Dear reviewer/editor:

We ssincerely appreciate your suggestions and help on this paper. We are pleased that the reviewers saw merit in our paper, and recognised the importance of this relatively new line of work. We read their comments with great interest, and we have managed to complete an extensive revision on time. We thank them for their efforts.

According to the two interactive comments, major revision of the manuscript is listed below:

Revision of introduction

1. The new introduction is more focused on the necessity of work on reservoirs, especially on the littoral zone. Limitations of previous work in the littoral zone were discussed as well as the unique contribution of this work. We have deleted some of the more general material about climate change.
2. We have refined the hypothesis and objectives, spelling them out more clearly and accurately.

Revision of methods

3. We have clarified several items in the 'methods' which the reviewers queried.
4. We have revised the description of statistical methods including some new analysis.

Revision of results

5. We have carried out more statistical analysis, and in particular we have looked at the negative fluxes as well as the overall fluxes, and tried to relate them to environmental variables.
6. Diurnal variation of the flux was added. This demonstrates that the diurnal variation is small.
7. Fig1, 2, 3, 5 was kept as before but improved in some specific details.
8. Fig 4 was replace by a new figure which showed not just flux variation among water levels, but also variation among months and times of day. Furthermore, the new Fig 4 also showed differences between 'natural land' and farmland (which could explain why emission of all sampling plot 'C' looks higher).
9. Fig 6 was improved by including negative fluxes. The relationship between flux and DO was plotted separately as a new Fig 7 which showed better correlations. Relationships between flux and wind was not included anymore as the correlation is very low.
10. Details of plant species found in the littoral zone during each month are listed as a table.
11. Multi-ANOVA was done to show flux variations according to the factors: location, time of year and time of day. Location and time of year are strongly significant, time of day is not.
12. The correlation at natural land and farmland between flux and environmental factors was added.

Revision of discussion

13. The discussion was improved, both in logic and structure. New references were added. 50% of the text was rewritten according to the comments.
14. Discussion on flux from natural land and farmland of the present study was added.
15. A brief conclusion paragraph was added at the end answering the objectives and addressing the important hypothesis raised in the introduction.

For one-to-one response to each comment, see below please.

Anonymous Referee #1

General comments

This is a study of N2O emissions from a reservoir in China. Based on the area change upon a difference in low and high water level of 5 m the reservoir appears to be shallow over large areas. The sampling design seem rigorous by covering many water level regimes, being based on multiple sampling over the year to cover different seasons, diel sampling at each sampling day(?), flux chamber replication in space both taking nearby and more remote spatial variability within each water level zone. This extensive sampling gives the study a high potential for increased understanding of variability in space and time including spatial variaility by water level, spatial variability by vegetation types, diel variability, and seasonal or Monthly variability.

At the moment I do not think this potential is fully explored. There are many levels of variability studied that is not even mentioned in the paper. Further, the aims and unique contributions of the paper are not clearly expressed. The data is analyzed based on primarily single correlations and regressions without any outspoken strategy in terms of trying to explain different type of variability occurring at different levels in space and time with different environmental variables having synchronous variability. One way to approach this is to ask "What variability was greatest and need most attention in the future?". The "dimension" with greatest variability also dictates what environmental factors are likely to be important regulators. To just give an example (perhaps not relevant here): If the diel variability is greater than other types of variability, then it is not likely to find strong correlations with daily averages of environmental factors and variables having diel variability is needed to explain the observed diel flux patterns. It is not clear how such considerations are made when looking for correlations with environmental factors.

Several significant relationships are presented but the predictive power is very low and graphically it looks like the type of situation where statistical significance is reached because of a large number of data points, while the significant patterns do not help us gain new clear or improved understanding because of low predictive power. Some of these cases perhaps, and interestingly, point at a decoupling between N2O fluxes and environmental variables. Finally, the implications of the study are not explained clearly and with the amount of data available it would be nice to try to expand the results into more general implications in a clear way. I think this study has great potential if just these issues and the other comments below are considered carefully.

R: Diurnal sampling was done for each sampling day, seven times per day (including night).

