

Interactive comment on “Biogeochemical diversity and hot moments of GHG emissions from shallow alkaline lakes in the Pantanal of Nhecolândia, Brazil” by Laurent Barbiero et al.

Anonymous Referee #4

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Review of “Biogeochemical diversity and hot moments of GHG emissions from shallow alkaline lakes in the Pantanal of Nhecolandia, Brazil”

Summary

I'm sorry that I cannot be more encouraging at this time, and I expect that my review comments are helpful in revising your manuscript and try to improve somehow the meaning of this study. The main objective of this study (I admittedly that it is not completely clear to me) was to study the role of some physicochemical parameters in the greenhouse gas emissions-GHG (CH₄ and CO₂, I think and N₂O was integrated later as complement) in shallow alkaline lakes in the Pantanal of Nhecolândia, Brazil. To this end, the authors performed several campaigns at different years at different seasons

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in green and black water lakes (“freshwater lake” was omitted in the GHG results and discussion, you should remove all about this lake). I think, this manuscript contains several major weaknesses as pointed by other reviewers. For example:

Introduction

The introduction should refer more to literature of the studied type lakes (ponds) in GHG emission topics, and avoid integrate terms that are out the scope of the study and avoid unnecessary statements, for example:

(i) process-based models are mentioned but never used in the manuscript as tool, or even in the discussion of the results,

(ii) please refer properly the ideas and references in the introduction; Saunio et al. (2016) is a neat analysis of the global methane budget, but not for CO₂ and N₂O, then, you need to include literature about the topic (I would remove N₂O, since it is a last graph with few case of the study lakes and only for “green water lakes”). There are incorrect citations for the meaning of the sentences; Bogard et al (2014) is not a good citation for the meaning of the sentence,

(iii) include more introduction about your type of greenhouse gases studied, there are few information about which gases were measured.

2 The method section is lacking some important information. To mention only a few:

(i) The most important: there is no statistical section, then, there is no idea how you determined significant differences, linear analysis, how many samples per site, time and lake were done.

(ii) You need at least a reference to indicate the advantage of the method and/or a comparison between static chamber and your namely “dynamic chamber”. What is the purpose of it? Why didn't you set several static chambers and measure it? The way that you collect the gas samples is very difficult to understand. Finally, what is the purpose to use two different methods and no mentioned in the results and discussion

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sections?

(iii) There is an important missing information in the gas sample procedure about the manual pressure procedure. This is a critical problem, because if you don't know the % of vacuum made, you didn't know how much dilution contain the sample injected into the vial. Did you have a pressure manometer to measure it? What is the error of the manual vacuum pump and how much volume you can extract from the vials? I said this because it represents a systematic error that may increase bias in your measurements and may be explain your error bars and not the attribution that you made to Ebullition.

(iv) The calibration for CH₄ is wrong as you did it, since you used CH₄ standard 10 times over to the atmospheric concentration. So, maybe you will have critical bias in the calibration curve comparing data below to 690ppm from the calibration curve.

(v) Section 2.2.2. title should be "physicochemical analysis" (or similar), since using the title "Biogeochemical field indicators" is very vague, according to the measurements made in the study.

3 Results sometimes are discussed (in the result section) in speculative way for example:

(i) I cannot see Fig 3. Opposite trend mentioned in page 35 Lines 26-29.

(ii) Carefully in the temperature results, you didn't measure at the same time (even you measured different years I think), then, temperature fluctuations is due that environmental conditions during a day, or I am wrong and you measured all lakes at the same day, did you do that?

(iii) There is no term of variation (e.g. standard deviation, standard error, variance, among others) and number of samples in the gas emission section, then I don't believe that statements as in Page 8, line 8 "The differences in the emission values between the floating chambers were moderate".

(iv) Which peaks are in Figure 7, the figure is very confusing, would be better to repre-

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sent in another way (all about gas data), because error bars (also indicate what is the term of variation) are mixing and it is impossible to understand.

(v) How did you know that CH₄ bubbling was moderated (Page 8 Line 12)? the heterogeneity could be as result of moving the chambers, even it is confused why you measure with different methods (static and dynamic).

(vi) Most of the time I need to assume your term of bloom and I believe you, however I cannot see the trends in the Figure 9 and why black water is not shown. What is the meaning of trend for this study? In some figures, some parameters didn't change along the time, so is it is a trend?, if I am right you need to include in Figure 9 the trend for "Black water lakes".

4 Discussion of the results remains mainly speculative, and the statements are sometimes questionable for example:

(i) Page 9 Lines 16-20 is a very vague discussion,

(ii) Please read the manuscript mentioned in Page 9 Line 29; you are working with ponds and it is out of the scope to mention methane paradox. Grossart and Tang are working with a oligotrophic lake with a very particular conditions, and Bogard et al. (2014) is an enclosure experiment to confirm methane paradox in water lakes. I think your results are more correlated to the microbial activity in sediments but no as production of methane in the water column (in oxic conditions), and your experiment doesn't allows to speculate it.

(iii) I am sorry but section 4.3 is a very speculative supposition, you didn't test any experiment to validate your supposition about microbubbling CH₄. Additionally, you can't mentioned methanotrophy, since you didn't measure methanotrophy activity. Because you showed large gas emissions, it doesn't mean that methanotrophy is suppressed. You are measuring only the total emission; that is the result of the CH₄ produced minus CH₄ oxidized by methanotrophs. Then you don't know the rate of methanotrophy

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activity, which probably is large or small, but you need to have proof to mention it.

(iv) Section 4.4 is very speculative since you didn't measure during rainfall conditions, please remove it.

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