Authors' response to anonymous referee #1

We would like to thank referee #1 for taking the time to do a constructive and thorough revision of the manuscript. Following the comments, we produced an improved new version of the manuscript that hopefully satisfy publication criteria.

The following color code is used to structure the text below:

- *Referee comment*
- Authors' response
- Changes to the manuscript

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.

We agree with the referee that discussing the broader impact of the results increases the value of the study. Hence, we expanded section 3.7 of the discussion (paragraphs 1, 2, and 5) to include more comparison of Batang Ai with other reservoirs, and discuss how weaknesses in the current modeling framework could affect a wider range of reservoirs based on similarities with Batang Ai (in terms of location, DOC concentration, soils properties, or stratification regime).

"*Despite high temperature, Batang Ai's very low water organic matter content (Table S1) offers little substrate for net heterotrophy, and its strong permanent stratification creates a physical barrier potentially retaining CO_2 derived from flooded biomass in the hypolimnion. The only three other sampled reservoirs in Southeast Asia (Nam Leuk and Nam Ngum in Lao PDR, and Palasari in Indonesia) also exhibited low organic C concentration (for reservoirs in Lao PDR) and low to negative average surface CO_2 diffusion despite their low latitude (Chanudet et al., 2011; Macklin et al., 2018). This suggests that, while additional data are needed, low CO_2 diffusion may be common in Southeast Asian reservoirs, and likely linked to low organic C content.*"

"*This may explain the unusually low DOC concentration in the reservoir and its inflows (0.3 to 1.8 mg L^{-1}, Table S1) that are among the lowest reported in freshwaters globally (Sobek et al., 2007). Clay-rich soils are ubiquitous in tropical landscapes (especially in Southeast Asia, Central America, and Central and Eastern Africa) (ISRIC - World Soil Information, 2019), however, their impact on global-scale patterns of aquatic DOC remains unknown. This may be due to a lack of aquatic DOC data, with the most recently published global study on the subject featuring only one tropical system and a heavy bias towards North America and Europe (Sobek et al., 2007). Exploring the global-scale picture of aquatic DOC and its link to watershed soils characteristics would be a big step forward in the modeling of reservoir CO_2 diffusion. Indeed, had the G-res model been able to capture the baseline emissions more correctly in Batang Ai (close to zero* *given the very low DOC inputs), predictions would have nearly matched observations. Finally, note that the G-res model is not suitable to predict CO_2 uptake, which was observed in 32 % of flux measurements in Batang Ai due to an occasionally net autotrophic surface metabolism favored under low C inputs (Bogard and del Giorgio, 2016). Improving this aspect of the model depends on the capacity to predict internal metabolism of aquatic systems at a global scale, which is currently lacking. Overall, reservoir CO_2 diffusion models may be less performant in certain regions, like Southeast Asia, due to an uneven spatial sampling distribution and a general lack of knowledge and data on C cycling in some parts of the world.*"

"*The model relies strongly on surface CH_4 patterns to predict excess CH_4 in the deep layer, which could explain why it underestimates CH_4 degassing in Batang Ai. Similar strong stratification patterns are ubiquitous in the tropics, with a recent study suggesting a large majority of tropical reservoirs are monomictic or oligomictic (Lehmusluoto et al., 1997; Scott Winton et al., 2019), hence more often stratified than temperate and boreal ones. This suggests that CH_4 degassing is potentially more frequently underestimated in low-latitude reservoirs. The G-res effort to predict CH_4 degassing is much needed given the importance of this pathway, and the next step would be to refine this model and develop predictions for other currently missing fluxes like CO_2 degassing and downstream emissions in the outflow. Our results suggest that improving latter aspects requires a better capacity to predict GHG accumulation in deep reservoirs layers across a wide range of stratification regimes.*"

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 second paragraph of section 3.7 (copied above) was modified to include a discussion about larger scale patterns of clay-rich soils and their potential effect on aquatic DOC concentration.

General comments

1) The manuscript alternates between talking about C emissions and GHG emissions and to some degree treats those synonymously. Since N2O is measured, while CO is never mentioned, I would suggest simply talking about GHG emissions in form of CO2, CH4 and N2O.

This was modified and 'GHG emissions' was used instead of C emissions throughout the text.

2) It needs to be clarified whether CH4 oxidation downstream is included in CO2 emissions or not. From my understanding, it should be.

This was clarified in the method section 2.5:

"*The amount of CH_4 oxidized to CO_2 along the 19 km of river stretch for each sampling campaign was calculated as the product of F_{ox} and $[CH_4]_{source}$. The resulting CO_2 produced by oxidation was included as part of CO_2 downstream emissions.*"

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

Uncertainties were added in the main text when absent.

