

Interactive comment on "Rooting and plant density strongly determine greenhouse gas budget of water hyacinth (*Eichhornia crassipes*) mats" *by* Ernandes Sobreira Oliveira Junior et al.

Anonymous Referee #1

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The link between trace gas emissions and biological invasions is still poorly explored and deserves attention from the scientific community. In this paper, the authors try to elucidate the effect of water hyacinth density on net GHG emissions, by using a mesocosm approach and by measuring net CO2 and CH4 diffusive and ebullitive fluxes under controlled conditions. My general opinion is that, even if the topic could be attractive to the readers, the experimental design, the initial hypotheses and final results here presented are not enough substantial to be published in Biogeosciences. My decision is therefore to reject, and I encourage the authors to resubmit in a minor journal. Please find some comments that I hope will help in revising the manuscript.

TITLE The term 'mats' is not appropriate, since you are working in small mesocosms.

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I would not use the word "strongly", as only a part of your results is statistically significant. All along the paper, I'd rather talk about biomass and not density, as density takes into account the weight of a single plant, and this can be very variable.

INTRODUCTION In general, the introduction is excessively focused on the water hyacinth; you could improve the text by citing other studies carried out on other weaklyanchored hydrophytes, such as Trapa natans, or other floating-leaved rhizophytes (Nuphar spp.). Your results from not-rooted mesocosms could even be compared to those found within free-floating plants (Lemna spp. mats, or Azolla spp.). The following studies could give the reader some insights into the topic:

- Bolpagni, R., Pierobon, E., Longhi, D., Nizzoli, D., Bartoli, M., Tomaselli, M., & Viaroli, P. (2007). Diurnal exchanges of CO2 and CH4 across the water–atmosphere interface in a water chestnut meadow (Trapa natans L.). Aquatic botany, 87(1), 43-48

- Grasset, C., Abril, G., Guillard, L., Delolme, C., & Bornette, G. (2016). Carbon emission along a eutrophication gradient in temperate riverine wetlands: effect of primary productivity and plant community composition. Freshwater Biology

- Caraco, N. F., & Cole, J. J. (2002). Contrasting impacts of a native and alien macrophyte on dissolved oxygen in a large river. Ecological Applications, 12(5), 1496-1509

- Ribaudo, C., Bartoli, M., Longhi, D., Castaldi, S., Neubauer, S. C., & Viaroli, P. (2012). CO2 and CH4 fluxes across a Nuphar lutea (L.) Sm. stand. Journal of Limnology, 71(1), 21.

* page 4 line 17: I believe that the expected effect of rooting on GHG emission should be better explained and justified. I do not think that 'chimney effect' is the right term for the ecophysiological mechanism you are referring to. Indeed, the mechanism of gas transport through the macrophytes aerenchyma is widely defined as 'convective flow' or 'pressurized flow' and implies complex interactions between aerenchyma structure, internal pressure and gas concentration. I do not think that Bastviken (2009) is the right reference, try instead with:

-Grosse, W., Armstrong, J., & Armstrong, W. (1996). A history of pressurised gas-flow studies in plants. Aquatic Botany, 54(2), 87-100

-Konnerup, D., Sorrell, B. K., & Brix, H. (2011). Do tropical wetland plants possess convective gas flow mechanisms?. New Phytologist, 190(2), 379-386 *page 5 line 6: The hypotheses of the study are not clearly stated; what are you expecting as a result of the experiment?

METHODS I think the main weakness of the experimental design is the lack of replicates. Each treatment is tested only on n=4, which is not adequate in order to obtain a robust result. From the description, I understand that you measured fluxes on 3 different dates and then pooled the results together and expressed as mean \pm error. Well, this makes 12 pseudo-replicates, not factual replicates.

*page 6 line 3: "...phosphorous propagules." I think the verb is missing in the sentence.

*page 3 line 9: If 100% treatment is given by 413 g, how could 50% treatment be given by only 160? Maybe it would be better to talk about high/low biomass instead of density/coverage and number of plants.

*Paragraph 2.3. This paragraph needs to be more explicit and detailed. Maybe I'm missing some important information, but I do not see how you can be sure that no CH4 bubbles were emitted while you measured "diffusive" fluxes. Also, how long the incubations lasted, during the day and during the night? It would be interesting for the reader to see the possible difference between day and night results, especially for CO2 fluxes. In general, it would be interesting for the reader to see the values corresponding to each date of measurement, for both diffusive and ebullitive fluxes.

I have a serious concern about the length of your incubation for measuring ebullitive fluxes. In literature, you can find many papers demonstrating that small volumes of air in the headspace (around 6 liters above the water, in your case) can be quickly

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saturated in CH4. Thus, if you measured only T0 and Tf after 24h, you most probably underestimated your flux, because the slope of your regression was affected by saturation in the headspace.

Of course, the degree of saturation in the headspace would depend on the CH4 concentration in the water; I think that it will help to show the dynamic of CH4 in the water throughout the duration of the experiment.

*page 7 line 21: how did you sample the headspace? Were the samples transferred to vials or directly injected in the GC? Which volume of injection? Please be more precise.

*page 8 line 12: Is it a total biomass (leaves+petioles+roots)? How did you measure the fresh weight? Is it a standard error or deviation? As I can understand, this is the weight of a single plant. But in Methods section you indicated a plant weight was 160 g (as FW or DW? Error?). Did the weight of a single plant changed throughout the 50 days experiment?

*page 8 line 21: replace by "Statistical analyses"

*page 9 line 13: To which water sample does this sentence refer to?

*page 10 line 5: there should be a mistake relating the name given to the figures (S1A, S2...).

*page 10 line 18: If some results are not significant, then do not give importance to them. Statistical analyses did say they are not significant.

DISCUSSION This section is confusing and should be reorganized. Before doing that, you should clearly state your hypotheses in the Introduction section as simple questions which can be answered by yes/no or true/false. Then reorganize your discussion answering those questions. At present, the discussion contains many elements which are not pertinent. Many references are not appropriate.

I also think that you should spend some considerations on the impact that highproductive plants have on sedimentary processes in correspondence of periods of senescence of plants. That is, in late summer and fall, the sedimentary oxygen demand could increase and enhance CH4 and CO2 benthic fluxes. What is happening when the plants are not fixing as much carbon as in summer? What about the build-up of dead biomass on the bottom? Which could be the real C budget on a complete year?)

Please do not put again the reference to the figures in Discussion section.

*page 12 line 5: the reference to a submitted article is not suitable.

*page 12 line 12-15: those sentences are not supported by results, please remove them. If you really expect what you wrote, please support those sentences with references and put them in the Introduction section as a part of your initial hypotheses.

*page 13 line 15: Typha domingensis is a helophyte. You'd rather talk about weaklyanchored or free-floating hydrophytes in your discussion.

*page 13 line 21: Based on your results, obtained from small mesocosms, you cannot extrapolate to "open waters".

Tables: Authors must use standard deviation and not standard error of mean, the former referring to data variability around mean of a sample of population (this is your case), the latter referring to precision for an estimated population mean (this is not your case).

C5

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