

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 #5

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GENERAL COMMENTS

It was a pleasure to review this well-conducted manuscript that adds to the growing literature on C cycling in highly dynamic tropical lakes. This manuscript entitled “Biogeochemical diversity and hot moments of GHG emissions from shallow alkaline lakes in the Pantanal of Nhecolândia, Brazil” authored by L. Barbiero et al. reveals novel findings on intense but little-known C cycling processes at low latitudes. The topic would be of high interest to readers of Biogeosciences.

However, there are serious flaws that should be considered. My main concerns are:

- The sedimentation rates are not contextualized in the introduction or objective, but

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they are highlighted in the methods, results and discussion. One important point is that this ms would benefit from reporting changes in Organic Carbon Burial instead of Total Sedimentation rates. Authors should include the profile of Organic Carbon Content in the sediment at least in part of the lakes to reduce speculation in the discussion section (page 9, lines 8-20). Lakes studied here could have high organic burial even showing low total sedimentation rates and vice versa.

- Authors should reformulate the study design section to clarify differences of sampling between static and dynamic chambers. Also, they should report what lakes are assessed for each method, as there are figures with 1, 3 or 4 lakes. It's very confused in Figures 2,3, 4, 6 7 8 and 9 if lakes are different lakes or the same in different seasons.

- Overall, all legends are very poor and should be fully revised (e.g. no mention on each lake and season analyzed, number of sampling or even what means symbols and bars, such as a question: Mean and standard error?). In addition, authors should name (e.g. A, B, C. . .) panels of each figure

- The ms would benefit from any statistics treatment for Figures 6, 7, 8, 9 and 10, such as a two-way ANOVA to test the effect of different lakes and time on each key variable.

- The discussion section shows confused subsections (e.g. which were wrong like sedimentation rates within "Diversity of surface waters" or vague like "Specificities of green water alkaline lakes"). All subsection titles in the discussion section might be removed or fully revised. Authors should take care with the expressions "significant" or "significance", as they have not already addressed any statistics with their dataset.

- Also, speculative discussion on aerobic production of methane should be better addressed or removed (page 9, lines 23-31). The aquatic primary producers produce a very labile OC substrates to methanogenesis and their blooms could favor anaerobic production in the sediment, which is not necessarily oxic as waters. Indeed, few millimeters within the sediment might be enough to get anaerobic mineralization sites (see Sobek et al, 2009, Limn. & Oceanog.). Your study design does not allow interpretation

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on aerobic methane production in these shallow lakes.

- Other unnecessary speculative discussion is the role of CH₄ microbubbles to the total outgassing (page 10, lines 18-21). Authors should compare dissolved CH₄ in surface waters with that evasion rates from chambers. They have a clear study design to confirm the role of bubbles on CH₄ evasion to the atmosphere, which is not properly considered. Finally, authors should cite references to their comparisons (page 10, lines 21-22). In relation to air-water CO₂ fluxes, authors should discuss your results with the global review for alkaline lakes from Duarte et al. (2008, J. OF GEOPHYSICAL RESEARCH)

- It is not clear how could authors interpret from their results the influence on early rainfall in this subsection of discussion. The ms show same lakes before and after the rainy season. Therefore, this discussion might be possible, but the authors do not explore their results.

- In order to better address the upscaling, authors should clarify the source of the number of days without and with moderate or intense phytoplankton blooms (e.g. do they have any own dataset or only visual impression from these lakes? Or other source?). In addition, authors did not discuss any role of the observed daily variation on the upscaling.

MINOR COMMENTS

- Authors should revise the confusion related to the term “algae blooms”, as other primary producer considered important (cyanobacteria) is not algae. A better term might be phytoplankton bloom. They should revise this term over the whole text.

- References are lacking in the analytical methods (e.g. page 5, section 2.2.2), and a fully revision is still needed for each method.

- Authors should include a point after the term “wetland” in the page 9 (line 11).

- The sentence “Consistent with Martins (2012). (...) throughout the season” (page

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9, lines 7-8) should be rephrased to anything like “Our results confirmed previous evidences on the different functioning of black and green water alkaline lakes (Martins, 2012), ...”

- What season do you mean in this complement “despite a very close mineral chemistry throughout the season” (page 9, line 8)?

- Page 10 (line 12): “strong sunshine” or “high incidence of solar radiation”?

- The terms “fast or quick calculations” over the text should be replaced to anything like “rough estimates” (e.g. page 11, lines 5 or 14-15).

- Authors should clarify that this ms assessed the variability over time and not spatially within the lake. They might complement the sentence “within the lake” to “within the lake over the daily cycle and year seasons”.

- It's vague the sentence “. . . to estimate GHG emissions” (page 11, lines 20-21). What do authors mean? A regional or a global context? The GHG emissions of one of the freshwater wetland of the world? In addition, this conclusion without any argument seems speculative.

- Figure 1: Images need scale and source.

- Figure 4: What exactly means filled and dashes lines or the arrow? This kind of description should be also in the legend.

- Figures 6 and 7: Air-water fluxes and dissolved concentrations of a given gas should be in a same figure with two panels

- Figure 9: I did not understand why both CO₂ and CH₄ of the lake P is separated in another figure? Authors should organize all data in a same way among figures.

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