Flux variations and correlations on different scales were analysed at different sampling positions, time of year and time of day. Flux variations at different spatial and time scales were shown in a new figure. The correlations at different levels in space were showed in the revised manuscript, but not at different time scales because no appreciable difference or big improvement in r was observed. Discussion of the reasons for the low coefficients was improved or added including the presence of non-linear relationships which would lead to low coefficient in simple correlation analysis, and constraints of soil moisture and nutrients which might inhibit the velocity of N_2O production and the apparent responses to other environmental parameters.

Detailed comments

Abstract Line 7: Unclear what control site means here as stable control conditions may be difficult to maintain under fluctuating water. Please clarify in what way these two sites served as control. (This is explained in the later text so this comment is about clarity for those only reading the abstract. However I think the word control site signals something else than what is the case here and what is called control site here does not stand out as very different from the other sites with stable water moisture, e.g. the NF site, so I wonder if it would not be good to omit using the word "control" to reduce the risk of confusion.)

R: This control area (SFC) was set as a control for the seasonal flooded area (SF). It had more or less the same vegetation and similar soil conditions as SF before SF was flooded. SFC was assumed as a substitute for SF to explore what the flux would be if there was no water level fluctuation. It is on slightly higher ground and so it was not flooded. More specific statement is now given in the abstract and elsewhere.

L17: Were N2O and CH4 measurements performed simultaneously or at different times? This is essential for the interpretation of the comparison.

R: Yes, N_2O and CH_4 measurements performed simultaneously.

L 18-20: The sentence "It showed that N2O flux and CH4 flux was influenced by distinct factors and in differing ways." is a bit vague. Would it be possible to briefly explain how N2O and CH4 fluxes and regulation differed instead?

R: The sentence has been rewritten in a specific way.

L20-22: Instead of ending the abstract with emphasizing the complexity and challenges – please highlight the unique implications from this study and how it leads forward towards better understanding the complexity and reducing the future challenges.

R: The revised abstract ends with specific implications as "The littoral zone is a hot-spot for N_2O in the summer, especially when the shores of the lake are used for farming of maize. But in terms of the overall greenhouse gas budget, the fluxes of N_2O are not as important as those of CH_4."

1 Introduction After reading the introduction it was not clear to me what the unique contribution of this study will be. I am not contesting the uniqueness of the work but just note that this needs to be clarified. What specific knowledge gaps are addressed that has not been considered properly before? Are there any hypotheses to be tested? Even though I understand the need of descriptive studies tageting similar things at different locations to generate data for later synthesis work, it is beneficial if such studies could also test hypotheses or specifically address knowledge gaps. At the moment,the message I get from the Introduction is that similar work to in a few previous studies is now repeated in a new location, but I think this impression may not be true, and I would wish to learn from the introduction in what way this study is leading forward and providing a unique contribution (e.g. new hypotheses, better study design or measurements...etc; a now location may be fine too if there are very special reasons for believing that this location is important)

Below also a few references that I think could be important in the context of this study (both in the Introduction and Discussion parts) but do not seem to be considered at present:

Guerin, F., Abril, G., Tremblay, A., Delmas, R., 2008. Nitrous oxide emissions from tropical hydroelectric reservoirs. Geophysical Research Letters 35.

Huttunen, J.T., Vaisanen, T.S., Hellsten, S.K., Heikkinen, M., Nykanen, H., Jungner, H., Niskanen, A., Virtanen, M.O., Lindqvist, O.V., Nenonen, O.S., Martikainen, P.J., 2002. Fluxes of CH4,CO2, and N2O in hydroelectric reservoirs Lokka and Porttipahta in the northern boreal zone in Finland. Global Biogeochem. Cycles 16, 3-1 to 3-17.

Liengaard, L., Nielsen, L.P., Revsbech, N.P., Priemé, A., Elberling, B., Enrich-Prast, A., Kühl, M., 2013. Extreme emission of N2O from tropical wetland soil (Pantanal, South America). Front. Microbio. 3.

R: The introduction was revised. More statements focused on the specifics of the present work. We think the big improvement of this research is the sampling both in space and time which was expected to provide more representative data on N_2O emission for the littoral zone to match its diverse and dynamic emvironment. Another way in which the work is 'special' is the possibility of comparision with the CH_4 fluxes. Also the impact of the opportunitic agriculture (maize crops). The over-arching hypothesis in this work is: the littoral zone is a hot-spot of N_2O emissions that is influenced by seasonal changes in the water level. We have stressed these points in the revised introduction.

