REPLY TO David Bastviken

We thank David Bastviken for his positive comments on the submitted manuscript.

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.

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."

Page 3275: 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).

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.

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)*

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 seems strange giving 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)?

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 CH4 from the bubble dissolves partly in the water, its composition change, being enriched in N2 and O2 and probably other gases and the volume of the bubble decrease. The deeper the water column is, the smaller is the CH4 concentration in the bubble reaching the surface. We measured only CH4, CO2 and N2O in the bubbles. The concentrations of CO2 and N2O were both around 1% most of the time and cannot explain the low CH4 concentrations.

As mentioned in the original submission, we observed a decrease of the CH4 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.

CH4 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 CH4 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 CH4 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.

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?

Reply: *The paragraph was reworded as follow*

"Finally, we found a very low correlation between ebullition and reservoir bottom temperature (r^2 = 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 CH4 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*

REPLY TO REVIEWER #2

We thank the reviewer for his thorough review of the manuscript and his positive comments. All typos, suggested rewording ... were accepted and are not commented in our present answer. All comments were taken into account and most of them were accepted unless specified.

General comments:

1. The language needs some help still in order to make this paper a bit easier to read.

Reply: *the manuscript was read by an individual with good expertise in English*

2. Abstract – The artificial neural network should be briefly explained in the abstract and thensay that it explained 46% of the variability. Really pull out the main points of your paper and make the abstract full of those points. I think you can stress less on the methods used in the first part of the abstract. Just mention them briefly and get right to the main points about emission results and physical drivers.

Reply: *The abstract was reworded. Fewer details are given on the methodology and more results are given now*

3. The introduction gives me the impression that this is a methods paper, but the title of the paper says it will talk about physical controls on emissions. Therefore, I would scale back the methodology talk in the introduction and bring out the physical part more. Right now you jam a lot of physical information into basically one sentence. The fact that you used all those different technologies is a unique part of this study but if the physical results are a bigger deal, as the title suggest, then you should focus on that more in the introduction. You also conducted a study on a dam, not a natural system, yet you do not discuss dams much at all in your introduction. You should at least bring up the age factor in regards to measuring dams. In general, the introduction needs a rewrite to focus it more on the main findings of the study.

***Reply**: We deployed very basic and common equipment for the measurements of CH4 diffusion and ebullition (floating chambers, funnels or bubble traps) together with EC which is the most recent method for the determination of CH4 emissions. Even if the order of magnitude of emissions obtained by EC was already compared with total emissions by classical technics in Eugster et al. (2011), we are the first to present time series of emissions with concomitant variations of the different emission pathways (diffusion, ebullition and total emissions) with a large number of individual measurements (former figure 1 and now in the new figure S2). Therefore, we think the technical aspect of this study has to be highlighted. Furthermore, in order to identify the physical processes of emissions and especially the daily bimodal variations of CH4 emission, we had to discriminate diffusion and ebullition in order to show the mid-day peak of ebullition. Therefore, we think the validation of the different methods and their comparison is mandatory in the article and is a key component to convince the audience on the physical processes we found.*

The introduction of the article is not focused on GHG emissions from hydroelectric reservoirs but on aquatic ecosystems in general because this study deals with the controls of CH4 emissions by diffusion and ebullition which are common in natural aquatic systems and hydroelectric reservoirs. The controlling factors are the same, only the source of carbon and the magnitude of the water level change are different. Based on the literature on natural systems and reservoirs, we already listed all known controlling factors for both diffusion and ebullition. In addition, hydroelectric reservoirs were poorly studied in terms of processes, so comparison could mostly be done with natural aquatic systems.

For the reasons listed above, we think the introduction of the submitted manuscript fulfil the requirement of the reviewer on the physical control aspects and on the focus on dams.

However, some changes were made to make our points clearer according to the comments above.

4. To better illustrate the complicated sampling scheme of this study I suggest adding a table and a map figure. The table would actually be an expansion of Table S3, which has the details of the EC deployments but not of all the other ebullition deployments. IT would be good to know the deployments and depths for trap measurements. I understand you have a lot of

measurements but perhaps they can be summarized somehow. Secondly, a map figure should definitely be added (can be placed in supplemental) that will first give the reader the idea of where the reservoir is, what its shape and size are, but also some details regarding sampling should be shown. IT would be good to actually see 4 maps with the EC footprint for each deployment and then approximately where FC and Trap sampling took place in the footprint. This would also be nice to see for the trap measurements that you additionally did for a year and over different flooded ecosystems. I highly suggest some maps to help the reader follow what is going on where. Perhaps also tables in the supplementary stating how many funnel and FCs were done, etc.

