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Interactive comment on "Physical controls on CH_4 emissions from a newly flooded subtropical freshwater hydroelectric reservoir: Nam Theun 2" *by* C. Deshmukh et al.

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We thank David Bastviken for his positive comments on the submitted manuscript and for his suggestions to improve it.

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Line 7-8: It seems that inland water as used here includes wetlands, while in many cases inland waters are defined as running waters and water bodies but not including other types of wetlands. I prefer this latter meaning because I think we should use definitions that goes hand in hand with flux types and flux regulation, but the terminology is a bit confusing in many papers at present. Please be clear on how the terms used here are defined.

Reply: We agree with the reviewer, and the sentence was modified as follow: The emission from aquatic ecosystems (wetlands and inland freshwaters) is the main source of CH_4 on Earth (IPCC, 2013) representing 40% of total CH_4 emissions and 75% of natural CH_4 emissions (IPCC, 2013). Emissions from inland freshwaters alone would correspond to 50% of the carbon terrestrial sink (Bastviken et al, 2011). The order of magnitude of CH_4 emissions from inland waters is probably conservative (Bastviken et al., 2011).

Line 19-21: Please check the structure of this sentence. I am not a native English speaker but it seems strange. I would also say that diffusive fluxes have been studier far more than ebullition and I think this would be important to note. Reply: We agree that diffusion has been studied more than diffusion. The sentence was modified as follow: Among the different known CH_4 pathways to the atmosphere, diffusive fluxes and, to a lesser extent, ebullition have been the most studied ones in natural lakes and anthropogenic water bodies (i.e., hydroelectric reservoirs, farm ponds, etc.).

Line 24: Please consider “under anoxic conditions”: and please double check my language suggestions – I may be wrong.

Reply: Sentence was changed as suggested: "Methane is produced under anoxic conditions in the sediments or the flooded soils during the mineralization of organic matter."

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Line 13: May I suggest “ by discrete sampling with funnels or floating chambers, ebullition was shown to dominate compared to diffusive ”?

Reply: Sentence was changed as suggested: In most of the ecosystems where it was determined by discrete sampling with funnels or floating chambers, ebullition was shown to dominate diffusive fluxes (Bastviken et al., 2011).

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Line 1-2: Two other studies reporting no or negligible bias from floating chambers are Cole et al. 2010 in Limnology & Oceanography Methods 8, 285-293 and Gålfalk et al. 2013. JGR Biogeosciences 118, 770-782. I think the evidence that properly designed chambers are fine is accumulating and it may be good to show this.

Reply: These studies are now cited

Line 2-5: A detailed comment: I think it is best to say that chambers always capture both diffusive flux and ebullition if present. In low ebullition environments these flux components can be separated by variability patterns among replicate chambers (e.g. Bastviken et al 2004) but in high ebullition environments bubble shields may be needed to estimate diffusive flux by excluding ebullition from some chambers (Bastviken et al 2010).

Reply: We agree with reviewer's comment. This was included as follow: FCs capture both diffusive flux and ebullition if present. In low ebullition conditions, these flux components can be separated by variability patterns among replicate chambers (e.g. Bastviken et al 2004). In high ebullition environments, bubble shields may be needed to estimate diffusive flux by excluding ebullition from some chambers (Bastviken et al 2010).

Line 23: Why was the modelling used for a four-year period? Why not other time frames?

Reply: Previous studies have suggested a decrease in emissions with the age of reservoir (Abril et al., 2005; Barros et al., 2011). Since in our modeling approach, age of the reservoir has not been included, model can only be used for gap-filling/interpolation of ebullition, not for future prediction as specified in the manuscript.

Page 3278: Please consider providing a map showing the reservoir and all locations where the different measurements were performed. This map could perhaps also indicate different foot-print distributions. Such a map would make it easier to understand the extent of the study.

Reply: A map showing the flooded land cover was added in the manuscript and maps of the eddy covariance footprint were added in the supplemental

Page 3287: Sentence starting at line 29: I am not sure I understand the sentence “Statistical analysis of May 2009 data shows that DEEC are significantly different ($p = 0.1075$, Table 2) with the sum of the diffusion and ebullition discrete sampling.” To me a p-value > 0.05 indicates “no difference”. Please clarify.

Reply: The p value for May 2009 is <0.0001. Value was modified in the text and in table 2.

Page 3288: Line 6-7: I do not understand the sentence “But, in a handful occasions, DEGC and DEEC exceed DTBL, DGC, DGA by a factor up to 100 (Fig. 1c).” and the following discussion where this seems surprising that needs to be explained. Is it not logical that diffusive flux plus ebullition exceed diffusive flux only in systems with a lot of ebullition? Does this have to be discussed extensively?

Reply: It was a confusing attempt to convince reader that those fluxes were dominated by ebullition, and not by diffusion. This paragraph was completely reworded and shortened (see section 4.1.4)

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Discussion regarding CH_4 content in bubbles: I find the low CH_4 proportion in the bubbles a bit surprising and the explanations are sometimes difficult to understand. The solubility explanation seems strange giving that much higher CH_4 percentages are typically found in cold waters of high latitudes where solubility should be greatest. If methane oxidation happen in the sediment if would convert CH_4 to CO_2 which is very soluble: : :and thereby decrease bubble size rather than reducing the CH_4 percentage. Could it be other gases transported from the water to the bubbles thereby diluting CH_4

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or could this simply be combined with oxidation in the bubble traps? Any correlation between CH_4 percentage and funnel deployment time: or versus depth (reflecting time for bubble gas exchange in the water column)?