The reviewer's recommended references are cited in revised manuscript.

Methods and onwards (Page and Line numbers or section used from here).

P5337 L18. Is there any suitable reference for Level II Environmental Quality Standards?

R: Yes, it's a national standard (Environmental Quality Standards for Surface Water of People's Republic of China GB3838-2002), the number of the file and the access website was added in text.

P5338 L7. It is unclear what the site NF is representative for. This is important for future attempts to upscale fluxes from different environments. Please clarify.

R: Site NF was 'seldom' flooded .The water level reaches here only in exceptionally wet years, and not in this year. Explanation was added.

P5338 L24-28. Does opaque here mean that chambers were not transparent to PAR? If so, how could this have affected potential fluxes from plants?

R: The chambers were made of stainless steel. We think it was possible that the artificially induced dark changed the N_2O flux. But based on the previous researches, we cannot make sure how the fluxes had been changed since significant and insignificant differences both have been reported, e.g. Zhongjie Yu et al, 2012 and Dongqi Wang et al, 2009. In addition, the artificially increased temperature in any transparent chamber would make it more difficult to distingush any light effect from any temperature effects.

P5338 L25. What brand of gas sampling bags was used. Has these bags been tested for N2O?

R: The bags are produced by Guangming Research and Design Institute of Chemical Industry, China. This type of bag is designed and produced for gas sampling and analysis. We tested if storage period in the sample bag influences the concentration over one week, but no significant difference was observed.

P5339 L5. Please describe briefly how fluxes were calculated.

R: We added the formula and explanations for each parameters.

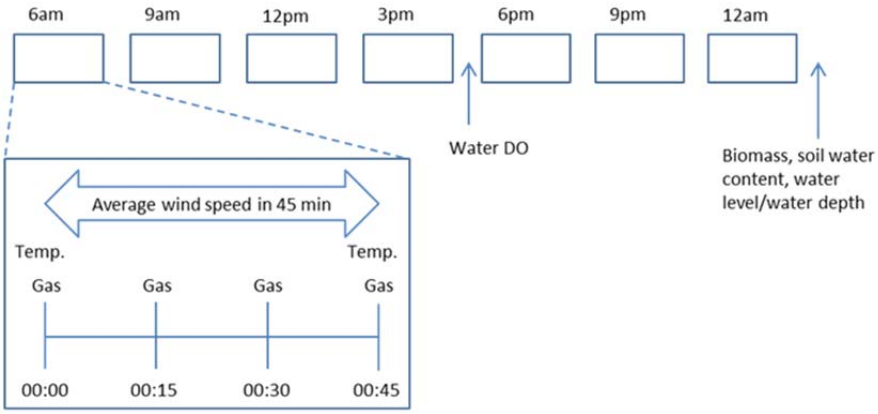
P5339 L6. So, new positions each time. Could this have affected the results by introducing unknown variability? What is known about local variability?

R: Yes, in order to get a better understanding of the relationship between biomass and flux, the plant material inside chambers was harvested after every campaign, so that we could report biomass and see whether it was correlated with flux. Using new positions might introduce a biomass difference since the vegetation growth would not be exactly the same.

In addition, there might be difference in terms of soil nutrients caused by invisible historical events in centimeters to meters scale, e.g. decomposition of necromass (plant or animal).

P5339 L10-16. So the time span of the different analyses varied? Please see comment to figure about drawing lines between sample points implying that data are valid for integration. This may not always be the case.

R: The precipitation was the weekly average which is the only parameter not measured by ourselves. As shown below, the diel wind speed and diel temperature were measured at the same time as the diel flux. Water DO, biomass, soil water content, soil water level/depth was just measured one time per sampling campaign at the location of each chamber.



P5340 L4. How was the soil extracted for pH measurements? There are several common protocols.

R: We used 1:5 soil-water extractions. This is now added in text.

P5340 L9-16. See comment to figure regarding the piecewise regression (which I am not convinced is a good idea).

R: We kept the piecewise regressions with temperature and soil nitrate but deleted the other two (we agree that no considerable piecewise regression exists in their cases). See below, please, for more explaination.