Reply: *A general map of the reservoir showing the flooded ecosystem and sampling sites was added in the manuscript (new figure 1). In the supplemental, four maps of the EC footprint were added (New figure S1).*

The number of chamber and funnels deployment in the footprint of the EC is already given in table 1. The number of funnels (n= 4811) for the one-year and a half monitoring at a fortnightly/monthly frequency is given in the text together with the depth ranges. All data are in the figures 6-9 (new numbering). In our opinion, an additional table with all the raw data is not necessary.

5. In the sampling strategy section there is a quite confusing description of the water depths and water level changing rates for each deployment. Perhaps a figure in the supplemental could help illustrate this better. Can you acquire water level data for the entire 3 or 4 year period and then point out on there where your deployments were? If you could also divide the figure into seasons (WD, WW, CD) then this would really help facilitate the reader in understanding the hydrological conditions of the reservoir.

Reply: *A figure (new figure 2) showing together the field experiment times and the meteorology/water level was added (new figure 2).*

6. Clearly by my questions below about the ANN, it needs to be more clearly explained in both the manuscript and the supplemental. The text should begin with a very general

description of what it is and why you are using it. Terms should be described, such as 'training' and 'generalization cost' and 'weights'. The supplemental would benefit from text too so that the reader can follow the equations better.

Reply: *The whole section 3.6 was rewritten to fulfil the requirements and some text was added to the supplementary material (section S1)*

7. Results and discussion section – There is a lot of good data and analyses in this paper, but it is hard to keep it all straight while you are reading it. I would highly suggest splitting this section into two separate sections – results then discussion. Actually, I believe the first section (Assessment of emissions) is primarily results, while the rest of the sections are more discussion already. I have made some suggestions in the specific comments as to where to do some splitting, but they are definitely not comprehensive. If you choose to leave the results and discussion together, however, I implore you to split the first section (Assessment of emissions) into separate sections: (1) diffusion, (2) ebullition, (3) total emissions including comparison between methods, and (4) discussion ebullition specifically (from page 14, line 14 to the end of that section). The rest of the sections are sufficient as is. But I highly suggest splitting results from the discussion.

Reply: *The structure of the article was chosen because of ANN. ANN results cannot be presented before the discussion on the controlling factors of ebullition which were identified by the field measurements. Discussion about relevant parameters is essential to link what we know in theory from literature and what we learn from our data. The mixing of both approaches leads to the possibility of testing the ANN with data known as important and data we suppose relevant. But all that cannot be done before discussing the relevance of input parameters. Therefore, the manuscript cannot be organized with separate result and discussion sections.*

However as suggested we divided the section 4.1 in four sections will generate very short section. All detailed comments related to the change of structure of the manuscript were therefore not considered.

8. Very important: the abbreviations are different between the text and the figures and tables. Please be consistent. This will put a strain on the reader to understand your already complicated study. You use FCGA and FCGC in the text but DGC and DGA in the tables and figures. And I understand that you use DE in the tables and figures for the diffusion plus ebullition measurements, but I would suggest using T for 'total' instead. So my suggestion for variables to remain consistent throughout the paper text, tables, and figures are the following: DFC, DGA, DTBL, EFUN, TEC, TFC+FUN

Reply: *In the original manuscript, there was some confusion between the abbreviation for the methods and those for the measured flux itself. This is now stated more clearly. We kept DE for diffusion+ebullition.*

9. You use the word 'evidences' a lot but it does not sound right. You should use other words

instead like 'indicates'.

