

# Reply to reviewers

## Report #2

The manuscript has been significantly improved by the authors since the first submission, and I think it will be a highly valuable contribution to the research field. Based on my two major concerns on the initial submission I appreciate 1) the clarification of the different methods used and their different associated uncertainty (i.e. the new Table 1 and related text). It is now much more clear how/when/where the sampling was conducted. Still I miss that the authors do not transparently show the data including regression equations for how the two different sensor derived CH<sub>4</sub> data (both spatial mapping and continuously over time on Albis) are converted to “true” concentrations. Although conversion factors and estimated uncertainty are given in the text and table 1, to present the data behind those conversions (in the SI) would improve the strength even further. High-frequent dissolved CH<sub>4</sub> data for running water systems are very rare in the literature while such an addition would be very appreciated from the research community.

As outlined in the manuscript text (L 148ff):

„The range of concentrations from the water samples used for calibration was rather narrow (178 – 258 nmol/L), thus we used a conversion factor (water sample conc. / ppm from GGA) which was  $88.7 \pm 23$  nM / ppm.“

Thus, we cannot provide a regression line, but can only provide the basic data as shown in the little table for the SI now:

Date	Conc. From GC (nmol/l)	from GGA (ppm)	Conversion factor	Mean $\pm$ SD
19.08. 9:14	216	1.70	127.67	$88.7 \pm 23.1$
19.08. 9:49	183	2.37	77.18	
19.08. 11:04	178	2.10	84.88	
20.08. 7:37	258	3.83	67.29	
20.08. 11:22	214	3.23	66.17	
21.08. 7:20	191	2.24	85.19	
21.08. 9:06	243	2.16	112.19	

I also appreciate 2) that the authors have improved the clarity and strength of the conclusions section to better reflect the study. However, I think that the text flow in those additions could be improved, currently they are added a bit rough and not so well linked to each other. For example:

“Our results also show that diurnal pattern may differ between different habitat types. Light and temperature play different roles in shaping temporal variability of CO<sub>2</sub> emissions in different habitats. Although there was considerable variability of GHG concentrations in different aquatic habitats, spatial variability of k<sub>600</sub> in rivers cannot be ignored.”

We improved the text flow in the conclusions. It reads now: “Although we only provide a snapshot case study at a German river, we can derive a number of conclusions relevant for the quantification of GHG emission from large temperate rivers.

We show that short term temporal variability is both relevant and complex. It is now evident from several studies that day and night measurements are necessary to come up with realistic emission approaches. CO<sub>2</sub> probes are becoming more and more popular. Deploying them in numerous rivers

will improve global riverine CO<sub>2</sub> emissions estimates. Our results also show that diurnal pattern may differ between different habitat types. Light and temperature play different roles in shaping temporal variability of CO<sub>2</sub> emissions in different habitats.

We also show that spatial variability of CO<sub>2</sub> in different aquatic habitats can be considerable but is not the only factor leading to spatially variable fluxes. Also k<sub>600</sub> varied between habitats and spatial variability of k<sub>600</sub> in rivers cannot be ignored. This point becomes probably less relevant in larger rivers where the side habitat area is small compared to total river area. There is a need for more studies addressing spatial variability of k<sub>600</sub>.

We also show principle differences between aquatic and terrestrial GHG emissions both in terms of quantity and regulation. River sediments drying up at low discharge need to be considered at least for CO<sub>2</sub> budgets. However, when it comes to total GHG emissions, lower CH<sub>4</sub> fluxes compensate for higher CO<sub>2</sub> fluxes from dry sediments; this is a scenario already hypothesized for reservoir sediments (Marcé et al., 2019).

Finally, our data show that anthropogenic modification of the river (here: the construction of groynes) has the potential to alter GHG emissions significantly. In our case, the groyne fields nearly doubled CH<sub>4</sub> emissions from the river.”

## Report #3

Overall assessment of the revisions

The authors have revised the manuscript thoroughly and well-grounded and addressed most of my previous comments and the comments by the other reviewers reasonably well. I think that the manuscript will be a valuable contribution for the reasons I outlined earlier in my review. I just have a few minor comments following up on some revisions. My line numbers refer to the version with changes tracked.

Thank you for your careful reading.

Specific comments

Regarding my comment “I think there is a mismatch in gas fluxes and concentrations shown in Table 1 and in Figures 3/4...”: thank you for clarifying the difference in the data source between Table 1 and Figures 3/4. To avoid potential confusion among readers, I suggest to clarify this difference in the table / figure captions.

