

# ***Interactive comment on “Tidal controls on trace gas dynamics in a seagrass meadow of the Ria Formosa lagoon (southern Portugal)” by E. Bahlmann et al.***

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Reviewer 3 The manuscript offers new and necessary insights into the understanding of trace gas dynamics in coastal ecosystems affected by tidal cycles. The strong influence of tides on CO<sub>2</sub> and CH<sub>4</sub> fluxes, reported in this study, are noticeable and worth publishable, with important implications in sediment-water and water-air fluxes of green house gases. However there are some points to consider (in order of importance) before publication in BG:

1) Some concerns arise when considering the design of the dynamic flux chambers used for the measurements. This has been a recent delicate issue that should be de-

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tailed and discuss carefully. The authors are aware of these concerns and an extended explanation of the methodology is given, but some clarifications should be added. I basically agree with the comments, regarding this issue, of Anonymous Referee #1.

We provided an extended description and discussion of the chamber design. See the response to reviewer 1.

2) Regarding the VOC results: I don't really see the point in delivering all the VOC information in the paper, unless there is a better and deeper discussion of the results. In my opinion, the results are shown in such a way that interpretation is not straight. COS, DMS, propane and butane are expressed as relative "enhancement" to the averaged flux, which makes it hard to compare with the rest. I suspect this is because they are not present in the standard used and hence no absolute magnitude can be computed. In any case, I suggest the authors keep the halocarbons data but remove the S-Compounds and Hydrocarbons data. Indeed, if they finally decide to remove also halocarbons data and just focus on CO<sub>2</sub> and CH<sub>4</sub> fluxes I would still suggest its publication. In that case, I definitely suggest a section in the discussion where the biogeochemical mechanisms potentially modulating these fluxes debated.

Regarding the VOC data see the response to reviewer 2. A discussion of the biogeochemical mechanisms potentially modulating the fluxes would surely help to complete the picture. However, we cannot substantiate this with additional data such as sediment oxygenation and trace gas profiles in the sediment. Thus such a discussion would remain superficially and become speculative. For these reasons we decided to discuss this not in more detail.

3) The results are discussed with detail but I miss in the discussion or conclusions an expanded view of significance and repercussion derived from the unexpected CO<sub>2</sub> and CH<sub>4</sub> results. There is a tentative up-scaling calculation using their CH<sub>4</sub> flux data and a global seagrass coverage area. I don't really believe in this kind of global up-scaling estimations. Seagrasses encompasses a huge range of different ecosystem

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conditions that do not necessarily mirror Ria Formosa lagoon conditions. Instead of calculating global emissions, I would recommend to discuss and compare the trace gases emissions measured with those of other seagrasses worldwide. Also, how is climate change affecting Ria Formosa lagoon tidal cycles? Discuss possible effects on changes of trace gas emissions over the lagoon due to predicted tidal changes.

Though we are confident with our results we think that these unexpected flux dynamics require further substantiation. Therefore we have been careful in discussing the repercussions from these findings restricted ourselves to the statement the following statement (pp 10588-89, L. 27-4)” Hence, our results highlight the importance of accurately addressing the perturbations of turbulent flows in flux chamber studies. If the observed flux enhancements are more than just episodic events this may have fundamental implications for our understanding of the carbon and trace gas cycling in coastal environments.” With respect to the upscaling see the response to reviewer 2. A discussion of climate change is clearly beyond the scope of this paper and would be very speculative at this stage.

4) Figures and text can be improved. See specific comments below. Some sentences are too long and not easy to follow. I recommend to check on the writing (some extra commas and points would not hurt) and try to make shorter and more clear statements. Also carefully check the acronyms used. Some of them are not defined (DIC) and some are not placed properly in the sentence (NCP). Double-check also the chronological order of the references within the text.

We have carefully checked the text and the acronyms and provided definitions for all of them. See also the responses to the specific comments below and to reviewer 2.

Specific comments:

P10573, L11-12. This is a false statement. The development of benthic chambers for underwater incubations is far older than 2006, and have been used for the purpose of both trace gas and nutrient fluxes. The authors should be aware and refer to pub-

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lications other than Silva et al. 2008 and Barron et al. 2006 (e.g. Nicholson et al. 1999, Larned 2003, Ferron et al. 2009). G. J. Nicholson, A. R. Longmore and W. M. Berelson. Nutrient fluxes measured by two types of benthic chamber. *Marine and Freshwater Research* 50(6) 567 – 572, 1999. S. T. Larned. Effects of the invasive, non-indigenous seagrass *Zostera japonica* on nutrient fluxes between the water column and benthos in a NE Pacific estuary. *Marine Ecology Progress Series* 254: 69–80, 2003. S. Ferron, S. F. Alonso-Perez, T. Ortega, J. M. Forja. Benthic respiration on the north-eastern shelf of the Gulf of Cadiz (SW Iberian Peninsula). *Marine Ecology Progress Series* 392: 69-80, 2009 We apologize for these omissions and include the references into the manuscript.

