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Interactive comment on "Effect of sporadic destratification, seasonal overturn and artificial mixing on CH_4 emissions at the surface of a subtropical hydroelectric reservoir (Nam Theun 2 Reservoir, Lao PDR)" *by* F. Guérin et al.

Anonymous Referee #1

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General comments:

This manuscript deals with important topic of methane emissions from artificial impoundments. Especially important is that in this study the spatial coverage is good and the time series cover multiple years. New significant reservoir features, which might be related to the power plant construction, are revealed. These lead to extremely high methane fluxes to the atmosphere. This study interests broad audience and seems appropriate for Biogeosciences.

In general, the structure of the manuscript is well organized and flows in a logical

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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 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.

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.

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.

Minor comments:

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

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"...the vertical profiles of temperature and dissolved oxygen in the water column..."

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.

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?

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.

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 precise how the measurements were conducted. Using word "station" leads the reader to think of a fixed mast or platform or such.

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.

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?

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

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the reader that these equations can be used for this reservoir and for different parts of the reservoir.

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.

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?

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?

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?

Page 11366, lines 6-7. “It therefore suggests that the residence time…”. 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.

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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.

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.

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 CH_4 to the area of strong vertical mixing. Therefore, this spot has even larger spatial impact causing outgassing of CH_4 from large area.

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 CH_4 mass balance was not conducted and there are unclear components in CH_4 cycle, like possible lateral transport of CH_4 (page 11368, lines 6-11).

In general, the conclusions are well written and concise.

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.

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?

Figure 3. (c) is missing.

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.

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