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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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Review of Biogeosciences Discuss., 11, 3271–3317, 2014, Physical controls on CH4 missions from a newly flooded subtropical freshwater hydroelectric reservoir: Nam Theun 2.

This study combines multiple approaches to measure methane emissions from a tropical hydropower reservoir. Flux chambers, submersed funnels (bubble traps), water concentrations combined with modelling of gas exchange coefficients, and eddy covariance measurements were performed in a thorough way with multiple replicates and repeated measurements for 1.5 years. Integrative modelling was made with an

interesting artificial neural network approach revealing that primarily atmospheric and hydrostatic pressure is important for fluxes. Data also indicate good agreement between flux chamber + bubble traps and eddy covariance measurements. This whole study is important by addressing a type of environment for which we need to learn more about greenhouse gas fluxes (tropical reservoirs which are extensively debated for their potential emissions), and by showing a high level of ambitions and great efforts regarding obtaining high quality flux measurements. Such extensive data accounting for both spatial and temporal variability are needed to better evaluate inland water methane emissions. The attempt to model ebullition is also very useful. Also from my own experiences of tropical work I know how difficult and demanding it is to produce such high quality data as those presented here and I congratulate the authors to a well conducted study. I suggest publication of this manuscript after minor revisions based on my comments below.

Page 3274:

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.

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.

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

Page 3275: Line 13: May I suggest "...by discrete sampling with funnels or floating chambers, ebullition ebullition was shown to dominate compared to diffusive ..."?

Page 3276:

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.

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).

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

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.

EC methods: I am not able to fully evaluate the EC-methods but the text is convincing and shows awareness of recent relevant studies so I assume everything is correct.

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.

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?

Page 3293:

Discussion regarding CH4 content in bubbles: I find the low CH4 proportion in the bubbles a bit surprising and the explanations are sometimes difficult to understand. The solubility explanation ssme stragen give that much higher CH4 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 CH4 to CO2 which is very soluble. . .and thereby decrease bubble size rather than reducing the CH4 percentage. Could it be other gases transported from the water to the bubbles thereby diluting CH4 or could this simply be combined with oxidation in the bubble traps? Any correlation between CH4 percentage and funnel deployment time. . .or versus depth (reflecting time for bubble gas exchange in the water column)?

Page 3296:

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?

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?

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?

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.

Figure 8. Panel b: The similar color for temperature and modeled flux can cause confusion. How about making a thin black line for temperature?

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