Reply: *Changes were done when necessary*

Specific comments:

2. PAGE 1, Line 24 – add 'respectively' after 'chambers'

Reply*: We measured diffusive fluxes with the floating chambers only but ebullition was quantified with both funnels and floating chambers. Therefore, the addition of "respectively" in this sentence does not reflect our results.*

6. PAGE 2, Line 25 – Saying inland waters are the main source of CH4 on earth is a little misleading as its predominantly only wetlands ... CHECK THE IPCC 2013

Reply: *We cited IPCC 2013 for wetlands only and Bastviken et al for Inland waters without welands and sum up the two emission factors to stress the importance of freshwaters aquatic ecosystems. As mentioned in our answer to David Bastviken, we rephrased it.*

9. Line 14 – start a new paragraph with the 'The release of bubbles is triggered by...' and expand upon this. How were these parameters measured before? Was the resolution high enough? As high as what you will show? Are some physical drivers more relevant in certain places than others and why?

Reply: *In this section, we are citing 24 different studies in very different ecosystems. The sampling frequency varies from a few measurements to thousands. It is impossible to give this kind of details in the introduction and to evaluate if it was enough or not. Some parameters are probably more relevant than others in some specific environments but this analysis is out of the scope of our paper.*

20. Page 9, line 2 – You have not described 'GC' yet. You need to spell it out and put all GC details here.

Reply: *OK for spelling it. For a better coherence, the GC measurements are described after the description of all the methods for emissions.*

22. Line 25 – shouldn't Ca be the solubility concentration of atmospheric methane?

Reply: *This is the "CH4 concentration at equilibrium with the overlying atmosphere", Corrected*

23. Line 27 – how did you get k600? That is not described

25. Line 4-11 – I do not understand what you are trying to describe here. Oh, I think I see. You are saying that since you had ebullition in your chambers that a k600 could not be determined from the chambers measurements via k = F/Cw-Ca. Therefore, you completed the Dtbl formula with a k600 determined from wind speed from both those references and then averaged them. You need to state all of this more clearly. Clearly, this whole paragraph is

about k600 and I did not get it at first since I asked question #22. You should perhaps show the formulas you used from those references as well. How different are they?

Reply to comments 23&25: *k600 were calculated from Guerin et al., 2007 and McIntyre et al., 2011 when the chambers captured ebullition as it was indicated in the original manuscript. Since we finally did not really used k600 from our dataset, we modified this paragraph to make it clearer by removing the first sentence that was confusing. The formulations we used are from recent articles therefore we do not think it is necessary to provide them. The differences between them are given in the text: Guerin et al. (2007) is the only available for tropical reservoirs and takes into account the rain whereas McIntyre et al. (2011) takes into account buoyancy effect on fluxes.*

26. Line 13-20 – You have no description of the funnels nor a reference that would describe them. You need to add something here. What material? How tall? What was the collecting container on top? How did you sample it? You also don't describe the sampling resolution. How many times did you sample in that 24-48 hour period? IF you left it there that long, should you be worried about re-dissolution into the water in the collecting container? Did you refresh the funnel?

Reply: *As now stated in the MS, funnels are made of PET and we used butyl-rubber stoppers on top of it. One sample was taken after 24-48h as done, for instance, by Wik et al. (2013, JGR Biogeosciences) and many others.*

We cannot completely exclude that oxidation occurs but we minimized the surface contact between the bubbles and the water and we did not find any correlation between the time of deployment of the funnels and the CH4 concentration in bubbles.

27. Line 20-23 – I don't understand what you did with Ega and FC measurements. Please explain more clearly. It appears you did something with the FCGA measurements to get at ebullition but you must state that clearly here.

Reply*: More details are given now in the MS*

28. Line 25 – there is the GC information that needs to be put earlier.

Reply: see reply to comment 20

29. Page 11, line 2 – The ANN and MLP both need references

30. Line 5 – same day and same depth? But is it the same location? Why do you do this? Why by depth?

31. Line 9 – what is a training process?

32. Line 11-18 – this should be the first paragraph of the section but should begin with a very general description of what an ANN for a general audience and why you are using it

33. Line 19-22 – the weights were found during the training? This should go with the first paragraph (lines 2-10)

34. Line 24 – what is a generalization cost??

35. Line 29 – should be Tables S2 and S3

Reply to comments 29-35*: The whole section was completely rewritten taken into account the above comments.*

More specifically for the comment 30, a sentence was added in the text. Raw data are averaged to avoid repetition of the same meteorological data and the same depth value for different fluxes. This would rather introduce noise in the signal than relevant information. It is of course done for data at the same location. If the location is different, depth, day and meteo data are different.

