

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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The paper of Bahlmann et al. presents an advanced way to directly measure fluxes from coastal sites. It was shown that the method works for several gases including CO₂, CH₄ and VOCs. The paper presents nice results over different states of tidal cover, and generally contributes to the understanding of sediment flux dynamics of different gases, especially of CO₂ and CH₄. This could also be a nice way to broaden our understanding of flux dynamics of further trace gases not mentioned. However, I am a bit confused by the presentation of the VOCs. I would appreciate a small section in which it is described how the “enhancement” in fluxes was calculated. Does

C8457

1 mean that fluxes are twice as high as the mean? Which mean was used to calculate the enhancement and why this mean? Also, how could you ensure that this is really enhancement and not the drift of the measurement system if there was no standard available or was their an internal standard? I will further go into detail in the specific comments section. Nevertheless, I feel like this could also be nice for the specific compounds that could not yet be quantified once a standard for them is available. Therefore, I feel like it would be sufficient to mention that these compounds can also be measured here, but I am not sure that they need to be included to the current extent.

The calculation of the enhancement factors is explained in the figure caption of figure 4. The enhancement was calculated relative to the mean fluxes measured during air exposure. Thus, an enhancement of 2 means, that the mean flux was twice as high as the mean flux during air exposure. For the compounds present in the standard the enhancement was calculated from the absolute fluxes given in table 2. The GC-MS was calibrated with a Scott TOC 15/17 standard containing 65 compounds at 1ppm each in nitrogen. 1ml aliquots of the standard were measured 3 to 5 times the day. These standard measurements showed no substantial drift during the day for all components in the standard. Thus we do not expect a drift for the compounds not present in the standard. Given this it is in principal possible to first calculate relative fluxes from the peak area and use these to calculate enhancement factors. Our intention with including these compounds was to show that the flux enhancement during tidal submersion is a common feature of trace gas fluxes in tidal systems that could be observed for all compound classes. Following the reviewers suggestion we will remove DMS, COS, propane and butane because these were not quantified by a standard. But we will keep the halocarbon data and the data for CS₂ and propene to show that the flux enhancement during tidal submersion is a common feature of trace gas fluxes in tidal systems that could be observed for different compound classes.

I generally would advice to check the paper for comma and apostrophe placement (e.g. VOCs is written without apostrophe). Several citations throughout the text are not in

C8458

chronological order. The authors should check for this in the whole text. I would also recommend to check the text for too complicated and long sentences (especially in the discussion section). Some are difficult to follow. I have pointed some out in the specific comments below.

We have done this throughout the manuscript.

Specific comments:

P10572 L25: sulphur-bearing (or without the minus, but it needs to be persistent throughout the text)

This is now sulphur-bearing throughout the text.

P10575 L1-2: Can the authors specify in which direction fluxes were calculated? In L1, the authors measure that they calculate from the difference of the inlet to the outlet, but in the equation this is the other way around. Maybe it could also be helpful to mark the in- and the outlet in Fig. 1, then this would be clear at first glance.

We will specify this and rephrase the sentence in line 1 P10575 as follows: "The fluxes were calculated from the concentration difference between the outlet and the inlet of the chamber. Hence emission fluxes are positive." The flow direction in fig. 1 is marked by arrows. We will label the inlet and outlet in fig.1

P10577: How were the precision and the drift of the measurement systems determined / monitored? How was the working standard prepared? The GC-MS was calibrated with a Scott TOC 15/17 standard containing 65 compounds at 1ppm each in nitrogen. 1ml aliquots the standard were measured 3 to 5 times the day. These standard measurements showed no substantial drift during the day for all components in the standard. A detailed description of the analytical procedure is given in the Supplement of an accompanied paper (Weinberg et al. 2014)

P10578 L9: "following" instead of "followed" We have corrected this.

C8459

P10578 L21: Instead of "circadian" maybe "diurnal" (and if it's not a clear day-night-cycle, then "diel") We have replaced circadian by diurnal.

P10579 L24: Here, the Scott TOC 15/17 standard is mentioned. I think some clarification regarding this standard is needed in the method section (see comment regarding P10577). Was this a liquid standard? A gaseous standard? Etc.

See previous comment.