P5341 L2-9. Are the negative results considered? In one figure they were apparently not. I think an equally thorough analysis of the negative results could be interesting. ...I also think that the main fluxes should for at least one value be presented also in mmol m-2 h-1 units to give a reference point for those used to this unit.

R: Yes, the negative results have now been considered. To clarify that, this paragraph was rewritten to make it more clear. And the negative flux was also added in scatter plots so that in the revised manuscript all flux-related figures and tables show both positive and negative flux, i.e. all data. The main flux in units of mmol m^{-2} h^{-1} is now added in result.

P5341 Section 3.3. Figure 6, showing no visible correlation with log-transformed data, makes it very difficult to imagine any important relationships. It is a bit surprising that Table 1 indicates so many significant relationships. The highest r2 (correlation coefficient) is 0.35 vs DO which is very low given that the regression coefficient R2 is the square of r2 right? Further if the piecewise relationship for temp and nitrate is true this should substantially weaken any linear correlation. With enough data points almost all correlations become significant, but at low R2 they may not have any practical meaning. This is an important discussion I think and it is also important to show awareness of this when choosing what

results are most important and should be highlighted from the study. I would consider emphasizing the low R2 and the absence if clear relationships rather than stressing that there were significant relationships.

R: The coefficient in the table was r.

We agree with your opinion. Any non-linear response, including piecewise relationship, should weaken any attempted linear relationship and therefore a low r may not have practical meaning. We added discussion on the possible reasons for the low coefficients including weakening by non-linear relationships and constraints of soil water condition or nutrients.

The scatter plot of water DO and flux were re-plotted using average values of each spot. The r increased to 0.8 (r2=0.65).

P5342 L5. Why is the lowest flux noted in the text -2.29 when much lower fluxes are noted and visible in Figure 6 (as low as -27). If many negative fluxes are ignored very good reasons for this should be given. At present I do not understand how data were treated and how to interpret the results...and this undermines my confidence in the study. Please make necessary clarifications.

R: This was caused by taking an average at different time scales, i.e. the -2.29 was lowest montly average flux while the -27 was the lowest daily average flux. The conflict is now dealt with in the revised manuscript. To avoid confusion, clarifications on calculation methods were added when necessary in revised text.

P5342 Section 4.1. Why not also refer to Table 2 for comparisons with other studies?

R: Added.

P5343 L1-16. CH4 fluxes and thereby the N2O to CH4 ratio cannot be properly evaluated without more information about the CH4 fluxes. Were they measured from the same hambers (if so good; if not comparability can be compromized by spatial or temporal variability)? Is ebulliton included or not in the CH4 fluxes?

R: Yes, N_2O and CH_4 was from the same chambers. All gas samples were analysed at the same time using gas chromatography for both N_2O and CH_4 concentrations. Unfortunatly, we did not collect ebullition gases using inverted funnel or similar equipment. Ebullition might have occurred occasionally, but it's hard to make sure.

P5344 L15-17. Does this mean that there may be a flooding pulse in N2O emissions for a few hours that is likely missed if there is not continuous sampling? If so, what does this mean for the interpretation of the presented results?

R: It might be. So the observed emission might be lower than the real and high frequency monitoring would certainly be better. A few lines are added to acknowledge that fact.

P 5345 and onwards - Section 4.3.2 - 4.3.4. I am not really convinced by this discussion because I am not sure there are any clear relationships between N2O fluxes and the environmental variables in this study. Significant regressions do not mean much if there are many data points and low R2. I would try to reanalyze the data and combine fluxes and variables acting at similar time scales. I would also try multiple regressions trying selected combinations of variables. If this does not reveal any stronger relationships the data may even indicate decoupling between flux and many environmental variables thought to be important, which is also interesting.

R: The correlation was analysed seperately according to different months, water levels and locations of the chamber. No improvement was obtained. The r of linear, and several non-linear multiple regressions including single or multiple factors also was low, just as in simple correlation analysis.

The discussion on wind speed was deleted because of the low r. The plot of flux with temperature, soil nitrate and water DO was re-plotted. It makes the relationship clearer. In addition, discussion on the likely reasons for low coefficients was added in this section.