36. Page 12, line 2 – ‘in the CH4 emissions’ – in all emissions or just ebullition?

***Reply**: All emissions that is diffusion, ebullition and total emissions since we have evaluated all these emission pathways through different methodologies*

38. Line 11 – by seasonal basis I guess you mean those 3 seasons (WD, WW, CD)? You should be specific here.

***Reply**: the sentence was modified to explicitly give the seasons*

39. Page 12, line 28 – after ‘four methods’ list the methods in parentheses to remind the reader.

***Reply**: most of this section was modified and the methods are now listed*

40. Line 30, 31 – you list ‘FC’ and ‘FCGC’ – but shouldn’t they both be ‘FCGC’ – however they should match Table 1 and figures too.

***Reply**: see our answer to general comment 8. The section was modified to clarify*

43. Page 13, line 3 – you cite Figure 1 here but then barely discuss it at all here and only very little later on – is the figure necessary in the main manuscript? Could it be in the supplemental?

***Reply**: we agree with the reviewer and put the figure in the supplemental*

47. Line 10-12 – Because you have these numbers already in tables, you do not need to display them again in the text. You can things like this for example ‘Overeall, the average

Dgc (~1.1) was comparable to the average Dtbl (~1.4) for all four field campaigns, however the range for Dtbl was slightly larger.'

***Reply**: all ranges were removed but we kept SD*

49. Line 18 – the EGA abbreviation should not be right after saying 'ebullition of CH4' but after saying that you used the FC with a GA to measure it. This is all confusing. The way you measured ebullition with the FCGA was not described in the methods either.

***Reply**: see answer to comment 27*

50. Line 22-24 – again here you can get rid of numbers since they are in tables and only mention an approximate average

***Reply**: all ranges were removed but we kept SD*

51. Line 18-27 – I would start this paragraph stating that 'the FCGA was used to measure diffusion but about 50% of the measurements also showed ebullition emissions' and then state the results/averages of the this method and the funnels and then state that these methods differed by deployment times.

***Reply**: most of this section was modified in the revised MS*

55. Page 14, line 1-2 – I don't understand this last sentence. The chamber measurements were higher than the EC measurements? And you think since only 50% of the chamber measurements caught bubbles while the EC should have been measuring them all the time that the chambers will over estimate emissions if they get sporadic bubble measurements, opposed to the EC system that integrates over both emissions? Did you not get peaks in the EC from when you think bubbles occurred? While the average measurements from EC was lower than FCs in Eugster et al. 2011, the peaks in EC data coincided with chamber

measurement values. You need to back up this statement but I think you should do this in a discussion section (if you separate results from discussion). Plus everything in this section up until now was results – these can clearly be made into a results section.

***Reply**: All this section was modified to make it clearer*

When a chamber captures ebullition, this is integrated over a very small surface for the duration of the deployment. When ebullition occurs in the footprint of the EC, this ebullition is attributed to the whole footprint area although it probably occurs at much smaller scale. Therefore as bubbling is not spatially homogeneous, chamber technique leads to an over-estimate when it captures ebullition.

However, total emissions by EC and by FC always show the same general variations.

56. Line 3-13 – I think this paragraph can be with the previous section too in regards to total emissions.

***Reply**: All this section was modified to make it clearer*

58. Line 12 – here is the only time you state how many funnel measurements were ever used and we have nothing else to compare this too. You should have a table in the supplementary or something describing the funnels and chamber measurements performed.

***Reply**: all funnel measurement numbers were in the table 1 of the original submission and are in its new version*

59. Iine 14-to end of section – this should be a separate section as it specifically talks about ebullition and it is mostly a discussion point

***Reply**: see answer to comment 7*

61. Line 16 – change to ‘emissions determined within an EC footprint of thousands of m2’ – and you don’t report the actual EC footprints anywhere. You should have them in figures or at the very least a table somewhere.

***Reply**: figures were added in supplementary material (figure S1)*

63. Line 24-26 – you are saying here that the funnel measurements were similar to the EC estimates for ebullition but earlier (page 14, line 33) you said that they were different (at least for the totals, which I presume the totals were mostly controlled by ebullition). So then back to the statement on line 1-2, why would chambers overestimate emissions but not funnels relative to EC values? I see your point at the end of this page about longer versus shorter deployment times. I think this is the main issue. Of course the longer you measure or the more area you measure the larger the integration of all values (low, which are more common, and high) and thus the lower the resulting fluxes. These are great points but need to be made much clearer in their own section in the discussion!

