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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.

E. Bahlmann et al.

enno.bahlmann@zmaw.de

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Reviewer 1

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P10574 L1-3: The authors briefly list several ways in which chamber based measurements can lead to unrealistic fluxes, but provide no references and no mention of confounding mechanisms in the discussion. The authors do reference Gao et al. (1997) and Meixner et al. (1997) later in Methods, but these are very different designs than that presented here. These references list several obligatory tests that must be done before dynamic chamber results can be accepted on a scientific level. First, it must be

verified that advective flux is negligible relative to diffusive flux, i.e. that the calculated sediment flux does not vary with changing airflow. Second, a 'chamber blank' must be carried out to quantify non-sediment fluxes. The latter could be substantial given that water of unknown chemistry can enter and be equilibrated with the atmosphere inside the chamber.

The main criticism of the reviewer 1 is that the description of the chamber design and the discussion of potential biases are not sufficient. In agreement with this reviewer 3 suggests a more detailed description of the chamber system. We already provided a more extensive description of the chamber system (included now in the supplements) and will discuss potential caveats more careful in the discussion section. While deposition fluxes are very sensitive towards the aerodynamic properties of the chamber, it can be assumed that the emissions of most VOCs are not sensitive against the turbulent conditions inside the chamber. The reason is that the production of most VOCs is independent of the headspace concentration (Pape et al., 2008). Further, above waterlogged sediments the surface resistance is large relative to the boundary layer resistance. As outlined in Zhang et al. (2002) this again makes the trace gas fluxes insensitive against the aerodynamic properties of the chamber. In addition to our previous reply we would like to point out that Tengberg et al. (2004) compared three different types of stirred benthic chambers and found no significant differences between these chambers. Hence the authors concluded that benthic chambers are insensitive to the hydrodynamic conditions as long as the water is well mixed and the sediment is not re-suspended. As suggested by reviewer 1 we would further like to add a short section depicting the atmospheric mixing ratios of CO2 and CH4. These data support the flux dynamics observed with our chamber. We would like to point out that the main focus of this manuscript are i.) to show the strong imprint of the tides on trace gas fluxes in coastal ecosystems and ii.) to highlight the importance of accurately addressing the perturbations of turbulent flows in flux chamber studies. Both points may have severe implications for our understanding of trace gas dynamics in coastal ecosystems. Regarding the magnitude of the fluxes during submersion we agree with the reviewer and

have clearly stated this in the manuscript on page 10587, line 9-12: “As the strength of advection in our chamber system relative to ambient conditions is unknown we can currently not appraise the quality and reliability of the difference chamber systems “.

We do not agree with the referees’ general statement that it must be verified that advective transport is negligible relative to diffusive transport. This may hold true for aerial conditions when the flux is primarily controlled by diffusion but truly not for submersed conditions. A major outcome of our study is that advective transport processes substantially affect the trace gas exchange across sediment water and leaf water interfaces during submersion. Under submersed conditions the air flow through the chamber and the position of the frits will most likely affect the advective transport processes and thus making the trace gas fluxes sensitive to the flushing flow rate through the chamber. Regarding the magnitude of the fluxes we agree with the reviewer and have clearly stated this in the manuscript on page 10587, line 9-12: “As the strength of advection in our chamber system relative to ambient conditions is unknown we can currently not appraise the quality and reliability of the difference chamber systems. However these differences highlight the importance of accurately addressing the perturbations of turbulent flows in benthic flux chambers. We suppose that the reviewers statement concerning advective exchange refers to the work of Gao & Yates (1998) and hence to aerial conditions. Gao & Yates (1998) investigated the trace gas emissions above a constant soil source and pointed out that an artificial pressure deficit inside the chamber may cause an artificial advective flow. In their study they found that drawing air through the chamber is the main source for pressure deficits in the chamber headspace. In our set up the air was pumped through the chamber. The resulting pressure surplus, estimated from Poiseuilles law, is 0.8 Pa. Above impermeable waterlogged sediments this will not introduce any artificial advective flux.

Chamber blanks and losses of analytes to the chamber wall are more a problem of reactive trace gases such as NOx, O3 or ultra-trace gases but rather a problem for CO2 or methane. All materials used here have been shown to be appropriate for CO2

and methane measurements and we are currently not aware of any CO2-flux chamber study reporting blank problems for CO2 or methane.

