Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-385-RC1, 2019

Interactive comment on "The carbon footprint of a Malaysian tropical reservoir: measured versus modeled estimates highlight the underestimated key role of downstream processes" *by* Cynthia Soued and Yves T. Prairie

Anonymous Referee #1

Received and published: 6 November 2019

The manuscript investigates GHG emissions from a reservoir in Sarawak, Borneo, Malaysia. GHG emission data from reservoirs in the tropics is scarce since the majority of studies are conducted in North America and Europe. The number of tropical and subtropical reservoirs is increasing due to the growth of the hydropower sector, hence the data presented in this manuscript is valuable and needed for better quantification of GHG emissions from hydropower.

The manuscript separates between four emission pathways: Surface diffusion, sediment ebullition, degassing at the turbines and downstream emissions. It presents a comprehensive measurement based estimate of the CO_2, CH_4 and N_2O emissions from this particular reservoir. Also, it compares the results to model based estimates, calculated for this reservoir.

The authors suggest parameter for model improvement, which broadens the impact of this study beyond the individual reservoir. However, the discussion about the relevance of this study for emission estimates from other reservoirs needs to be discussed in more detail. The discussion about the broadened impact is in my opinion needed for a publication in Biogeosciences.

While the authors address the relevance of reservoir soil type, rather than just latitude for model calculation, it should be further discussed how representative these low-carbon soils are for other reservoirs in Southeast Asia.

The manuscript has a good structure and all methods and equipment are well described, except the cases I address in the specific comments. The manuscript results and discussions are merged in one chapter, which in my opinion benefits the reading flow in this case.

General Comments:

1) The manuscript alternates between talking about C emissions and GHG emissions and to some degree treats those synonymously. Since N_2O is measured, while CO is never mentioned, I would suggest simply talking about GHG emissions in form of CO_2, CH_4 and N_2O.

2) It needs to be clarified whether CH_4 oxidation downstream is included in CO_2 emissions or not. From my understanding, it should be.

3) Uncertainties of values that are not listed in a table of the main manuscript should be included when mentioned in the text.

4) At many places, the formatting of variables and parameter units is not correctly done. For proper formatting, variables should be in italic type, while descriptive indices should

be in roman type (more important is to make sure the indice's types are consistent throughout the manuscript). Also, there should be only half spaces between values and units and no '.' between the units.

Specific comments:

Line 16: delete space in 2 639

Line 26-27: The sentence should be changed, since the flooded landscapes can be changed into GHG sources to the atmosphere, not the carbon balance.

Line 94: I would reconsider the choice of variable symbols in Eq (1). While it is unambiguously assigned, it is advisable not to use s, S, mV and V in the same equation.

Line 99: Why is the unit of k given? None of the other parameters are assigned specific units.

Line 110: The headspace fraction is very small. Did you recalculate the equilibrium concentration based on the volume of ambient/carbon free air? You should mention it, if you did. And why did you decide to use different headspace containers than for N2O?

Line 106: Up to this point, measurement of k_CH4 is not mentioned. I would recommend explaining it earlier in this paragraph rather than in the next one to avoid confusion.

Line 116: Figure1 –> Figure 1

Line 124: Units of EBD are not defined. Is it in meter?

Line 133: Why does the reservoir surface gas concentration upstream of the dam give an approximation of the natural baseline? I would consider concentrations in the reservoir inflows to be the better approach. Can you clarify the idea behind this decision? Also, you should consider splitting the whole paragraph into two. You try to convey a lot of information in it that left me confused after reading it the first time.

Maybe inclusion of the downstream sampling sites in Figure 1 could help clarify as well.

Line 156: How about degassing? If it is included in the downstream emissions, you must mention that. Though I would not advice the use of inconsistent definitions of the terms throughout the paper.

Line 169: Clarify whether you mean 13 m from the ground or at a depth of 13 m.

Line 173: Missing unit: (Secchi depth > 5 m).

Line 180: Figures 2 –> Figure 2 and: CO_2 fluxes were variable –> CO_2 fluxes varied

Line 249: This is incorrect. The absolute CH_4 fluxes were lower than the CO_2 fluxes. Most likely it is meant that CH_4 CO_2eq fluxes were higher than CO_2 fluxes.

Line 256: 2 639 –> 2639

Line 332: you mean withdrawal depth increase.

Line 480: In Table 1, uncertainties of the separate emission pathways are missing.

Line 485: You do not explain the meaning of the emphasis on River. Also, it should be clarified that those are the reservoir inlets rather than the downstream river. And the header formatting is off so it is not clear which values belong to which processes.

Line 490: I would appreciate inclusion of the stations in the downstream river. Maybe in a separate panel on a different scale.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-385, 2019.