***Reply**: All this section was modified to make it clearer. Also see comment 55.*

64. Page 15, line 8 – how was this measured exactly?? Which methods were used?

***Reply**: As indicated the manuscript, this was calculated from the field results listed in the table 1, that is a calculation of the contribution of ebullition (obtained by discrete measurements) to the total emissions obtained by EC*

68. Line 16 – you are talking about an analysis of the time series but what analysis? Is it what you describe on line 21? I would start with describing this analysis (line 21) and then discuss it later. You can report this analysis and figure in the results and then save the discussion points for later in the discussion when you can talk about the various physical controls you are investigating (the major point of the paper).

***Reply**: the use of “analysis” was inappropriate, we modified the sentence*

78. Line 8-11 – why do both observations prove this? Is it because peaks occurred no matter the buoyancy flux? Be more specific here.

***Reply**: The text was modified as follow:*

"On one hand, nighttime peak of CH4 emission coincides with low but constant buoyancy fluxes (i.e.; most instable water column) and moderate atmospheric pressure drop. The fact that the buoyancy flux does not decrease during the peak of CH4 indicates a low control on the emissions, if any. On the other hand, daytime peak of CH4 emissions are linked with maximum buoyancy fluxes which cannot enhance emissions (i.e. most stable water column). These observations tend to prove that CH4 bursts in the night and around noon (up to 100 mmol m-2 d-1) could be entirely attributed to the atmospheric pressure drops that triggered ebullition, more than any buoyancy effect."

81. Line 26-29 – should be in methods

***Reply**: part of the information that should be in the methods was transferred in the sections 3.1 and 3.4*

83. Page 18, line1-6 – all results and you could place these values in a table and then you don't have to repeat them

***Reply**: see answer to comments 4 and 7*

84. Line 6 – I feel like a discussion of what differences in volumes means is missing. If this is simply in the results and not important then it really doesn't need to be discussed in the discussion section

***Reply**: This result is not discussed because there is no clear way to explain the seasonal variations. This is a complex phenomenon that depends on the water level variations, pressure and temperature (CH4 production and solubility).*

90. Line 30 – 'higher in the shallow zones (median = 21.52%) compared to the deeper zones (12.78%), which can be explained by the dissolution of CH4 from bubbles being more efficient in deeper waters (McGinnis et al. 2006)'... however your waters were only 10 m deep at the deepest, right? That's not a huge difference in terms of CH4 dissolution but size can double in that 1 atmosphere. Use Ostrovsky et al. 2008 (L&O Methods) to get an estimate.

***Reply**: The sentence was modified as follow:*

"According to McGinnis et al. (2006) and Ostrovsky et al., (2008), the decrease in the CH4 concentration in bubbles by the dissolution of CH4 for a maximum water depth of 10 m can reach up to 20%. Therefore, this process could explain the variation of CH4 concentration in bubbles according to depth."

94. Line 13-17 – about the flooded ecosystem should be in results and is also very much thrown in here – you don't really mention much about this aspect of the study – is it important? Or relevant???

***Reply**: The spatial variability of ebullition according to the flooded ecosystems was thought to be relevant but we observe no statistical difference thus our discussion on this aspect is very short*

99. Line 28-32 – if this is about Fig. 7f, g then it should be discussed later after Fig. 7a-e

***Reply**: The entire figure 7 (now figure 8) was described briefly before we mention Figure 7,f,g.*

104. Page 21, line 14 – is r2=0.03 even worth discussing? Was temperature really negatively correlated? The absence of correlation is more believable.

105. Line 23 – change sentence starting 'As a matter of fact, it is worth trying...' to 'Ultimately, relevant parameters, such as those identified here, should be considered in non-linear models for ebullition even if they are not highly correlated with ebullition.' ... but why? I don't see the reasoning for this here. Temperature didn't correlate and it did not meet your a priori expectations. This would say to me that it is simply not relevant in this situation but of course trying it at first to determine this is the proper procedure.