P10573, L16-17. Provide a reference for that statement. Suggestions: Armstrong 1979, Larkum et al. 1989. W. Armstrong. Aeration in higher plants. *Adv. Botanical Res.* 7:225-332 AWD Larkum, G. Roberts, J. Kuo and S. Strother. Gaseous movement in seagrasses. In: Larkum AWD, McComb AJ and Shepherd SA (eds). *Biology of Seagrasses*, pp 686-722. Elsevier, Amsterdam. We added both references.

P10573, L29-P10574, L4. References in each of the problems stated should be added. We have added references discussing these problems in detail.

P10576, L26-27. You indicate that the lines were sampled for 5 min. Indicate also how often the sampling was performed. The sampling lines were sampled consecutively. Thus each sampling line was sampled for 5 minutes with a sampling interval of 15 minutes.

P10577, L20. You say “High time resolution”, but how high is that. If you clarify that in the methods (see previous comment) it is not necessary to state it here again. We think, the “high time resolution” is an important advantage of our approach and thus like to outline it here.

P10581, L1-4. Too long sentence. Please use commas, re-phrase or use two short sentences instead of a long one. We have split the sentence. It is now:”The authors

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of this study did not come up with a conclusive explanation for this observation. They suggested either lateral transport in the sediment in combination with salinity gradients affecting the source strength and/or enhanced gas ebullition due to increased pressure from the water column.”

P10581, L10-12. In the sentence “Variation in the :: ” : this statement is true only of you consider lineal variation. Add “lineal” between “ a” and “change”. We have added linear as suggested by the reviewer.

P10581, L17-18. References should be added. Some references are given in the following sentence. We added additional references for higher fluxes during low tide attributed to deep pore water circulation in line 21 and removed the sentence at the end of this paragraph (P.10581 L.27-29)

P10584, L3-6. It is impossible to appreciate that in the figure 2 and hence is hard to follow this sentence and some further discussion. I suggest that y-axes are re-scaled in Fig 2 accordingly. We have rescaled the y-axis for CH<sub>4</sub>. See also the response to the reviewer’s comments to fig.2.

P10584, L8-10. Too long sentence. Add some commas (after “emission”, “methane peak”, and “pore water”), or re-phrase. We have added commas as suggested by the reviewer.

P10584, L13. I could be wrong, but I don’t think DIC acronym has been described. If necessary please do so. We have done so. See also the reply to reviewer 2.

P10585, L14-18. Re-phrase using shorter sentences. We have done so and changed the order of sentences. See response to reviewer 2.

P10585, L18-24. I recommend to remove this paragraph. In my opinion is too speculative. Seagrasses encompasses a huge range of different ecosystem conditions that do not necessarily mirror Ria Formosa lagoon conditions. Instead of calculating global emissions, I would recommend to discuss and compare the trace gases emissions

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measured with those of other seagrasses worldwide. Also, how is climate change affecting Ria Formosa lagoon tidal cycles? Discuss possible effects on changes of trace gas emissions over the lagoon due to predicted tidal changes.

We feel confident with this statement. Even substantially higher fluxes from seagrass meadows as found here, would not change the current perception that coastal marine sources are of minor importance on a global scale. See also the response to reviewer 2. We have compared the methane and CO<sub>2</sub> fluxes to with those from other seagrass studies. For methane this has been done in section 4.2. See also the respond to reviewer 2. For CO<sub>2</sub> this has been done in Section 4.3

P10586, L26. Change the order of “(NCP)” right after its description. Place it after “production”. We have done so.

P10586, L5-10. Too long sentence. Please re-phrase with shorter statements. We have done so.

P10588, L20. Change the word “show” for “suggest” We have done so.

Figure 1. I recommend adding  $C_{in}$  and  $C_{out}$  for clarification We have marked the inlet with air in.

Figure 2. a) I recommend adding the actual tidal change, as tide changes are gradual, such as light intensity. The way it is shown now it simulates an ON/OFF false scenario. b) I recommend to re-scale CH<sub>4</sub> flux Y-axis to better appreciate the changes during tidal changes. c) Consider removing the Temperature. d) Add units in the legend. a) We would like to stay with the presentation of the tidal cycle as it is because the actual tidal height was not measured. b) We will rescale the Y-Axis. See also response to reviewer 1. c) We will remove the air temperature as it is not discussed. d) Units were provided in the legend.

Figure 3. I recommend adding the actual tidal change, as tide changes are gradual. The way it is shown now it simulates an ON/OFF false scenario. See previous com-

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ment.

Table 2. and Figure 4. I suggest considering the possibility of removing S-Compound and Hydrocarbons data. See general comment above. See response to general comments and to reviewer 2.

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Interactive comment on Biogeosciences Discuss., 11, 10571, 2014.

**BGD**

11, C8465–C8471, 2015

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