The question for the exchange of water during the measurements is more serious and may become a problem under conditions of strong wave actions as we will show later. Thus metering the water exchange is clearly an improvement for future work. In our previous reply we have shown that the water exchange is not an issue for methane and the VOC’s but may affect the CO2 fluxes.

P10574 L8-9: The authors present no evidence for this statement. Yes, bubbling air through a small volume of water would increase turbulence, but that does not mean it is more representative of natural conditions than a static chamber, particularly when non-linear relationships are involved. Our intention was rather to point out the main differences between our chamber system and those used in previous gas exchange studies than claiming, that our system is more representative of natural conditions than static chambers. We have clearly stated this on page 10587, line 9-12, when it comes to the discussion of the magnitude of the fluxes“As the strength of advection in our chamber system relative to ambient conditions is unknown we can currently not appraise the quality and reliability of the difference chamber systems. However these differences highlight the importance of accurately addressing the perturbations of turbulent flows in benthic flux chambers.” Nevertheless there is overwhelming evidence from the literature that sufficient mixing of the water inside the chamber is a prerequisite for assessing exchange processes under submerged conditions. See for instance Tengborg et al. (2004) and references therein.

P10575 L18: Because the frits are 12 cm above the sediment surface, I expect that there would be a notable change in water turbulence and air-water-sediment transfer velocity when the water rises above or drops below 12 cm. Can the authors comment on this? We also expected a notable change in the turbulence and corresponding change of the transfer velocity across the sediment water interface, when the water rises or drops above / below 12cm but this does not show up in our flux data. It took

between 7 15 minutes to change the water level inside the chamber by 12 cm, being on the same time scale as our sampling frequency. Thus we assume that the sampling frequency was not sufficient to monitor such an effect. In particular during rising tide this coincides with drastic changes in the CO2 and methane concentrations inside the chamber superimposing changes in the transfer velocity.

P10576 L26: The sampling regimen needs some clarification. Line 26 notes that each of three lines were sampled consecutively for 5 min, but P10578 L8 describes results on 5-10 min intervals. This gives the impression that chamber samples were collected every five minutes, with less frequent atmospheric sampling. Also, was the inlet air to the chamber pumped from the 2 m line, the 4 m line or some other intake?

The sampling lines were indeed sampled consecutively resulting in a time resolution of 15 minutes. The time interval given on page 10578 L8 is an estimate that accounts for the response time of the chamber. We will change this to < 15 min. The inlet air was taken from the 4m line.

P10577 Results: A time-series figure showing Cin and Cout would help explain some of the authors' results. Was Cin relatively constant, with most of the fluxes driven by variability in Cout? Could flux estimates have been affected by rapid changes in Cin and subsequent equilibration with the chamber water? Estimated or observed water depth would also be worth plotting here. As shown in our previous response, the flux variability cannot be explained by variations of the inlet concentration. In particular during low wind speed the inlet concentration shows the same variability as observed in the chamber, and thus strongly supports the temporal flux pattern observed with the chamber. A section showing the atmospheric mixing ratios of CO2 and CH4 is included in results section under point 3.4 in the revised manuscript. We prefer not adding any estimated water depth in figure 2. As this might imply a correlation between the fluxes and water depth that is not justified by measurements.See also the response to reviewer 3.

P10583 L 5-8: The change described by Werner et al. (2006) was in the horizontal flow velocity and only in the top 2 cm. I expect that the horizontal flow velocity characteristics inside a chamber that is pressed 5 cm into the sediment could be very different. This is true. Our hypothesis is based on the observation that this drop regularly occurs before the water entered or after the water left the chamber. Thus this is rather an analogy. We agree with the referee that the observed drop cannot unambiguously be attributed to changes in the horizontal flow and that the flow characteristics inside the chamber could be different to those outside. We formulated our hypothesis more carefully and changed the sentence: "Although the chamber will certainly affect the water flow in the top sediment, this may provide a clue to explain the observed drop in the emission fluxes.".

P10583 L 18-29: Without an analysis of chamber aerodynamics, it cannot be determined whether the static air in and near the sediment was an artifact of the chamber design. Also, did the spike occur before any flood water had entered the chamber? In our previous response we have shown that mixing of the entire chamber volume is achieved within 0.4 min preventing the buildup stagnant air layers above the sediment. Hence we can rule out a chamber artifact. The spike occurred with the flood water entering the chamber not before.

