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Biogeosciences Discussions

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.

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We would like to thank Referee 2 for her/his comments and suggestions. Please find our itemized list of responses below, as well as our revised manuscript (with tracked changes). Our responses are structured as follows: (1) comments from referees/public, (2) authors' response, and (3) authors' changes in manuscript indicating the page and line of the changes when applicable.

1) I think that the paper by Oliveira Jr et al., titled "Rooting and plant density strongly determine greenhouse gas budget of water hyacinth (Eichhornia crassipes) mats", is an interesting manuscript, with some novel aspects of the contribution of free-floating macrophytes in regulating GHG gases balance; especially considering the role that

the rooting may play in CO2/CH4 release or consumption by regulating the interaction between pleustophytes (their root system) and sediments. My main reservation with this manuscript is that it seems to be a "local study" with few data, and is lack of global sound to fit the Biogeosciences' targets.

2) Thank you for highlighting the novelty of our paper. We strongly feel that the global relevance of our paper is demonstrated by the wide-range and rapid spreading of the water hyacinth in tropical and subtropical areas, and its expected large-scale effect on greenhouse gas (GHG) emissions. A few field studies indeed focus on water hyacinth's effect on GHG emissions, and although it is clear that the effect can be strong, these studies contradict each other with respect to the direction of the effect. Apparently, large-scale field studies alone are insufficient to explain these differences and to pinpoint the underlying processes causing a change in GHG emissions in water hyacinth stands. Therefore, we set up a controlled laboratory study, to elucidate the biogeochemical causal mechanisms involved, which we consider this a vital part of studying this global issue. To explain the global relevance and urgency of our experimental study, we now stress to the global relevance of our work both in the introduction and in the discussion (page 5 lines 4-17 and page 13 lines 6-11). Please also see our reply to the first remark of referee 1.

3) Page 5 lines 4-17; Page 13 lines 6-11.

1) In my opinion, two major aspects have not been adequately addressed in the manuscript. The first one is the actual sink role of the water hyacinth. I believe that this plants, as well as large part of the aquatic phanerogams with very fast life cycles can be efficient sink of C, but only "temporary". They produce a lot of fresh biomass that just as quickly goes towards a rapid degradation/mineralization. This aspect needs to be discussed in detail in order to correctly evaluate the contribution of this species to the GHG balance

2) We agree. Indeed, it is possible that a large part of the plant biomass will decom-

pose after senescence. This will liberate the C taken up by the plant as CO_2. The burial efficiency of C taken up by the plant may well vary among systems. We now explicitly mention this in the introduction (page 6 lines 1-9). A very interesting question is: if the plant material decomposes, is it emitted back to the atmosphere as CO_2 or as CH_4 (CH_4 being a much stronger GHG than CO_2)? As the plants can create anaerobic conditions in the water column, they create favourable circumstances for methanogens and systems dominated by the species may therefore act as net GHG emitters irrespective of the C balance. In our experimental systems, for instance, the CH_4 emissions increased 17-fold on average comparing control and high coverage treatment. We now mentioned this important fact in the discussion section (page 13 lines 19-20 and page 14 lines 1-3).

3) Page 6 lines 1-9; page 13 lines 19-20 and page 14 lines 1-3.

1) The second aspect is related to the mechanism that probably underlies the observed increase in methane emission with increasing the biomass of the species and the roots/sediment interaction. The authors talk about a "chimney effect" based on the possibility of the plant's roots to carry the gas into the atmosphere. I do not know what is actually the mechanism, however the contact of the roots with the sediment stimulates a complex series of biogeochemical processes that can be taken into consideration in the present research. For example, it could be very interesting to know whether there are any differences in terms of oxygen availability (as well as for nutrients) in the treatments with mesh, above and below it. The availability of oxygen and methane along the water column (beneath the plant mat) could integrate the presented results, supporting them in a more appropriate way.

2) Although we did not determine the exact mechanism underlying the plant-mediated transport in the water hyacinth, it is likely that it is largely driven by convective flow and passive molecular diffusion (Konnerup, 2011). We also refer to our comments to Referee 1. We now mention this explicitly in the introduction. We also included new references (Dacey and Klug 1979; Grosse et al. 1996; Cronk and Fennessy

2016; Konnerup et al. 2011) (Page 5 lines 1-2). We also replaced the term 'chimney effect' for "plant mediated transport" throughout the manuscript (e.g page 5 line 4). We indeed agree that it would be interesting to study CH_4 and O_2 concentrations over a vertical gradient. However, as the water depth was only 38 cm in our experiment, we assumed concentration gradients within an aquarium to be of minor importance as compared to the concentration differences between treatments. We presented the O_2 concentrations (measured at a depth of 20 cm) in figure 2 of the manuscript. We unfortunately do not have data on CH_4 concentrations in the water layer.

Cronk, J. K., and M. S. Fennessy. 2016. Wetland plants: biology and ecology. CRC press.

Dacey, J., and M. Klug. 1979. Methane efflux from lake sediments through water lilies. Science 203: 1253-1255.

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

3) Page 5 lines 1-2; e.g. page 5 line 4.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-297/bg-2016-297-AC4-supplement.pdf

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