

Interactive comment on “Effect of sporadic destratification, seasonal overturn and artificial mixing on CH₄ emissions at the surface of a subtropical hydroelectric reservoir (Nam Theun 2 Reservoir, Lao PDR)” by F. Guérin et al.

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Received and published: 5 October 2015

All co-author thank the reviewer for its thorough review

1-REVIEWER : In general, the structure of the manuscript is well organized and flows in a logical order. However, some important facts are revealed later in the text, and therefore the manuscript would benefit from rearranging the text. For example, it is not clear for the reader that RES1-8 seem to behave rather similarly and RES9 is an exception. This could be solved by more clearly separating these two in the text, maybe

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even dividing them into their own chapters. Also, the reservoir should be described more precisely in 2.1. At least reservoir depth should be described and coordinates given.

ANSWER: The difference between RES1-8 and RES9 in terms of CH₄ concentrations will be stated more clearly than it is in the section 3.2 by adding: “The concentrations at RES9 were up to 10 times lower than the maximum bottom concentrations at the other stations for a given season. Since the station RES9 behaved differently from the other stations, results from this station will be treated separately.”. The two groups of stations are already described and discussed in separate sections in the sections 3.5 and 3.6 (Results) and 4.2 and 4.3 (Discussion) and already fulfil the reviewer’s requirements. The required reservoir characteristics were added. The reservoir description was not extended since all information necessary for the understanding of the article is included in the site description and the reservoir was described in details in several publications (Chanudet et al., 2015; Chanudet et al., 2014; Descloux et al., 2015; Deshmukh et al., 2015; Deshmukh et al., 2014; Descloux et al., 2014)

2-REVIEWER : Many topics are mentioned in the introduction in a way that suggests that the authors will return to these points. Therefore, it seems surprising that these themes are not discussed in the Discussion or Conclusions. In the first paragraph of the Introduction, it is mentioned that rivers downstream of dams and CH₄ ebullition are not considered in the estimates of CH₄ effluxes from hydroelectric reservoirs, and that these are a large source of discrepancy. And yet, only diffusive fluxes from the reservoir are considered in this manuscript. In the next paragraph, spatial heterogeneity of CH₄ emissions is attributed mostly to ebullition. Seems that this study contradicts that statement, but this is not clearly discussed.

ANSWER: We agree with the reviewer on the current lack of connections between the beginning of the introduction and the content of the manuscript. We added a few sentences to mention that ebullition and downstream emissions from the Nam Theun 2 Reservoir were quantified (see Deshmukh et al, 2014, Biogeosciences and Desh-

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mukh et al., 2015, Biogeosciences Discussion, companion paper) and that the current manuscript focus on emission by diffusive fluxes at the reservoir surface. The spatial and temporal variations at the stations RES1-8 and the temporal variations at the station RES9 are discussed in details in the sections 4.2 and 4.3, respectively and are also clearly stated in the conclusion. In order to summarize our results on the complex seasonal and spatial variations at the stations RES1-8, a few sentences were added in the section 4.2.

3-REVIEWER : Methods section needs some improvement. Were the measurements taken from a fixed platform? If not, was the boat anchored? The time of the day, or time range, should be given when the measurements were taken in general. It has been shown that gas fluxes depend on wind speed and heat flux (e.g. MacIntyre et al., 2010), and these vary along the course of the day. It can cause bias to the results if the measurements were taken always at the same time. This is not to say that the study should have been conducted some other way, as this approach is typical in these studies with manual sampling, but just that the reader is aware of this. The possible bias should also be discussed in the text.