P10580 L3: Here, clarification on how the enhancement was determined is severely needed. And why is "enhancement" discussed for the halogenated VOCs when these could actually be quantified? I understand that this is probably for comparison with the other gases, but I don't really see the point when for example CO₂ and CH₄ are also given in absolute values and COS and CS₂ aren't really discussed. Either this concept needs to be better explained or I would use the total number. The concept of "enhancement" as it is used here is quite difficult to grasp.

We have added an explanation of the enhancement here. The sentence is now: "The average enhancement during tidal immersion (relative to the average fluxes during air exposure) ranged from 4 – 12 for CS₂ the halocarbons CH₃Br, CH₃I CHCl₃ and CHBr₃." The absolute fluxes of the halogenated VOCs are discussed in detail in an accompanied paper (Weinberg et al. 2014). The main focus of this manuscript is on the tidal controls on trace gas fluxes. For this purpose we found the enhancement more illustrative because it makes the tidal effect comparable among different trace gases. This is hard to show using absolute flux values. Thus we would like to stay with the enhancements here. See also previous comment.

P10580 L16: Is it possible that there is a word missing behind "high"? E.g. "tide"? This is true. We have inserted tide here.

P10581 L4-6: Please rephrase this sentence. Something is messed up in the word order. Maybe it would be helpful to divide the sentence. I would also recommend to break

C8460

up the following sentence as it is difficult to follow as well. We followed the reviewers recommendation: "The Ria Formosa lagoon has a negligible inflow of freshwater and a year round salinity between 35 and 36 PSU. This makes salinity driven lateral changes in methanogenesis and benthic respiration implausible."

P10582 L17-20: I would recommend breaking this sentence into two. We have done so and further changed the order of the sentences: "In accordance Yamamoto et al. (2009, noted a concurrent increase of the redox potential of the sediment with increasing CH₄ and CO₂ fluxes during tidal inundation. The CH₄ fluxes observed in the Ria Formosa lagoon provide a mirror image of these oxygen dynamics."

P10582 L24: Not sure that "confounding" is the right word here. Maybe "interfering with" instead? Following the reviewers recommendation we have replaced confounding by interfering.

P10584 L1-6: I am a bit confused by this explanation. I thought that CH₄ fluxes were higher in the night (P10578 L6-7)? This doesn't get clear here in the discussion. Higher methane fluxes during night were only observed for the methane peak. See page 10578. However we don't get the point here as this section does not deal with day night differences. We referred to the night time fluxes in order to compare the respiratory CO₂ with methane.

P10584: I would abbreviate "Methane" here as was done in the remaining text. We have done this throughout the manuscript.

P10584: Please introduce the short explanation for DIC, some readers might not be familiar with this abbreviation. We will do so: "(DIC = Σ CO₂*+HCO₃-+CO₃2-)"

P10585 L13-17: Please rephrase. This sentence is too complicated to be able to follow it to the end. We have rephrased the whole paragraph to make it more readable. It is know:" CH₄ emissions of *Z. noltii* community averaged 0.31 mmol m⁻² d⁻¹ with ~76% being released during tidal immersion. They are about 4 fold higher than CH₄ fluxes

C8461

from the non-vegetated sediment community (0.07 mmol m⁻² d⁻¹ with ~93% being released during tidal immersion). Oremland (1975) reported CH₄ production rates ranging from 0.26 to 1.80 mmol D-1 from a *Thalassium testudinum* bed and production rates ranging from 0.08 to 0.19 mmol d-1 from a *Syringopodium* sp. Community. In a study of Deborde et al (2010) the methane production rates in the surface sediments of *Z. noltii* sites were generally below 0.040. mmol-2 d-1 (being the detection limit of their method. Somehow in contrast to our results they observed higher production rates in unvegetated sediments ranging from 0.01 to 0.8 mmol m²d-1. However, our CH₄ fluxes from the unvegetated sediment agree well with those reported by Deborde et al. (2010) for the Arcachon lagoon (0.04 to 0,78 mmol m²d-1). Bartlett et al. (1987) and Delaune et al. (1983) reported decreasing CH₄ fluxes with increasing salinity. CH₄ fluxes decreased from 17 to 34.2 mmol m-2 d-1 at salinities around 1 PSU to 0.17 to 0.85 mmol m-2 d-1 at salinities above 18 PSU. Though a direct comparison of these values with our data is difficult due to the differences in salinity our data fell well into the range given for higher salinities Middelburg et al. (2002) have estimated the average CH₄ flux from European estuarine waters to be 0.13 mmol m-2 d-1, which is about twice the fluxes of the non-vegetated sediments of the Ria Formosa lagoon. Hence our data suggest that apart from body circulation (Jansen et al. 2009; Grunwald et al. 2009) skin circulation may substantially contribute to CH₄ fluxes in tidal flats."

