors allude to the relative contributions of different gas sources in sections 3.3-3.6 but never actually present these relative values. As far as I can tell, they only report the raw fluxes. I suggest the authors focus their statements only on what is directly supported by the results presented in the manuscript, and/or make their presentation of results clearer. I might further add to the results/discussion to sufficiently explore what is being declared in the paper's title, abstract, and conclusion.

Finally, I suggest expanding what is briefly mentioned at line 390-391: relative contributions of sources and processes governing gas concentrations vary with hydromorphology. I think an expanded discussion pertaining to Figure 6, after adding CO2 to the figure (and contextualizing it with Figure 2), would help tremendously here, as the influence of the reservoir hydrodynamics could be explored more thoroughly. Similarly, the authors would benefit from engaging more with the existing literature on spatiotemporal variability in gas concentrations within large lakes/reservoirs (e.g. Chmiel et al 2020; Natchimuthu et al. 2017 as examples).

Following is a list of smaller considerations. Line numbers are in parentheses.

(25) There are many other citations that are relevant here, in addition to DelSontro et al. (2018), which also show inland waters are significant sources of greenhouse gases. I suggest a more thorough reference set. Also, 'surface inland waters' implies you are

also talking about rivers/streams. If so, you need river-specific references as well.

(113 & 119) 'Soued et Prairie' should be 'Soued and Prairie'

(123-124) The reference provided here (another biogeoscience paper by the authors) caused me great confusion because it suggests that the interpolated data analyzed in this manuscript is already published, despite the writing style of the methods suggesting the opposite. This needs to be clarified by the authors because if any of this data/methods are already published, I think that should be explicitly declared in this manuscript.

(Figure 1) I'm not sure I'm convinced that all notable inflow is coming from these two rivers, and this is likely influential when working at the scale of an individual reservoir. I might suggest adding hydrography to Fig 1, or something similar, to show that there aren't really other noteworthy streams/rivers flowing into the reservoir.

(126-128) Along with the previous comment, because you are assuming all inflows are only from these two rivers, reservoir Q could be underestimated (as far as mass balance is concerned). I suggest adding a brief clarifying statement if this is the case.

(136) 'form' should be 'from'

(134) Should specify you are referring to surface area rather than area

(Fig 2) The boxplots are never explained in the main text or caption. Please define these. Also, please include the number of data points composing these boxplots either in the figure or caption.

(Figure 3) There is no explanation of what Figure 3 is actually plotting until section 3.7, after much of the figure's results have been presented. I think this should be mentioned earlier in the manuscript to clarify what is being presented.

(Figure 3) Y-axes need values (i.e. the densities). X-axes need to be scaled uniformly for each gas. In its current form, it is very difficult to compare branch versus main

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basin. Similarity, please add subpanel labels and refer to the specific subplot the paper is currently discussing.

(220-221) I'm unfamiliar with this R package but just because you can swap the depth term for the mixed layer depth does not mean that the model is physically realistic for a lotic environment. For example, k600 is often associated with different physical processes in lakes versus rivers and thus modeled differently. This needs an explicit consideration in the manuscript, i.e. why is it ok to run a model built for lentic waters in a lotic environment?

(337-338) Isn't this result just a function of the metabolism uncertainty being so high that it fundamentally effects the aggregate budget ('T' in Figure 3)? Or am I misunderstanding something? This is in line with my earlier comments pertaining to drawing conclusions from these metabolism values.

(367) Do you mean 'hydrological continuum'? Also, it is worth nothing that Hotchkiss et al. (2015), which is cited here, is explicitly a study on the lentic hydrological continuum, and not reservoirs or lakes or any lotic waterbodies. I suggest a more appropo reference.

Chmiel, H. E., Hofmann, H., Sobek, S., Efremova, T., & Pasche, N. (2020). Where does the river end? Drivers of spatiotemporal variability in CO2 concentration and flux in the inflow area of a large boreal lake. Limnology and Oceanography, 65(6), 1161-1174.

Natchimuthu, S., Sundgren, I., Gålfalk, M., Klemedtsson, L., & Bastviken, D. (2017). Spatiotemporal variability of lake pCO2 and CO2 fluxes in a hemiboreal catchment. Journal of Geophysical Research: Biogeosciences, 122(1), 30-49.

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