ANSWER: The following sentence was added to the Sampling strategy section (2.2): All samples and in situ measurements were taken in the morning or early afternoon from an anchored boat. Most of the time, the boat is attached to buoy at the station. When no buoy is present, an anchor is used, with care in order not to resuspend surface sediments. As the sampling started from the surface, the bottom water was sampled almost an hour later and should not be influenced by the perturbation generated by the anchor. In contrast with the results of (MacIntyre et al., 2010), Sahlé et al (2014), we show in Deshmukh et al (2014, biogeosciences) at this site during several field campaigns between 2009 and 2011 that there was no enhancement of the diffusive fluxes (or a negligible enhancement) during continuous measurements of CH₄ emissions by eddy covariance. Only ebullition had a semi-diurnal pattern. We therefore believe that this potential bias is negligible in our case.

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Minor comments: 4-REVIEWER : Page 11354, line 18: “physico-chemical parameters” seem to refer only to temperature and dissolved oxygen. For making it easier for the reader to follow, I suggest to write “. . .the vertical profiles of temperature and dissolved oxygen in the water column. . .”

ANSWER: Physico-chemical is be replaced by temperature and oxygen concentration

5-REVIEWER : Page 11355, section 2.3.2. “Surface and deep-water samples for CH₄. . .”. I read this as only two samples of CH₄ concentration were taken, one from the surface and one from the bottom. However, e.g. in Fig. 2 many other sampling depths between these two are presented. Please clarify the sampling strategy more clearly.

ANSWER: In the revised version, this will be rewritten as follow: Surface samples were taken with a surface water sampler (Abril et al., 2007) and other samples from the water column were taken with a Uwitec water sampler.

6-REVIEWER : Page 11356, line 18. “...water and air CH₄ concentrations were applied...”. Previously, there has been no mention of measurements of atmospheric CH₄ concentrations. How were these obtained?

ANSWER: No atmospheric air was sampled. We used an average atmospheric concentration of 2 ppm, well in line with the concentration measured with the Los Gatos CH₄ analyzer we deployed for eddy covariance measurements. This was rewritten as follow: The CH₄ concentrations in water and the average CH₄ concentration in air (2 ppmv) obtained during eddy covariance deployments (Deshmukh et al., 2014) were applied in equation (1) to calculate diffusive flux.

7-REVIEWER : Page 11357, line 6-7. “For the determination of k₆₀₀, we used the formulations of. . . MacIntyre et al. (2010)”. Please specify which formulation was used. They present more than one in their article.

ANSWER: We used the equation (7) from Guérin et al (2007) which includes the combined effect of wind and rain on the gas transfer velocity. From MacIntyre et al. (2010),

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we used the average equation which included the dependency of k600 to wind speed whatever the buoyancy fluxes ($k600 = 2.25 U_{10} + 0.16$). This now specified in the revised version

8-REVIEWER : Page 11357, line 13. “. . .the boat drifted quickly. . .”. Which boat are the authors referring to? There is no mention of a boat before. Please describe more precisely how the measurements were conducted. Using word “station” leads the reader to think of a fixed mast or platform or such.

ANSWER: As mentioned in our answer to your second general comments, sampling was performed from a boat and this is stated in the manuscript. The word “station” is commonly used in limnology.

9-REVIEWER : Page 11357, line 19. “. . . and buoyancy flux from. . .”. How buoyancy flux was defined or calculated? There is no mention of measurements of heat budget components.

ANSWER: The buoyancy flux cannot be calculated with the dataset included in this manuscript. Therefore, saying that we calculated the fluxes taking into account was misleading since only one equation from MacIntyre et al (2010) was used whatever the heat fluxes. So the mention to buoyancy flux is removed.

10-REVIEWER : Page 11357, lines 21-22. “In the regulating dam where we observed the same vortexes as in RES9, . . .”. Please clarify what is meant with this sentence. By ‘same’ is meant ‘similar’? Is this based on visual observation?

