Author's response to comments from anonymous referee #1

We would like to thank the referee for his/her thorough examination of the manuscript and his/her constructive comments which were used to clarify and improve the manuscript as reported here.

This study is significant in that it attempts to determine the relative importance of various sources of CO2 and CH4 to evasive fluxes from a reservoir. The authors have identified four main sources (lotic inflow, hypolimnion, sediments, water column metabolism) and upscaled measurements and models of these fluxes to determine relative importance to the epilimnion. They find that there is a missing source of CO2 to both the branches and the main basin and a missing source of CH4 to the main basin. I think it's an interesting result that the model can't be closed, but it deserves more attention, perhaps in the title and the abstract. This work falls within the scope of Biogeosciences and generally scientifically sound, with a few exceptions.

We thank the referee for acknowledging the scope and significance of this research.

My main concern is with the handling of metabolism, and therefore the accuracy of the title and one of the key findings. The authors measure production of CO2 via aerobic metabolism with two methods: bottle incubations and single-station DO measurements. Additionally, the authors measure the metabolic production of CH4 with bottle incubations, which is an interesting and important aspect of this study. However, these metabolism measures are the most uncertain component of their model. The two methods for metabolic CO2 production disagree in sign and by an order of magnitude. And, the results for metabolic CH4 production are highly uncertain with the SE greater than the mean. I think all the authors can conclude is that their estimates for metabolism are highly uncertain and that metabolism has the potential to close the model because the other fluxes are relatively well constrained. Yet the title (Sources and processes sustaining surface CO2 and CH4 fluxes in a tropical reservoir: the importance of water column metabolism) makes it sound like the main finding this that metabolism is the most important source. Additionally, the abstract says, "internal water metabolism remains a dominant driver". I wish that the discussion of this missing sources (like in lines 455-459 of the discussion) was more straightforward in the title, abstract, and results section.

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.*"

Further, Figure 5, which I would interpret as aerobic metabolism not being a dominant control on CO2 dynamics, isn't even mentioned in the results section.

The deviation from 1:1 line in Figure 5 shows indeed that metabolism with a quotient of 1 is not the dominant force controlling CO_2 surface concentration which results from several possible factors as explained in the discussion section 4.2. Mentions of metabolism dominance were removed or edited (see previous comment) and Figure 5 was introduced in the result section 3.6.1 (L.330 - 333):

"*To complement metabolic rate data, surface O_2 and CO_2 departure from saturation were examined in both reservoir sections. O_2 oversaturation was observed in 44 % of cases in the main basin and 81 % in the branches (Fig. 5), which corresponds with the spatial patterns of net metabolic rates (Fig. 4b). CO_2 oversaturation was also widespread (74 % of cases), making many sampled sites oversaturated in both O_2 and CO_2 (55 % in the branches and 32 % in the main basin, Fig. 5).*"

I'm also unclear on which CO2 metabolism data is presented in Figure 3.

In the new manuscript version the caption of Figure 3 was edited to clarify the metabolism data presented, and we also 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)."

I'd still like to see a visual more clearly breaking down the relative importance of each source. For example, the abstract says that lotic inflows are responsible for 18%- 100% of CO2 and CH4 evasion from the branches. I'm having a hard time making that conclusion from the rest of the paper. For example, the SI table shows that on average 4.3 mmol m-2 d-1 of total CO2 flux from the branches (4.7 mmol m-2 d-) comes from inflows, which would mean that 91% of CO2 evasion is sourced from riverine inputs. If the authors are referring to individual samplings, then influx is between 204% and 18% of CO2 evasion. Fig 3 doesn't clarify things for me either because I find the color gradient confusing. Inflows are colored as 60% to 150+% of CO2 evasion, while evasion itself is colored as <-50% to >150% of evasive CO2 fluxes.

We agree that based on the previous version of Figure 3, the relative importance of each source was not clearly visually presented and the colored % axis was confusing given its large span (form negative to >100 %). Thus we redesigned Figure 3 which now clearly states the values of mean % contribution from each source. To avoid confusion, we marked <0% for negative areal rates instead of assigning them a negative percent contribution. Also, % contribution are now associated only to the mean of the normal distributions rather than considering the whole uncertainty range, which avoids having large ranges in % and focuses on the average contribution for each component. The range of 18-100% referred to the different sampling campaigns, but we agree that it is confusing and replaced it by the mean (>90%) in the corresponding abstract sentence (L.12 - 14):

"*Results showed that horizontal inputs are an important source of both CO_2 and CH_4 (> 90 % of surface emissions) in the upstream reservoir branches*."