Another question - why is not the diel variability shown and discussed more if the data exist?

R: Diurnal variation is now added.

P5347 L20-22. I do not understand the meaning of this sentence. Can it be clarified?

R: Soil moisture of the littoral zone is patchy and ranges from flooded to seasonally dry. Besides rice growing, crops do not tolerate flooding or drought. A more specific statement was added.

Section 4.4. I miss a discussion of the implications of this study.

R: One paragraph was added at the end, see below, to discuss the implications of this study, and we revisit the hypothesis as well.

"Finally, we return to our original hypothesis, which was: the littoral zone is a hot-spot of N_2O emissions that is influenced by seasonal changes in the water level. We find that the littoral zone is a hot-spot for N_2O in the summer, especially when the shores of the lake are used for opportunistic farming of maize. But in terms of the overall greenhouse gas budget, the fluxes of N_2O are not as important as those of CH_4."

Figure 1. I think then concept of this figure is nice. It seems that the figure includes some information that is not mentioned in the legend (e.g. difference between high WL and low WL and why SFC is referred to as a control and not just a different regime as any of the others). I do not understand the distances noted between sites A, B and C and would prefer to not have to find another paper to check this up. Can the legend be further clarified so that all its parts can be understood independently from the text?

R: More details are given in the legend, including but not limited to those you suggested.

The distances noted between sites A, B and C was in the horizontal. Species of A, B and C were listed in the revised manuscript as Table 1.

Figure 2. Both wind speed and air temperature are highly variable over the day. What is really shown in the graphs? Is it snapshot measurements indicated with the points (if so I wonder if interpolation is valid as the time of the day for the sampling may be critical) or is it some kind of daily or weekly average?

R: The air temperature and wind speed shown in Figure 2 was the daily average value which was monitored when gas samples were taken, i.e. wind speed was the mean of seven 45-min averaged wind speeds in one day while air temperature was the mean of 14 snapshot measurement in one day, as shown below.

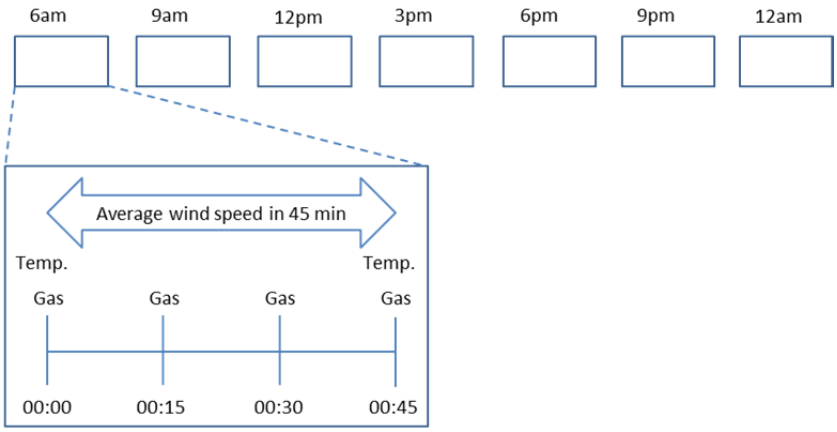


Figure 3. Interesting that the sum of NH4+ and NO3- is substantially lower at SFC than at the other sites. Why is that and could this be of importance when interpreting the data?

R: Besides maize, alfalfa (for cow feed) is another crop grown along water edge. One piece of land is not always maintained for growing maize or alfalfa, there is unregulated alternation depending on the farmer's view of what might be the most useful. Historical croping differences of land patches might be the reason for the patchy NH_4^+ and NO_3^- of soil. Compared to maize, farmers do not use N fertilizer (or not use as many as in maize cropping) for Fabaceae cropping, since Fabaceae can fix N themselves and grow well without extra N fertilization.

The highest N_2O emission was observed when highest NO_3^- occurred. Low NH_4^+ might inhibit nitrification.

Figure 4. What type of environment is NF representative for? All types of non-flooded soils? A narrow zone of moist soil near the water?

R: We think NF was typical of non-flooded soils. But it's hard to conclude that it represents all types of non-flooded soil since many other factors besides soil moisture also influence N_2O emission; pH for example.