P10585 L18-20: I wonder how variable emissions from different species of sea grass are? Is it plausible to calculate global emissions from one sampling site as the emission rates may vary significantly?

This is certainly true, but an immanent problem of upscaling trace gas fluxes is that it is simply impossible to cover all species and different biogeochemical settings. To make this clear, we used the term tentative. In the case of methane we are confident with our statement that the methane emissions from seagrass meadows will most likely not change the current view that marine ecosystems are a minor source for methane to the atmosphere. Our methane fluxes are by about one order of magnitude larger than

C8462

those reported by Deborde et al. (2010) and fivefold smaller than the largest production rates given in Oremland (1975) Including these data suggests a range from <0.01 to 2.5 Tg yr⁻¹. This still small compared to range given for marine sources (11-18 Tgyr⁻¹). We changed the paragraph as follows: “A tentative upscaling using our flux data and a global seagrass coverage area of 300.000 km² (Duarte et al. 2005) reveals a global CH₄ flux of ~ 0.5 Tg CH₄ yr⁻¹ from seagrass meadows. Including the data from Oremland and from Deborde global emissions may range from < 0.1 Tg CH₄ yr⁻¹ to 2.5 Tg CH₄ yr⁻¹. The worlds ocean including the productive coastal ecosystems are a minor source for atmospheric CH₄ contributing about 10% to the global emissions (Wuebbles and Hayhoe, 2002). Emissions including productive coastal areas have been estimated to be in the range of 11 to 18 Tg yr⁻¹ (Bange et al. 1994). Despite the large uncertainty in this estimate it is reasonable to suppose seagrass meadows being a minor global source of CH₄. “

P10586 L6-10: Please rephrase, this sentence is very complicated and long.

We have done so; it is now:” To the best of our knowledge, we present here the first assessment of how the respiration of a seagrass community varies over night along with the tidal cycle. Several previous studies used punctual measurements either with dark chambers or during the night to assess the community respiration Santos et al, 2004, Silva et al., 2008, Duarte et al, 2010, Clavier et al, 2011). These punctual data were upscaled to estimate daily respiration rates and to calculate daily metabolic budgets of seagrass communities. Our data show that this practice may seriously affect the estimation of the metabolic daily budgets of seagrass communities, particularly in the intertidal.”

P10586 L25: Please explain the abbreviation “NCP”. NCP refers to net community production, explained now in the text.

P10587 L10: “different” instead of “difference” We have changed this.

P10587 L17-21: To be honest, I wonder why COS, CS₂ and propane are included if

C8463

the discussion is beyond the scope of the paper and only enhancements of fluxes can be provided? I feel like they are actually not needed, and it would be sufficient in my opinion to mention that they can be measured. If they can't be quantified and can't really be discussed, I don't see the point to include them to this extension. We have stated that a comprehensive discussion of their sources and sinks is beyond the scope of this paper. Nevertheless it is worth showing that the fluxes of these compounds are also affected by the tides.

P10588 L21: Please remove the “a” that is written before “very few”. We have done so.

P10588 L23: “In contrast to” instead of “contrasting to” We have changed this.

Table 2: I find it confusing to put the compounds that can be quantified and the ones that cannot be quantified into one table. I would rather divide this table, because for me, it does not make sense to report total amounts and enhancements in one column when units are only valid for half of the entries. We have removed the compounds that could not be quantified against a standard. See previous comment.

Figure 2: For easier understanding of Figure 1 would mark the in- and outlet. See previous comment. The flow direction in fig. 1 is marked by arrows. We will label the inlet and outlet in fig.1

Figure 4: I don't really see the point to only show enhancements of fluxes if you can actually show real variations with total numbers. This refers to the comment that it is difficult to understand the concept of “enhancement”. If this is clarified in the text, it could improve the Figure. I think I would prefer the total amounts where total amounts are possible. And I am not sure why the compounds that are not really discussed (like COS and CS₂) are actually included here.

Variations of the absolute fluxes are shown in table 2. See also previous comments.

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C8464