ANSWER: The sentences: “The k600 was determined in the regulating dam (Deshmukh et al., 2015) located downstream of the turbine where we observed vortexes similar to those observed at RES9. In the regulating dam where we observed the same vortexes as in RES9, the k600 was 19cmh⁻¹ on average for 4 measurements” are rewritten as follow: “The k600 was determined in the regulating dam (Deshmukh et al., 2015) located downstream of the turbine where we visually observed vortexes

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similar to those observed at RES9. In the regulating dam, the k600 was 19 cm h⁻¹ on average for 4 measurements”

11-REVIEWER : Section 2.6. k is a critical component when calculating the fluxes. Some kind of error estimate should be provided when k is estimated from equations. It seems that the residence times are very short in this reservoir, giving reason to believe that there are significant currents. Gas transfer equations have no parameter for currents, even though they produce turbulence at the surface, as was noted also by the authors (page 11357, lines 14-17). For this reason, more justification would be in order to convince the reader that these equations can be used for this reservoir and for different parts of the reservoir.

ANSWER: The average residence time is 6 months ranging from 1.5 to 12 months as depicted in Figure 3 and the maximum water current velocity that was measured in the reservoir is 0.2 m s⁻¹ (Chanudet et al, 2012) as mentioned in the manuscript. Such water current velocities were only measured around the station RES9, anywhere else in the reservoir they were below 0.01 m s⁻¹. Therefore, the water current is unlikely to be a significant controlling factor of the k600 except at RES9 where it can increase it by a maximum of 2 cm h⁻¹ as mentioned in the manuscript. In addition, as mentioned in the section, TBL calculations were well in line with fluxes measured by floating chambers and eddy covariance (Deshmukh et al, 2014). We believe we already provided all justifications asked by the reviewer. However, the average water current velocity was added to the manuscript and the paragraph was improved by adding more details. We consider that the use of two different relationships for the k600 determination give a wide range of emissions and could be considered as the uncertainty of the fluxes.

12-REVIEWER : Section 2.8. There are no references and this is the first time I have seen this kind of approach to assess spatial and temporal variations of CH₄ concentrations and fluxes. Since this is not a standard procedure in limnological literature, more description might prove useful for other scientists to assess spatial and temporal variability of CH₄ in their studies.

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ANSWER: Based on Kruskal-Wallis and Mann-Whitney tests, no significant differences were found between the seasons and/or the stations. These test results were attributed to the very large range of surface concentrations due to the sporadic occurrence of extreme values (over 4 orders of magnitude). In order to reduce this range, the log of the concentrations was used. The resampling at a 15 days time-step was done for comparing time series with the same number of observations and avoiding issues related to oversampling. The main differences between the seasons and stations were the occurrence of fluxes higher than 5 mmol m⁻² d⁻¹. Therefore we used the frequency distribution and the skewness in order to discriminate the seasons and the stations. These two parameters and the correlation functions are common tools in statistical software. Based on the comments of Reviewer 1 and 2, the paragraph was rewritten as follow: "Since all tests indicated that the distribution of the data were neither normal nor lognormal at the stations RES1-8, Kruskal-Wallis and Mann-Whitney tests were performed with GraphPad Prism (GraphPad Software, Inc., v5.04). No significant differences were found between the seasons and/or the stations. These test results were attributed to the very large range of surface concentrations due to the sporadic occurrence of extreme values (over 4 orders of magnitude). In order to reduce this range, the log of the concentrations was used. For each station, the time series of the log of the CH₄ surface concentrations were linearly interpolated and re-sampled every 15 days in order to compare time series with the same number of observations. The log of the concentrations was used to determine the frequency distribution, the skewness of the dataset (third order moment), the auto-correlation of each time series and the correlation between the different stations. All analyses were performed using Matlab."

13-REVIEWER : Page 11360, lines 13-18. During WD and WW, the overall water column CH₄ concentrations seem to be rather high compared to other sampling sites, especially since the oxidation rate of CH₄ and k are estimated high at this location. Could the authors provide a reason or guess why the concentrations keep up so high?