Reply *to comments 104 and 105: as mentioned in our answer to David Bastviken, we removed the word correlation and the full paragraph was reworded.*

It is a very relevant parameter since it improves ANN results very significantly. We do not see its effect in the raw data because water level increases at the same time as the temperature. The ANN is able to deconvoluate these two antagonist effects.

107. Line 4 – where is this 21% results presented? You mention MLR very briefly and it seems to not have worked, which is fine. If this is the only presentation of the results then put '(data not shown)' in the sentence.

Reply*: We added (data not shown)*

112. How was the ANN validated? Was the 4 year time series results only evaluated with the short four campaigns or with the full year dataset? Modeled series looks higher than measured. Why?

Reply*: The ANN is "evaluated" through the validation process and the assessment of quality costs such as generalisation cost. In that case it is evaluated with the full dataset.*

Modeled series may be smoothed compared to measured series. The ANN gives a more flat version of what occurs in reality, i.e. the extremes are not well represented because extreme values in the data base are scarce compared to mean values. This could explain why very low and very high values are not well reproduced. An important point is that the ANN well reproduces the seasonal variation.

113. Line 24 – this 50% - is it 50% of the ANN results?? – same question for the 60-80% on line 26

***Reply**: these are ANN results as now specified in the MS*

115. Page 23, line 1 – I would not say this your method is 'costless' or 'effortless' – you couldn't have done the ANN without real data, correct? Therefore, you do have to put forth effort and cost. Please rephrase

***Reply**: we removed these words from the sentence*

116. Line 6 – why must it be used during a period of stable ebullition? I don't see the point.

***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.*

123. Line 12-16 – I think it is obvious now that ebullition is an overlooked pathway in reservoirs and I think you have more important points from your data to conclude on. Find the most outstanding points and make those your ending and really put forth why they are important for others.

***Reply**: It starts to be obvious in natural systems. In reservoirs, only one or two studies by DelSontro showed it, among which one in a very peculiar run-off the water old reservoir impacted by a city.*

Here we stress that ebullition in young tropical reservoirs could have been underestimated. This study is the first one to show it.

We do not put more weight on this than on the other concluding remark. That is one of them and the last one since we needed the full emission estimate to evaluate it.

124. Table 1 – you have FC in the notes for floating chamber but you don't use FC in the table. And I made suggestions above as to how to change the variables and you need to make them consistent throughout the text, tables and figures.

***Reply**: Footnotes were modified, as well as the table and all abbreviations are now consistent*

125. Figure 1 – I am not sure I see the point of Figure 1. Either put it in supplemental or bring the point out more in the text. Also put a box around the legend in all panels. In the caption describe what the variables are.

***Reply**: Figure 1 was moved to the supplemental (Figure S2) and we put boxes around the legends. All variables are now defined in the caption*

126. Figure 2 – Why is Ega not here? I would try to exaggerate the boxes in the lower scale more (from 0 to 30) so it is easier to tell what the small boxes have in them. Also make clear in the caption why March 210 has no ebullition data and what the difference is between the first two boxes in May 2009 and March 2011. Put variable descriptions in figure caption.

***Reply**: This figure is now Figure 3. E_{GA} was measured only in march 2011 therefore we exclude it from the graph. The scale is the most adapted to all data presented in the graph so*

it was unchanged. There is no difference between the first two boxes in May 09 and March 11. The variables are now described in the caption.

127. Figure 4 – what are the solid and dashed lines in panels e-h?

***Reply**: Now this is Figure 5. The solid line is the regression line and the dash lines represent the confidence interval, this was added to the caption*

128. Figure 7 – why is the line and r'2 not shown in panel h?

***Reply**: Now this is Figure 8. It does not appear since showing the anti-correlation between the ebullition and the temperature would lead to misunderstanding (see comments 104-105 and our answer to David Bastviken comments on this correlation.*

129. Figure 8 – does the box description also apply to panel a? Then say so or if not, then what? And why is Fig S3 not part of this figure? S3 seems to be important and it can easily fit in this figure.

***Reply**: Now this is figure 9. The box description applies to both panels as now specified in the caption. The main information (linear regression, r2 and p value) of figure S5 (former figure S3) is in the text and the figure do not provide crucial information, therefore this panel was not added to the figure 9.*