Author’s response to comments from anonymous referee #1

I think the authors did a good job of responding to most the reviewer comments. I especially appreciate that the authors reframed the title and abstract to match the key results.

We are grateful for the referee’s feedback which pushed us to substantially improve the manuscript.

One remaining concern is Figure 3. Some questions that are still coming up for me, as a reader, include:

-Why is a flux accounting for 100% surface flux the same tan color as fluxes that account for <10% of this metric? Maybe the color scale should be removed?

In light of this comment we changed the color scale to represent the direction of the rate (sink versus source) rather than the rate value, which should be visually more useful to the reader.

-In the figure legend, what does “The x-axes and color scales represent the areal rate of CO2/ CH4” mean?

This part of the figure legend was changed to avoid confusion:

“*The x-axes represent the areal rate of* CO_2 *or* CH_4 *and the color scale indicates the sign of the rate*”

This is not a deal breaker but perhaps still a suggestion that authors could use: In general, I find the discussion section well written and, as a reader, would appreciate the other sections to be written in this clear and concise style.

We did our best to make all sections of the manuscript clear and concise. We modified slightly the text throughout to improve the flow.

Other comments:

Line 138: form --> from

Fixed

Line 270: add comma after basin

Done

-The supplemental materials should be organized in order of reference in the text

This was fixed, Tables S2 and S3 were moved up right after Figure S2 to respect their order of appearance in the text.

-Some of the plot labels have typos (e.g., Figure 4 mmolO2, Figure S5 mmol.m-2.d-1).

We have now made the units labelling uniform.

-I also found the plot labels of CO2/CH4 confusing, initially interpreting it as a ratio.

To avoid confusion, “CO_2/CH_4” was replace by “CO_2 or CH_4” in the caption of Figure 3 and axis label of Figure S4.

Line 402: it seems like a negative relationship between [CO2] and temp could be due to solubility?

The referee raises a good point here, although, when accounting for solubility, the [CO2] – temperature relationship is still strongly significant (p-value < 0.001 and $R^2_{adj} = 0.19$).

Line 434-5: Are there any citations to back-up the statement that other studies have attributed variable metabolism rates to “thermocline stability regulating hypolimnetic water incursions to the epilimnion”?

A citation was added here (Coloso et al., 2011) Line 438

Author’s response to comments from anonymous referee #3

I thank the authors for a thorough and thoughtful response to my comments. The authors largely addressed my comments with responses that I support (particularly shifting the paper’s focus to the overall gas budgets).

We appreciate the referee’s acknowledgement of the work done and thank her / him for the pertinent and useful feedback.

I have two additional comments (mostly minor) left to address, still regarding the treatment of 1) the metabolism results and 2) a discussion on reservoir morphometry. Line numbers refer to the most recent version of the manuscript.

1) Thank you for clarifying and amending the text and title to reflect the uncertainty in the net metabolism results (namely Figure 3 and discussion pertaining to that). The % relative contributions added to Figure 3 are very helpful. However, I still struggle with the ‘% of mean total flux’ for M and T (i.e. the percents in Figure 3 for net metabolism and the sum of estimated sources). Given the uncertainty of metabolism (often greater than its mean in Table S2), I still disagree with presenting only results that include the mean of these density curves. I suggest presenting two values for M (and thus T): one including net metabolism and one that calculates metabolism by closing the mass-balance. Put another way, I would like to see the 75% value at line 509 presented for all Figure 3 subpanels and subsequently discussed in the manuscript in addition to the discussion at 501-510. I also encourage a brief discussion on how this high uncertainty implicates your argument at line 378-380: “In many studies, some components are only inferred by difference. While convenient from a mass-balance perspective, we argue that assessing all components together is necessary to clearly identify knowledge gaps as well as sources of uncertainty.” I agree with this statement in principal but the authors then never explicitly discuss how the very high uncertainties in their metabolism values force them to also present mass-balance results (i.e. line 509). I think you do a good job of noting this discrepancy, but you should add a few sentences explicitly engaging with this argument considering your results.

Following the suggestions of the referee, we modified Figure 3 to present the contribution of metabolism derived from a mass balance approach in addition to the empirical estimates. We also added several sentences in the discussion section 4.4 discussing the two approaches and how they complement each other:

“*Another way to decipher the role of metabolism, given its high uncertainty, is by difference in a mass balance exercise. Assuming mean estimates of all other components are accurate, CO_2 net metabolic rates would have to be equal to -0.8 and 2.1 mmol m^{-2} d^{-1} in the branches and main basin respectively for the mass balance to close. This corresponds to a contribution of -18 and 28*

% to the surface CO_2 flux in the respective sections (Fig. 3a, b), suggesting a substantial impact of metabolism on the CO_2 epilimnetic budget." (Lines 499 - 502)

"*This mass balance approach 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 (Fig. 3d). Even though this deductive approach is an indirect assessment of water column CH_4 metabolism, it emphasizes its likely key role in the reservoir epilimnetic CH_4 budget, while measured metabolic rates highlight the wide variability of this process and the need for more intensive research into its controls at spatial and temporal scales.*

The combination of empirical and mass balance approaches in this study provide not only a partitioning of the contribution of each source / sink in sustaining surface CO_2 and CH_4 fluxes, but also a clear picture of the uncertainties and challenges associated to the estimation of each component." (Lines 514 - 522)

2) I think your expansion on the spatiotemporal variability in gas concentrations at line 413 is great, however it is still not explicitly addressing the hydromorphology/morphometry of the reservoir. I think a study so squarely focused on relative changes from reservoir inflows to the main basin needs to comment on changes of reservoir morphometry, i.e. possible implications of changing reservoir volume, depth, shape, distance from horizontal inputs, etc. I understand you do not have the data to robustly analyze this, but some sort of literature-informed speculation is suggested. I think an expanded Figure 6 (as a separate Figure) could help parse out some influences here, though I understand that is not the focus of the Figure and do not think it is necessary (thank you for the clarification).

We elaborated the discussion on the impact of the main basin versus branches morphometries on horizontal inputs and other CO_2 and CH_4 sources:

"*The hydro-morphometry of these channels can explain the large impact of horizontal inputs in the branch section, which is characterized by a relatively small ratio of water to catchment area and a direct connection to the major river inflows creating a strong link between the catchment and the branches. However, when reaching the main basin, this link weakens due to a longer distance from river inflows and the dilution of horizontal inputs in a larger water volume. Thus, in the main basin, CO_2 and CH_4 are mostly driven by internal sources, diverging 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.*" (Lines 418 - 4265)