There is already “(data from Figure 3)” in the caption of table 2. We added “(same data as in table 2)” to the caption of Figure 4. We also added “Please note that time series data were measured independently from spatial data in table 2.” To the caption of Figure 5.

Regarding my comment “I would like to see more details on the statistical analyses used...” I am curious to see which base-R function you used for the linear mixed effects modelling. I am not aware of such a base-R function. I am only aware of functions in packages such as nlme or lme4. If package functions were used, they should be properly cited.

We used the lmer function, assuming that this is a base-R function. However, you are right that it is part of the lme4 package. We added the reference “Bates et al 2015). Thank you to pointing on this. We also consider proper citing of R packages important.

L. 226 I am not convinced about the “visual” check of autocorrelation, unless the authors can clarify this further and provide a supporting reference. To my understanding, it is recommended to formally

test if autocorrelation affects the outcome of linear mixed effects models (Zuur, A., Ieno, E. N., Walker, N., Saveliev, A. A., & Smith, G. M. (2009). Mixed effects models and extensions in ecology with R. New York: Springer.). This can be done by comparing models with and without accounting for autocorrelation, e.g. by using the likelihood ratio test.

Thank you for this advice. We performed a linear correlation analysis between light and temperature. We reformulated in the results: “Correlation with temperature or light (which were significantly linearly correlated, F-test  $p < 0.05$ ) including all data was not significant (F-test,  $p > 0.05$ )”. We did not perform a detailed analysis using likelihood ratio tests since we already used the AIC to detect the influence of the different drivers on model performance and the final model does only contain one driver.

Regarding my comment “L. 185-186 It is unclear to me why “Probe measurements of CO<sub>2</sub> and CH<sub>4</sub> concentrations were converted to fluxes using the measured gas transfer velocity of  $k_{600} = 5.5 \text{ m d}^{-1}$ ” I think it would be good to clarify in the text that  $k$  at the probe location was assumed to be the mean  $k_{600}$  measured in the side habitat.

OK. We added to the methods: “This assumes that  $k_{600}$  at the probe site was equal to the mean  $k_{600}$  measured in the side habitat.

L. 216 Shouldn't it be 5.2 instead of 5.5  $\text{m d}^{-1}$  (as shown in revised Table 2).

Yes – sorry. Typo corrected.

Figure 3: All three reviewers wondered why no map with CO<sub>2</sub> data was shown, which makes me think that many readers could be as confused as the reviewers about this decision. I understand and agree with the authors reasoning for not showing the CO<sub>2</sub> data. However, I would suggest to add a brief summary of this reasoning as note in the text / figure caption to avoid confusion.

OK. We added to the caption of Fig.3: “CO<sub>2</sub> was also measured by the GHG analyzer but data were not used because gas extraction was different for CH<sub>4</sub> and CO<sub>2</sub> and the system was optimized for CH<sub>4</sub>.”

L. 226-227 The sentence “If autocorrelation between driver variables was visually observed variables were chosen based on expert knowledge.” Does not make sense and needs to be rephrased.

We actually only choose variables based on the AIC as written in methods. We removed the sentence.

L. 257 The use of “Surprisingly” is ambiguous here, in my view. Is it surprising that oxic water samples produced methane, or that this production was low? Perhaps just delete “Surprisingly”.

We were surprised that the water did produce measurable amounts of CH<sub>4</sub> at all. We follow the reviewer and removed the “surprisingly” to avoid any confusion.

L. 304 I think the unit of par contains a typo and should not be divided by 2 ( $\mu\text{mol m}^{-2} \text{s}^{-1/2}$ )

No – this is not a typo. We divided the PAR data by 2 to fit them into the temperature scale of the figure.

L. 390-391 and L. 415 I don't agree with the statement that wind speeds were “rather constant”. Fig. S7 shows a pretty clear diel variability with highs during the day and calm periods during the night. I

agree that wind speeds were low overall as mentioned in L. 390 and I suggest to focus on the low speed and delete “rather constant”.

We agree and deleted “rather constant”.

Table S1 The Pearson coefficient should not be abbreviated with R<sup>2</sup>, but with R. Some numbers in the table are negative but a squared number such as R<sup>2</sup> cannot be negative.

That is true. The “minus” should indicate that we had a negative correlation. We added that information to the table caption.

Figure 5 The axis labels in the revised Figure 5 are now much smaller than before and very difficult to read. Please increase the font size.

we increased the font size

Technical notes

L. 302 space missing in “weremeasured”

corrected

L. 393 “/” should be “(“

corrected

L. 456 “sediemen” should be “sediment”

corrected