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.

We adjusted the formatting of variables, indices, and units throughout the text following this comment.

Specific comments

Line 16: delete space in 2 639

Fixed.

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.

The sentence was changed accordingly:

"The flooding of terrestrial landscapes can transform them into significant greenhouse gas (GHG) sources to the atmosphere"

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.

Variables symbols in Eq. (1) were changed to avoid confusion:

"$$F_{\mathrm{gas}} = \frac{s\,V}{m\,A}\,, \qquad (1)$$

Where s is the gas accumulation rate in the chamber, $V = 25$ L the chamber volume, $A = 0.184$ m^2 the chamber surface area, and m the gas molar volume at current atmospheric pressure."

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

The unit was removed.

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?

The reason for choosing a small headspace ratio for N_2O compared to CO_2 and CH_4 is due to the low N_2O concentration of the sampled water, which is very close to ambient atmospheric concentration. Having a low headspace ratio results in a higher signal of water N_2O compared to ambient N_2O in the analyzed gas sample, which reduces the error when back-calculating the original water N_2O concentration. In the case CO_2 and CH_4 we used a different container with a higher headspace ratio because we needed a larger volume of gas samples to analyse both the concentration and the isotopic signature on two different machines. Since we did not need a large water volume for CO_2 and CH_4 samples, using 60mL syringes was more practical in the field than using the 1.12L bottle used for N_2O. We modified the paragraph to mention the recalculation of original water concentration:

"Three analytical replicates and a local sample of ambient air were taken at each site and analysed by gas chromatography using a Shimadzu GC-2040, with a Poropaq Q column to separate gases and an ECD detector calibrated with 0.3, 1, and 3 ppm of N_2O certified standard gas. After analysis the original N_2O concentration of the water was back-calculated based on the water temperature before and after shaking (for gas solubility), the ambient atmospheric pressure, the ratio of water to air in the sampling bottle, and the headspace N_2O concentration before shaking. k_{N2O} was derived from measured k_{CH4} values obtained by rearranging Eq. (2) for CH_4, with known values of F_{gas}, C_{gas}, and C_{eq}. The k_{CH4} to k_{N2O} transformation was done using the following Eq. (3) (Cole and Caraco, 1998; Ledwell, 1984)"

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.

This was done.

Line 116: Figure1 –> Figure 1

Fixed.

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

The unit is now defined.

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.

The idea behind considering the reservoir surface gas concentration to calculate downstream emissions was to simulate a system with surface water withdrawal (similar to a natural lake outflow). This concentration was used as a lower bound and combined with the outflow concentration 19 downstream of the dam as an upper bound to give a range of potential downstream emissions. Originally we did considered using the concentration in the reservoir inflows as baseline values, however, we decided against it because of the extreme variations in gas concentrations observed in the inflow sites both spatially and temporally (ranging from 0.13 to 14.82 µmol L^{-1} for CH_4 and from 12.56 to 113.68 µmol L^{-1} for CO_2). This wide range in concentrations makes it difficult to define a baseline out of it.

Following a comment from the second review we decided to simplify this calculation by using only the value 19 km downstream the dam as a baseline gas concentration for the outflow. While this approach may underestimate downstream fluxes (if there is still excess from the reservoir gas at the 19 km point), it should not represent a large error on the estimate. Indeed, Figure 3 shows an exponential decrease of gas concentrations in the outflow, and the value at 19 km seems close to a plateau. We reformulated the first paragraph of section 2.5 and split it in two like suggested, and we also discuss the potential underestimation of downstream emissions with the new approach:

“**2.5 Degassing, downstream emissions and CH_4 oxidation**

Degassing of CO_2 and CH_4 right after water discharge, and downstream emissions of the remaining reservoir-derived GHG in the outflow river were calculated using the following Eq. (5):

$$F_\mathrm{d} = Q\ (C_1 - C_2), \quad (5)$$

Where F_d is the flux to the atmosphere, Q is the water discharge, and C_1 and C_2 are the gas concentrations in the water before and after gas emission respectively. For degassing, C_1 was defined as the gas concentration upstream of the dam at the water withdrawal depth and C_2 as the gas concentration measured at the powerhouse right after water discharge. For downstream emissions, C_1 was considered as the gas concentration at the powerhouse (start of the outflow river) and C_2 as the gas concentration in the outflow 19 km downstream of the dam. In this case, we considered that, after a river stretch of 19 km, all excess gas originating from the reservoir was evaded and gas concentration was representative of the outflow river baseline. This assumption potentially underestimates actual downstream emissions (in case of remaining excess gas after 19 km), however, given the observed exponential decrease of gas concentration along the outflow (Figure 3), emissions after 19 km are expected to be minimal compared to those in the 0 to 19 km river stretch, consistent with observations in other reservoirs (Guérin et al., 2006; Kemenes et al., 2007).