Minor comments related to scientific content:

- Should terrestrial inputs (like soil water) be considered another source? Also, because sediment cores couldn't be taken in the littoral zone, it seems like there might be a missing source or two (terrestrial inputs, literal zone) from the model. I do appreciate that the authors discuss that their sediment fluxes might be higher than the average. Is it justified to assume that water inflow is equal to water outflow of the reservoir? While soil water inputs (and other lateral flow) were not measured, they were implicitly accounted for as horizontal inputs by considering a steady state where the total amount of inflowing water to the reservoir equals the outflow at the dam. While this assumption is likely a simplification of reality, it seems like the best approach given the limited available data on the hydrography of the system. However, the questions of the referee on this point are very pertinent, thus we edited the method section 2.4 to better explain the choice of this approach, its assumptions, and its limitations (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.*"
- I don't agree with the statement that the two methods for CO2 metabolism "match fairly well" (line 311).
  We agree with the referee that this is an incorrect formulation, we rephrased that sentence as follows (L.325 - 326):
  "*In the main basin, incubation results ranged from -8.8 to 7.2 μmol L^{-1} d^{-1}, while the diel O2 technique captured a wider variability in net CO_2 metabolic rates from -19.2 to 6.1 μmol L^{-1} d^{-1}*"
- Line 332 doesn't match the data presented in Table S2. Horizontal inputs are in general an order of magnitude greater than vertical inputs, not in the same range.
  The statement was removed.
- The finding presented in lines 399-400 of the discussion section is not presented in the results section.
  A sentence was added in the result section 3.6.1 (L.322 -323):
  "*Daily metabolic rates showed no correlation with mean daily rain or light (Kendall rank correlation p-value > 0.1).*"
  A statement was added in the method section 2.7 for the collection of light data (L.210 - 211):
  "*…along with light sensors (model HOBO Pendant from Onset).*"
- Lines 411-415 belong in the results section
  These sentences were edited and moved to the result section 3.6.1 (L.330 - 333):

"*To complement the metabolic rate data, surface O_2 and CO_2 departure from saturation were examined in both reservoir sections. O_2 oversaturation was observed in 44 % of cases in the main basin and 81 % in the branches (Fig. 5), which corresponds with the spatial patterns of net metabolic rates (Fig. 4b). CO_2 oversaturation was also widespread (74 % of cases), making many sampled sites oversaturated in both O_2 and CO_2 (55 % in the branches and 32 % in the main basin, Fig. 5).*"

The discussion section 4.2 was also edited accordingly (L.443 - 445):
"*Additionally, surface O_2 versus CO_2 concentrations shows that the departure of these gases from saturation varies widely around the expected 1:-1 line, with many surface samples oversaturated in both O_2 and CO_2, especially in the branches (Fig. 5).*"

- Figure 6 is not presented in the results section
  Description of the results in Figure 6 were added to section 3.2 (L.270 - 274):
  "*In the main basin surface CH_4 concentration significantly decreased with distance to shore in Nov-Dec 2016 (R^2_{adj} = 0.54, p-value < 0.001), but this correlation was weaker ($R^2_{adj} \leq 0.13$, p-value ≥ 0.03) during other sampling campaigns (Fig 6a). Surface $\delta^{13}CH_4$ values varied widely, between -83.3 and -47.6 ‰, but did not show a consistent spatial pattern (Fig. 2f) apart from a positive correlation with distance to shore in the main basin in Nov-Dec 2016 (R^2_{adj} = 0.29, p-value = 0.01, Fig. 6b).*"
- Figure S2 – linear regression lines shouldn't be drawn if the relationship is insignificant
  We understand the referee's point here, although from our perspective the regression lines in Figure S2 are not used to represent the significance of the regressions (based on an arbitrary threshold) but rather as a visual representation of the different slopes reflecting the structure of the data spatially. Thus, we argue for keeping the regression lines regardless of significance, but we are willing to reconsider if this is deemed problematic.

Additional line-by-line comments:

- Line 7 – the qualifier "two potent GHGs" should be directly after the mention of the gases
  Fixed (L.7)
- Line 10 – replace "processes" with "sources"
  Fixed (L.9)
- Line 35 – remove ", especially"
  Fixed (L.35)
- Line 47 – "associated to highly" should be "associated with highly"
  Fixed (L.45)
- Line 49 – I'm not sure that I agree with the idea that GPP and ER are often studied separately. The papers that I read tend to report both. Is there a citation you can use to back up this statement?
  The sentence was edited and the statement removed (L.47 - 49).
- Line 56 – "lakes" should be "lake"
  Fixed (L.55)
- Line 95 – "in 9 sites" should be "at nine sites"
  Fixed (L.94)
- Line 105 (and elsewhere) – "Soued et Prairie" should be "Soued and Prairie" or "Soued & Prairie"
  Fixed throughout the manuscript.
- Line 136 – "inputs form the" should be "inputs from the"
  Fixed (L.137)
- Line 166 – "6-cm-wide" liner
  Fixed (L.168)
- Line 255 – The placement of the per mille enrichment range is misleading . . . It currently reads as if the range is the δ13CO2 value
  Fixed (L.266)

- Line 258 – The R2 value doesn't match the information in the table
  The R^2 value refers to the linear regression in Figure S1 while Table S1 presents the Kendall correlation coefficient and is cited here for comparison of the link between CH_4 and parameters other than TN.
- Line 267 – The values don't match the tables
  One of the value was rounded at the first rather than the second decimal causing the mismatch, this was fixed (L.281).
- Line 272 – "were" should be "was"
  Fixed (L.286)
- Figure S3 – I would expect the legend to introduce the plots in order
  Fixed
- Line 367 – This citation only applies to CO2
  Citations associated to CH_4 and to lakes and reservoir systems were added (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).*"
- Line 382 – remove "surface"
  Fixed (L.405)