ANSWER: The concentrations at RES9 from the surface to the bottom are always

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lower than the maximum concentration in the hypolimnion at other stations. This following sentence was added: "The concentrations at RES9 are up to 10 times lower than the maximum bottom concentrations at the other stations for a given season." 14-REVIEWER : Page 11361, lines 12-13. "In the dry year 2012, the reservoir bottom CH₄ concentration and storage was almost twice higher than in wet year 2011." Could the authors provide any explanation for this? This section is the result section and explanation requires taking into account aerobic oxidation, hydrology and water residence time so explanation are all given in the discussion. See from L23 P11365 to L7 P11364 of the submitted manuscript and the answer to the comment 16.

15-REVIEWER : Page 11362, lines 14-16. "The surface concentrations were not statistically different. . .". I read this so that the surface water CH₄ concentrations and fluxes varied independent of the season. However, there is, per visual observation, an evident pattern in both CH₄ concentrations and fluxes in Fig. S2. Also, later in Discussion, 4.1., the significance of stratification and overturn to gas concentrations and fluxes are described. Could the authors elaborate this paradox?

ANSWER: We agree that the baseline of the temporal evolution of diffusive fluxes and concentration depict a pattern with higher fluxes in the WD season. However, due to the occurrence of high fluxes and concentrations without clear seasonal patterns at all stations, there was no statistical difference between the seasons while using classical statistical tests as now explicitly mentioned in the MS (see answer to comment 12). The occurrence of extreme values precludes statistical tests to give the "expected results" based on visual observations of the graphs. In the section 4.1, there is a description of the seasonal dynamic in the water column, mostly based on bottom concentration and storage, not on the surface concentrations and fluxes. The surface concentrations and fluxes are described in the section 4.2 and in the figure 7 and it is said that high fluxes occur mostly in the WW season in the inflow zone and mostly in the CD in the rest of the reservoir.

16-REVIEWER : Page 11366, lines 6-7. "It therefore suggests that the residence time. .

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.". I think the authors have a nice idea here, but the statement is perhaps too simplified. The reason seems to be that higher water inflow and outflow rates (with appropriate characteristics, like colder T than in the reservoir) affect the stratification behavior in the reservoir, which results in changes in methane oxidation rate. Residence time itself gives no information of how the water body stratifies or not.

ANSWER: We agree with the reviewer that we should focus more on the destratification due to high water inputs. The text was modified as follow: "In wet years like 2011, the thermal stratification is weaker than in dry years since the warming of surface water is less efficient and the high water inputs alters the stability of the reservoir thermal stratification as shown by the sharper decrease and the larger range of ΔT in 2011 than in 2012 (Figure 3a). As a consequence, the oxygen diffusion to the hypolimnion was higher in 2011 than in 2012 (Figure 3b) and it enhanced aerobic methane oxidation by 20% in the water column in the WW season in 2011 as compared to 2012 (Figure 4). It therefore suggests that the hydrology affects both the thermal stratification and the hypolimnic storage of CH₄ in reservoirs, indirectly controls aerobic methane oxidation and ultimately emissions."

17-REVIEWER : Page 11366, lines 10-12. Could the authors provide a reason why sites RES1,3,7 and 8 were chosen? In general, the choice of which sites are discussed seems arbitrary.

ANSWER: These stations were not selected arbitrary. RES1 was chosen because of its highest skewness indicating that extreme events are more frequent at this station than at all other stations in the reservoir; they occur in both the CD and WW season. RES3 was chosen because overturn occurs mostly in the CD season during lake overturn. RES7 and RES8 were selected as they are located in the inflow zone with high and intermediate skewness, respectively. The following sentence was added: "These four stations were selected for their contrasting skewness (Figure S3) which gives an indication on the occurrence of extreme events and the facts that they are representative for all station characteristics (Table 1)."

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18-REVIEWER : Page 11366, lines 19-22. Could the authors clarify these lines. Do they suggest that during WD season at RES3,7 and 8 the reason for these high fluxes were overturn, as in CD season? What would be the cause for destratification during this season? Also, if there would be data available to validate these causes, it would be interesting to see.