Gas concentrations upstream and downstream of the dam were obtained by measuring, in each campaign, CO_2 and CH_4 concentrations in a vertical profile right upstream of the dam at a 1 to 3 m interval from 0 to 32 m, and at four locations in the outflow: at 0 (power house), 0.6, 2.7, and 19 km downstream of the dam (Figure 1). Sampling was done using a multi-parameter probe equipped with depth, oxygen, and temperature sensors (Yellow Spring Instruments, YSI model 600XLM-M) attached to a 12 Volt submersible Tornado pump (Proactive Environmental Products) for water collection. Gas concentration and $\delta^{13}C$ were measured as described in section 2.3. Water withdrawal depth ranged from 20 to 23 m and was estimated based on known values of elevations of water intake and water level compared to sea level. Gas concentration in the water exiting the reservoir was defined as the average measured gas concentrations in the ± 1 m range of the withdrawal depth."

We modified Figure 1 to include downstream sampling sites as suggested.

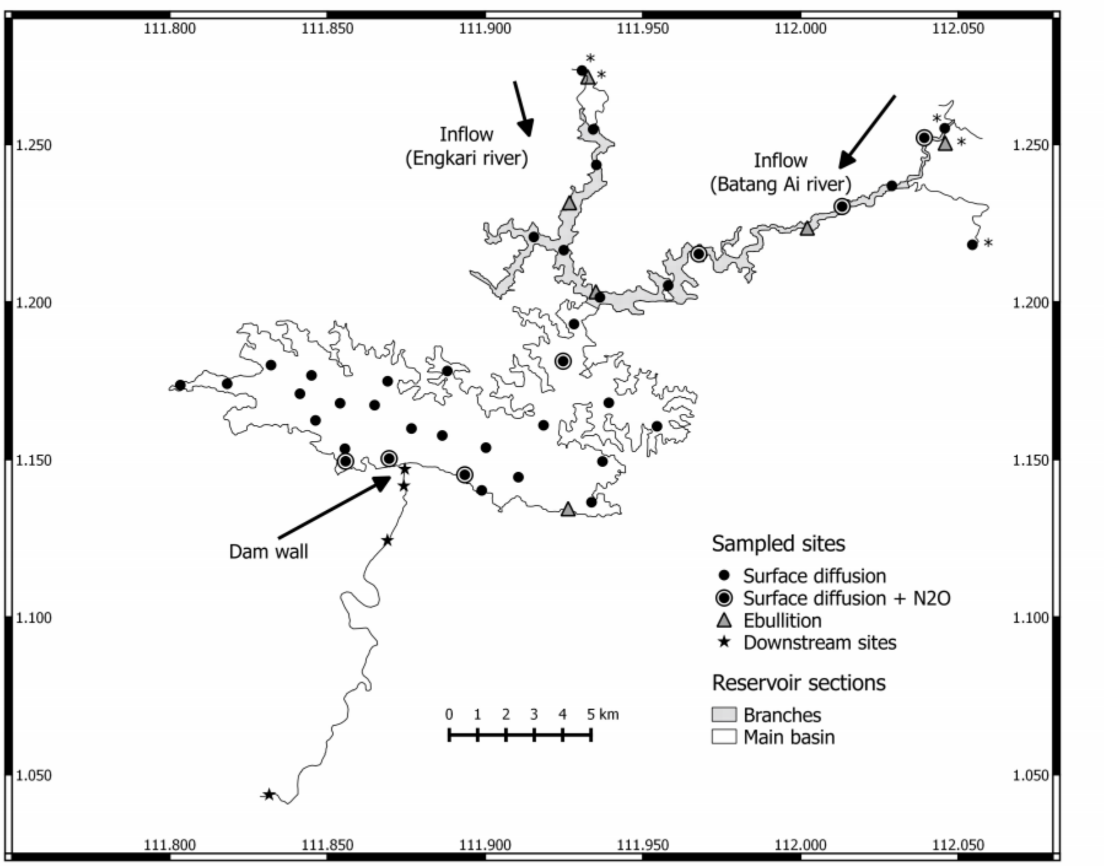

Figure 1: Map of Batang Ai showing the location of sampled sites and reservoir sections. * Represents the reservoir inflow sites.

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.