P10585 L 13-16: Can the authors suggest any reason for the disparity between their results and those of Deborde et al. (2010)? Trace gas fluxes above natural surfaces are generally highly variable in space and time and thus may account for the disparity between our results and those of Deborde et al. (2010).The methane flux above a surface depends on the Archean community producing methane, the methanotrophic activity in the sediment and the forcing of the exchange across the sediment water interface. The difference in the methane peak between night and day in our study highlight the importance of methanotrophic acitivity at the sediment surface on the methane efflux. Finally, in the Deborde study a static chamber was used that only measured the diffusive flux as outlined there, whereas our system measures the diffusive and advective

flux. This partly accounts for the differences between both studies.

P10585 L25: Refer to the comments of other G. Abril regarding CO2-based metabolism estimates. We have done so.

P10587 L1: There are several other ways that bubbling atmospheric air through the chamber may affect fluxes. The gas concentration and temperature in the chamber water will be at near equilibrium with the air. This could introduce artificial gradients and thermal perturbations, as mentioned only briefly in the introduction. Because this is reportedly one of the first studies to measure fluxes using an aerated chamber, the authors must provide a thorough discussion of the new caveats associated with this method.

We already responded to this point in our previous reply to the reviewer. As mentioned above Tengberg et al. (2004) found benthic chambers being insensitive to the hydrodynamic conditions as long as the water is well mixed and the sediment is not re-suspended. Our system fulfills both conditions. Under submersed conditions the temperature inside the chamber will depend on the heat conductivity of the chamber walls. With a heat capacity of 1.005 kJ kg-1K-1 for air and a mass flow of 0.28Kg h-1air the heat flow is 0.21kJ ΔT, where ΔT denotes the temperature difference between the air and the water. With a chamber volume of 8 L the resulting temperature change of the enclosed water can be approximated to 0.006 ΔT. The heat conductivity of the chamber walls (0.005m thickness, 0.24m^2 surface area) is about 0.32 kJ ΔT. Thus we can safely assume the chamber being in thermal equilibrium with the surrounding water.

P10588 L25-27: I'm not sure this statement can be supported without some sort of comparative control, i.e. simultaneous measurement of fluxes using a static chamber or periodic measurement of dissolved gas concentrations outside the chamber.

The effect of mixing on exchange processes across sediment-water and plant water interfaces under submersed conditions has been shown in a variety of studies (Werner

et al., 2006; Billerbeck et al., 2006; Huettel et al., 1996; Precht et al., 2004; Werner et al., 2006; Kim and Kim, 2007; Cook et al., 2007; Jansen et al., 2009; Clavier et al., 2009). We clarified this statement. It is now:" The main difference to most of the previous studies is the introduction of an turbulent flow in our flux chamber system resulting in substantially higher fluxes during immersion as previously reported for oxygen, DIC nutrients and suspended matter."

Technical corrections:

P10573 L12: References out of order. We have changed this.

P10573 L22: Gao and Yates (1998) is not in reference section. We suppose this refers to the citation on page 10574. We now have included the reference in the reference section.

P10573 L22-23: References out of order. We have changed this.

P10574 L25: Change drawn to pumped. We have changed this.

P10583 L10-11: Sentence is hard to read, it would help to reword or split into two sentences. We have split the sentence. It is know: "Thereafter, CH4 fluxes dropped to increase again with tidal height. In contrast the respiratory CO2 night flux showed a gradual decline."

P10587 L10: Change difference to different. We have changed this.

P10587 L10: Change 'can currently not' to 'cannot currently'. We have changed this.

Figure 2: It would be easier to see the CH4 fluxes if the plot were scaled such that the peaks were removed, as was done for the CO2 fluxes, with the values beyond the plot scale listed directly on the plot. There appears to be a few gaps in the CO2 flux data during the change from tidal immersion to air exposure. Can this be explained? It should be noted whether the temperature shown in figure 2 was measured inside or outside the chamber. If both were measured, it would be a worthwhile comparison.

We rescaled the methane plot as suggested by the reviewer. The data gaps are due to maintenance work to the chamber. We forgot to remove them for CH4 and corrected this omission. We will further indicate this in the figure caption. The temperature shown in figure 2 is the air temperature 3m above the ground. As suggested by reviewer 3 we will skip the temperature data.

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