Author's response to comments from anonymous referee #3

We would like to thank referee #3 for taking the time to provide constructive comments, essential to increase the clarity and quality of the manuscript. We have modified the manuscript accordingly as described here.

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 cannot 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.

We thank referee #3 for his recognition of the research novelty.

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.

This point was raised by the two reviewers, and following their feedback we do agree that the wording used in the interpretation of the results on metabolism and its relative importance were confusing and inadequate in some instances. In this regard, we have made major changes to the new version of the manuscript aiming at better presenting the role of metabolism and shifting the main message to the overall gas budgets. Changes were made throughout the manuscript as follows:

Title: "*Changing sources and processes sustaining surface CO_2 and CH_4 fluxes along a tropical river to reservoir system*"

Abstract (L.16 - 20): "*Water column metabolism exhibited wide amplitude and range for both gases, making it a highly variable component, but with a large potential to influence surface GHG budgets in either direction. Overall our results show that sources sustaining surface CO_2 and CH_4 fluxes vary spatially and between the two gases, with internal metabolism acting as a fluctuating but key modulator.*"

Results section 3.7.1 (L.356 - 360): "*Including the metabolism substantially shifts the mean of the CO_2 epilimnetic budget (sum of sources and sinks) to a negative value and drastically increases its uncertainty (Fig. 3a, b and Table S2), reflecting a potentially important but poorly resolved role of metabolism in the budget because of its variability. However, given that metabolism acts more likely as a CO_2 sink on average, our best assessment suggests that, vertical transport from deeper layers is the main source sustaining surface CO_2 out-flux in the main basin of Batang Ai*."

Discussion section 4.3 (L.475 - 478): "*When reported as mean areal rates, CH_4 metabolism ranged from net consumption to net production of CH_4 (-0.29 to 0.94 $mmol.m^{-2}.d^{-1}$), which reflects its potential in having a high impact, either positive or negative, on the epilimnetic CH_4 budget at the reservoir scale (Fig. 3d and Table S3).*"

Discussion section 4.4 (L.507 - 510): "*The combination of our results suggests that water column metabolism could be the dominant source of CH_4 in the main basin of Batang Ai, potentially sustaining up to 75 % of surface emissions in that reservoir section.*"

Conclusion section 5 (L.418 - 423): "*Nonetheless, the epilimnetic budgets of both gases presented a high sensitivity to water column metabolism. This result is likely representative of large systems with a high volume of water versus sediments, which is common for hydroelectric reservoirs. However, metabolic balances of CO_2 and CH_4 were extremely variable in space and time, switching from a net production to a net consumption of the gases, and leading to highly uncertain ecosystem-scale estimates, which emphasizes the key but unconstrained role of metabolism in the overall GHG budgets.*"

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).

We agree with the reviewer on expanding the literature on spatiotemporal variability, and thus included additional references in the text section 4.7 (Chmiel et al., 2020; Loken et al., 2019; Lupon et al., 2019; Natchimuthu et al., 2017; Paranaíba et al., 2018; Rasilo et al., 2017). We also expanded the discussion on the relative contribution of sources along the hydrological continuum by editing the last paragraph of section 4.1 (L.413 - 427):

"*The changing relative contribution of sources and processes shaping surface CO_2 and CH_4 concentrations varies with the system hydro-morphology, from the inflows to the main reservoir basin, and lead to a progressive decoupling between the two gases along the continuum (Fig. S2). The observed CO_2 and CH_4 coupling in the inflows and branches is associated to a common catchment source, as previously reported in other systems including soil-water (Lupon et al., 2019), streams (Rasilo et al., 2017), and lake and reservoir inflow areas (Loken et al., 2019; Paranaíba et al., 2018). Indeed, horizontal inputs are the main source of both CO_2 and CH_4 in the upstream reaches of Batang Ai, accounting on average for 91 and 92 % of their respective surface out-flux in the branch section (Fig. 3a, c and Tables S2 and S3). However, when reaching the main basin, driving sources diverge between the two gases, with vertical inputs from the bottom layer supporting on average 60 % of CO_2 compared to 2 % of CH_4 fluxes, while sediment inputs sustained 7 versus 23 % of CO_2 and CH_4 fluxes respectively in that section. This decoupling partly results from the two gases having distinct metabolic pathways: mainly aerobic for CO_2 and anaerobic for CH_4, leading to their sources and sinks being spatially disconnected in the main basin. Consequently, sediments being a mostly anaerobic environment are a more important source of CH_4 relative to CO_2, while the metalimnetic layer being oxic-hypoxic acts as a sink of CH_4 and source of CO_2 via aerobic CH_4 oxidation (Fig. S4). Overall, the spatial patterns reported here highlight the hydrodynamic zonation common in reservoirs and its diverging effect on CO_2 versus CH_4 cycling*."

Concerning Figure 6, we would like to clarify that it represents CH_4 patterns in the main basin only, so it is not meant to explore the effect of hydrodynamic changes throughout the river to reservoir continuum. The aim of this figure is rather to explore evidences of CH_4 production in the lateral sediment versus the water column using distance to shore as a proxy for distance from lateral sediment as a potential CH_4 source. Since lateral sediment are not known as a large source of CO_2 we don't feel that including CO_2 in Figure 6 is appropriate, unless we have misunderstood the idea behind the suggestion of the referee here.

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.

