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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 #1

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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.

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Discussion paper

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

Minor comments related to scientific content âĂć 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. âĂć Is it justified to assume that water inflow is equal to water outflow of the reservoir? âĂć I don't agree with the statement that the two methods for CO2 metabolism "match fairly well" (line 311). âĂć 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. âĂć The finding presented in lines 399-400 of the discussion section is not presented in the results section. âĂć Lines 411-415 belong in the results section âĂć Figure 6 is not presented in the results section âĂć Figure S2 – linear regression lines shouldn't be drawn if the relationship is insignificant

Additional line-by-line comments Line 7 – the qualifier "two potent GHGs" should be directly after the mention of the gases Line 10 – replace "processes" with "sources" Line 35 – remove ", especially" Line 47 – "associated to highly" should be "associated with highly" 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? Line 56 – "lakes" should be "lake" Line 95 – "in 9 sites" should be "at nine sites" Line 105 (and elsewhere) – "Soued et Prairie" should be "Soued and Prairie" or "Soued & Prairie" Line 136 – "inputs form the" should be "inputs from the" Line 166 – "6-cm-wide" liner Line 255 – The placement of the per mille enrichment range is misleading. . . It currently reads as if the range is the δ13CO2 value Line 258 – The R2 value doesn't match the information in the table Line 267 – The values don't match the tables Line 272 – "were" should be "was" Figure S3 – I would expect the legend to introduce the plots in order Line 367 – This citation only applies to CO2 Line 382 – remove "surface"

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