Degassing was added to the sentence:

"Batang Ai annual C footprint was calculated as the sum of surface diffusion, ebullition, degassing, and downstream emissions of CO_2, CH_4, and N_2O considering a greenhouse warming potential of 1, 34, and 298 respectively over a 100 years lifetime period (Myhre et al., 2013)."

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

This was clarified: "thermocline at a depth around 13 m"

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

Fixed.

Line 180: Figures 2 –> Figure 2 and: CO2 fluxes were variable –> CO2 fluxes varied.

Fixed.

Line 249: This is incorrect. The absolute CH4 fluxes were lower than the CO2 fluxes. Most likely it is meant that CH4 CO2eq fluxes were higher than CO2 fluxes.

Fluxes were indeed meant as CO2eq, this was clarified in the text:

"In all pathways, radiative potential of CH_4 fluxes were higher than CO_2"

Line 256: 2 639 –> 2639.

Fixed.

Line 332: you mean withdrawal depth increase.

Changed to "shallower water withdrawal" to avoid confusion.

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

Uncertainties were added in Table 1 as well as an additional column for downstream emissions for more clarity, and the title and headings were adjusted as follows:

Table 1: CO_2 and CH_4 dynamics downstream of the dam: gas export rate from upstream to downstream of the dam, degassing, result of CH_4 oxidation (CO_2 production and CH_4 consumption), downstream emissions, and total emissions to the atmosphere below the dam. Units are in mmol m^{-2} d^{-1} of reservoir surface area.

	GHG downstream of the dam (mmol m^{-2} d^{-1})				
	Exported	Degassed	Gain / loss. by oxidation	Downstream emiss.	Total emiss.
CO_2					
Nov-Dec 2016	40.62 (±2.27)	15.26 (±0.85)	0.90 (±0.13)	12.67 (±0.71)	27.93 (±1.56)

Apr-May 2017	37.80 (±2.11)	14.91 (±0.83)	0.59 (±0.08)	9.83 (±0.55)	24.70 (±1.38)
Feb-March 2018	37.98 (±2.12)	9.58 (±0.54)	1.80 (±0.26)	9.70 (±0.54)	19.30 (±1.08)
Aug 2018	38.07 (±2.13)	21.67 (±1.21)	0.38 (±0.05)	8.31 (±0.46)	30.00 (±1.68)

CH$_4$					
Nov-Dec 2016	14.84 (±2.10)	11.56 (±1.64)	0.90 (±0.13)	2.19 (±0.31)	13.76 (±1.95)
Apr-May 2017	7.32 (±1.04)	4.00 (±0.57)	0.59 (±0.08)	1.90 (±0.27)	5.90 (±0.84)
Feb-March 2018	12.47 (±1.77)	4.92 (±0.70)	1.80 (±0.26)	3.99 (±0.57)	8.91 (±1.26)
Aug 2018	10.71 (±1.52)	9.54 (±1.35)	0.38 (±0.05)	0.51 (±0.07)	10.05 (±1.42)

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.

Table 2 was reformatted to be clearer. The emphasis was explained and the meaning of “River” was clarified as representative of the inlets:

Table 2: Estimated reservoir and inflow areal and total GHG fluxes to the atmosphere (± standard error or 95 % confidence interval based on model standard error) from different pathways based on measured and modeled approaches.

	Diffusion			**Ebullition**	**Degassing**		**Downstream river**		**Total**
	CO_2	CH_4	N_2O	CH_4	CO_2	CH_4	CO_2	CH_4	
				Flux rate ($gCO_2eq\ m^{-2}\ yr^{-1}$)					
Reservoir									
Measured	113 (± 22)	153 (± 22)	-2.1 (± 4)	3.4 (± 1.9)	247 (± 14)	1342 (± 190)	163 (± 9)	456 (± 65)	2475 (± 327)
G-res model	577 (509 - 655)	161 (132 - 197)	NA	52 (32 - 83)	NA	468 (266 - 832)	NA	NA	1258 (1041 - 1636)
Barros et al. model	4671	176	NA	NA	NA	NA	NA	NA	4847
Inflows									
Measured	156 - 9538	248 - 22510	NA	10377 - 20498	0	0	0	0	10781 - 52546
				Total flux ($TgCO_2eq\ yr^{-1}$)					
Reservoir (meas.)	0.008	0.010	-0.0001	0.0002	0.017	0.092	0.011	0.031	0.169
River*	0 - 0.014	0 - 0.034	NA	0.016 - 0.031	0.000	0.000	0.000	0.000	0.016 - 0.08

*Represents the estimated pre-impounded river fluxes assuming they were similar to current fluxes from the reservoir inflows.

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

We modified Figure 1 to include downstream sampling sites as suggested.

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