Two references were included here (Bastviken et al., 2011; Raymond et al., 2013), including river systems (L.25).

- (113 & 119) ‘Soued et Prairie’ should be ‘Soued and Prairie’

  Fixed throughout the manusript
- (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.

  We apologize for the confusion. The data on surface gas fluxes is already published and reused in a different context here. This was clarified in the method section 2.3 by adding the sentence (L.115 - 116):

  “*Surface gas flux data used in this study are described Surface gas flux data used in this study are described in more details in Soued and Prairie (2020), a previous study on the C footprint of Batang Ai reservoir*.”
- (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.

  We agree with the referee that the two rivers represent a large part of the inflowing water but most likely not the entire mass of inflowing water. Unfortunately there is no available information on other potential inflows like smaller rivers or groundwater. However, to counter this problem when calculating horizontal inputs, we used total discharge (measured at the dam outflow) as a representative measure of total inflow to the reservoir (rather than using flow measurements from the two rivers). To clarify this, we edited the method section 2.4 as follows (L.124 - 133):

  “*In order to estimate the external horizontal inputs of CO_2 and CH_4, we considered that the total volume of water inflow and outflow (discharge measured at the dam) were equal, and equivalent to the mean of measured daily discharge (Q, in m^3 d^{-1}) during each campaign (considering minimal changes in inflow / outflow rates during a campaign). The approach of using discharge as a measure of total water inflow has the advantage of integrating all external flow (rivers, lateral soils, and groundwater) as water inputs to the reservoir. However, the fraction of inflow feeding the reservoir surface versus bottom layer, and its average gas concentration can only be approximated based on measurements from the two main river inlets (Fig. 1) due to the lack of data on other lateral inflows. Given that part of the inflowing water is colder and denser than the reservoir surface layer, only a fraction of it enters the epilimnion of the reservoir branches, and the rest plunges into the hypolimnion. We estimated that fraction (f_{epi}) based on temperature profiles in the East river delta and branch (sites P1 and P2, Fig. 1), and assumed it is representative of other water inflows to the reservoir.*”
- (136) ‘for m’ should be ‘from’

  Fixed (L.137)
- (134) Should specify you are referring to surface area rather than area

  Fixed (L.136)
- (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.

  The boxplot were actually formed by the points in the plots (each boxplot was composed by the 4 points representing each sampling campaigns), so since they were showing redundant information already represented by the points we decided to remove them.
- (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.

In the new manuscript version we added a concise method section 2.8 to clarify the purpose and calculations behind Figure 3 (L.240 - 247):

"***2.8 Epilimnetic GHG budgets***

Areal rates of horizontal, vertical, sediment, and metabolic inputs were combined into a sum of sources / sinks and compared to the rate of surface gas flux for each gas in each reservoir section. A mean and standard error were calculated for every component of the budgets based on measurements averaged across sites and / or sampling campaigns in order to obtain ecosystem-scale estimates of the components means and uncertainties. In the case of CO_2 metabolism, the ecosystem-scale average was calculated as the mean of the two average values derived from the incubation and diel O_2 monitoring methods. For every component, density curves were derived considering a normal distribution based on the mean and its standard error in order to visualize the relative magnitude and uncertainty of each ecosystem-scale areal rate (Fig. 3)."

- (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 basin. Similarity, please add subpanel labels and refer to the specific subplot the paper is currently discussing.
  Figure 3 was redesigned and X-axes scaled uniformly as suggested. Also, subpanel identification were added in all figures and referred to in the text. Concerning the Y-axis of Figure 3, it is not possible to add a common axis of densities since each normal distribution is on a separate horizontal axis. Though we believe showing densities might not be essential in this case since it would not offer substantial additional information needed to convey the message the Figure presents.
- (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?
  We understand the referee's questioning on this matter thus we added clarifying statement in the method section 2.7 (L.221 - 225):
  "*Note that even though the package used was originally developed for streams, it is easily transferable to lakes given that the model used (Eq. (8)) is generalized for all water bodies, with the parameter z_{epi} describing the depth of a mixed water column of either a lentic or lotic system, and with the K600 estimate relying only on data fitting to the model and not on system type.*"
  As mentioned, K_{600} estimates in the model are derived from maximum likelihood fitting of the data to the model rather being modeled by additional variables related to weather or hydrology, making the model independent of system type.
- (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.
  We agree with the referee that this statement is misleading and changed it to highlight the potential influence of metabolism rather than its definite role in the budget (L.356 - 360):
  "*Including the metabolism substantially shifts the mean of the CO_2 epilimnetic budget (sum of sources and sinks) to a negative value and drastically increases its uncertainty (Fig. 3a, b and Table S2), reflecting a potentially important but unresolved role of metabolism in the budget.*"
- (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.
  This was changed to hydrological continuum which is in fact a more appropriate word. We also added references associated to lotic systems while keeping the original reference deemed pertinent here since it addresses more explicitly the contribution of external versus internal CO_2 to surface flux. The sentence was edited as follows (L.386 - 389):
  "*All these results concord with the a progressively reduced influence of direct GHG catchment inputs and greater preponderance of internal processes along the hydrological flow continuum as observed in river*

networks (Hotchkiss et al., 2015) and in lakes and reservoirs (Chmiel et al. 2020; Loken et al., 2019; Paranaíba et al., 2018; Pasche et al., 2019)."