ANSWER: Actually, these high emissions in the WD seasons were associated with early rains and associated high winds that occur sometimes in the last fifteen days of May. Due to the very high hypolimnic CH₄ concentrations at this period of the year, a sporadic destratification due to wind and rain enhance vertical transport of CH₄ toward the surface and diffusive fluxes. This was added in the manuscript.

19-REVIEWER : Page 11368, lines 24-25. "This design enhances. . .". This is a good finding. I would assume that it also increases lateral transport of hypolimnic waters, which in turn bring more CH₄ to the area of strong vertical mixing. Therefore, this spot has even larger spatial impact causing outgassing of CH₄ from large area.

ANSWER: It increases lateral and vertical transport and the concentration at this site is close to the average of the concentration in the whole reservoir. The physical modelling and the measurements of vertical and horizontal water current (Chanudet et al, 2012) show that this is restricted to an area of 3 km², as stated in the manuscript. Therefore, we are confident with the extension of the area under influence of the water intake.

20-REVIEWER : Page 11369, lines 20-24. The authors state that these hot moments only occur a few days in a year. On the same page, lines 26-27, they also say that based on fortnightly measurements, 1 month sampling frequency is sufficient. In my opinion, this conclusion needs more explanation. If this is based on sampling interval of 2 weeks, how the authors can be confident that a significant amount of these hot moments, lasting only few days, were not missed during the study? Especially, since the full CH₄ mass balance was not conducted and there are unclear components in CH₄ cycle, like possible lateral transport of CH₄ (page 11368, lines 6-11).

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ANSWER: We obviously cannot be 100% sure that no hot moment was missed, be sure that the peak of emissions was not missed and be sure on the duration of the sporadic events. However, we never observed extreme emissions lasting more than three consecutive samplings, which corresponds to a duration of 1.5-2 months at a single station as it is visible on Figure 7. The text was modified has follow: “The quantification of emissions thus requires the highest spatial and temporal resolutions in order to capture as many hot moments as possible. At a single station, extreme emission events never lasted more than 2 months (3 consecutive sampling dates) and probably lasted less than 15 days most of the time (Figure 7). The auto-correlation function of the concentration time series indicate that a minimum sampling frequency of 1 month is required in this monomictic reservoirs for an accurate description of the change in the surface concentrations and estimation of the emissions (Figure S1).”

21-REVIEWER : Page 11370, lines 8-10. “The high frequency. . .”. Seems quite bold to say that one measurement per two weeks is not discrete and that it is high frequency, when it has been shown that e.g. wind speed is a major driving force of gas exchange, and wind speed has ample variation in much shorter time scale than 2 weeks. I suggest to rephrase this sentence since this manuscript actually deals more with the seasonal methane fluxes and discrete sampling and not so much with the actual gas exchange dynamics and high frequency sampling.

ANSWER: Our fortnightly monitoring (over more than 3 years) is “high frequency” as compared to most of the studies on lakes and reservoirs, which are based on “seasonal sampling” (2-4 sampling per year). We removed any mention to high frequency and discrete sampling in the first sentence which was modified as follow:” The fortnightly monitoring of CH₄ diffusive emissions at nine stations revealed complex temporal and spatial variations that could hardly been characterized by seasonal sampling.”

22-REVIEWER : Figure 2. The panels and axis fonts are way too small. Maybe less measurement sites could be shown and the ones that are shown are larger?

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ANSWER: The size of the graphs and fonts was increased but all stations are kept

23-REVIEWER : Figure 3. (c) is missing.

ANSWER: Added

24-REVIEWER : Figure 7. Check the letters in the panels. (g) is missing and (m) is excess. Also this figure suffers from being very small. The axis labels tick marks are unreadable.

ANSWER: Labelling of the graphs was be corrected and the readability of the figures is improved

Interactive comment on Biogeosciences Discuss., 12, 11349, 2015.

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