Reply: In this section, we will modify our discussion according to the elements below. As shown by McGinnis et al. (2006, JGR), during the rise of a bubble in the water column CH_4 from the bubble dissolves partly in the water, its composition change, being enriched in N_2 and O_2 and probably other gases and the volume of the bubble decrease. The deeper the water column is, the smaller is the CH_4 concentration in the bubble reaching the surface. We measured only CH_4, CO_2 and N_2O in the bubbles. The concentrations of CO_2 and N_2O were both around 1% most of the time and cannot explain the low CH_4 concentrations.

As mentioned in the original submission, we observed a decrease of the CH_4 concentration with depth during the WD and CD season whereas no statistical differences where observed during the WW season. However, we cannot discuss the volume of gas collected in the gas trap since we do not know the volume of gas that escaped the sediment/flooded soils and this impact strongly the volume of bubbles reaching the surface (Ostrovsky et al., 2008).

Our argumentation about solubility was not to explain the difference in terms of concentration between NT2 and other sites but it contributes to explain the seasonal variations. The colder the water column is, the smaller are the concentrations as mentioned in the original submission.

CH_4 concentration in bubbles is in the lower range of what is usually found in lakes and reservoirs as noted in the submitted manuscript and we have no clear explanation for this. The CH_4 concentrations are however in the same range as concentrations in bubbles reported for rice fields and vegetated wetlands (Rothfuss and Conrad, 1992; Frenzel and Karofeld, 2000; Kruger et al., 2002; Chanton et al., 1989; Tyler et al., 1997). The CH_4 concentration in bubbles in these ecosystems are supposed to be low because of a high methanotrophic activity in the rizhosphere of the vegetation permitted by a high ventilation of the soils by active transport of air through the stems of the vegetation. In the NT2 reservoir, there is almost no aquatic vegetation rooted in the littoral zone of the reservoir. However, the reservoir floods soils which are probably very compacted below a few centimeters. As a consequence, bubbles might develop close to the flooded soils/sediment-water interface. The area were bubbles were collected has a maximum depth close to the depth of the oxycline most of the year which implies that the first millimeters of the flooded soils are probably oxygenated in the area shallower than 10 m. In addition, during the lake overturn in the CD season and during the sporadic destratification events in the WW season, O2 could reach the flooded soil-water interface. Therefore, CH4 oxidation could affect the CH4 concentration in bubbles in the flooded soils before they escape and as a matter of consequence, the concentration of CH4 in bubbles are low.

Furthermore, we did not observe correlation between the CH4 percentage and the deployment time of the funnels. As a consequence, the low CH4 concentrations cannot be attributed to a biased sampling procedure.

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Paragraph starting at Line 21: With an r2 of 0.03, a significant relationship with temperature does not seem very important in this case, so perhaps the low r2 and thereby the low predictive power under these conditions and the temperature range and hydrodynamics in this case could be emphasized rather than providing various mechanistic explanations?

Reply: The paragraph was reworded as follow “Finally, we found a very low correlation between ebullition and reservoir bottom temperature (r2 = 0.03, Fig. 8h). This shows that, given the hydrodynamics and the temperature range experienced in the NT2 Reservoir, that this physical parameter has a very low predictive power. This is due to the co-variation of several factors at the same time hiding a possible effect of temperature on the benthic methanogenesis activity. The absence of correlation between temperature and ebullition is mostly due to the fact that the highest bottom water temperatures were often synchronized with the beginning of the WW season when the ebullition is moderated by the water level increase. This illustrates the complexity of controlling factors interacting at the same time, and one with each other in a non-linear way. As a matter of fact, it is worth trying a non-linear method to represent ebullition through several relevant parameters, identified in this section but not necessarily highly correlated with ebullition." As explained in the section 4.6, the addition of temperature as an input for ANN improved significantly the modeling of CH_4 ebullition.

Table 1: Would it be possible to clarify the abbreviations in a more direct way to make independent reading of the Table easier. For example instead of having one note pre row in the Method column, would it be enough to have one note for Method in the column head and then in this note spell out that e.g. DEGC is: : :, DEGA is: : :etc?

Reply: Table 1 was changed. All statistics were removed and are now in table 2 and the abbreviations for the methods are now explained in foot notes

Table 2: It is a bit difficult to understand what data was compared in the different tests (e.g. for the different p values give). It is not clear from of comparisons were made between columns or rows in the table. Can the Table be reorganized to show what statistical comparisons were made independently from the text?

Reply: Table 2 was changed and contains only the statistics to facilitate understanding

Figure 1. I see the point with having similar scales for all panels, but this makes it impossible to see any patterns among sampling times in panel (a). I think it would be interesting to see more of the data in this panel.

Reply: The scales were modified in the figure 1. This figure was transferred in the supplemental

Figure 8. Panel b: The similar color for temperature and modeled flux can cause

confusion. How about making a thin black line for temperature?

Reply: Changes have been made in Figure 8

Interactive comment on Biogeosciences Discuss., 11, 3271, 2014.