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Interactive comment on "Efficiency and adaptability of the benthic methane filter at Quepos Slide cold seeps, offshore Costa Rica" *by* P. Steeb et al.

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We could like to thank all four reviewers for their critical comments, which we think tremendously helped to improve the quality and clarity of this manuscript. We hope our responses and adaptations are adequate to accept this manuscript for publication in Biogeosciences. Please find our detailed responses below.

Anonymous Referee #2 Received and published: 16 March 2015 I found this manuscript a very interesting approach to investigate the adaptation of the anaerobic benthic methane filter to changing fluid flow. The authors use the common methods to derive key parameters of methane consumption in surface sediments. Then they

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use sub-cores for an experiment in the laboratory. They found that the zone of AOM decreases with increasing fluid flow and that most of the methane is consumed when methane flux is below 3 mmol m-2 d-1. Only rapid changes cause an increase in methane efflux. Although I like the approach, I found that more details in the method part and a clearer discussion would greatly improve the manuscript.

Major concerns: 1. The variability in the porewater chemistry appears not coherent with a steady state situation. Fig. 3 and 4 illustrate that not only sulfide as described, but also the other analyzed parameters vary over time, only the establishment of a SMTZ in the high flow experiment appears stable.

Author Reply: During most of the incubation time, the cores were not in steady-state. The system started with zero fluid flow to which then a low or high fluid flow was applied. It was the purpose of this study to follow the response of biogeochemical parameters to these applied flows. Towards the end of the incubation, a quasi steady state situation was reached in the high flow core, as we did not observe any more pronounced changes in the profiles. Steady state was never reached in the low flow core (see also additions to 4.1).

It would be helpful to know the precision of the measurements and how much water was taken from the experiment for the different analyses. Could you also indicate what changes in the concentrations can be expected if xx ml porewater are withdrawn from the experiment. After gaining an understanding of the typical errors due to porewater withdrawal and analytic procedures, one would know what are the 'real' changes over time and if a near steady state situation was reached.

Author Reply: With the normal rhizon sampling procedure, 8.1 % of porewater is removed from each sampling layer. This porewater is replaced by porewater from adjacent layers and ultimately by the seawater medium in the supernatant. This 8.1% replacement/dilution plus an analytical precision of <1% (ion chromatography) and 0.1% (TA titration) adds up to a total analytical error of ca. 9% and 8.2% for sulfate/bromide

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and TA, respectively. We added this information to 2.6. Sulfide, pH, and redox potential were determined prior to porewater sampling (with microsensors) and are therefore likely not affected by the rhizon sampling.

The low methane flux in the experimental setup (page 16055, line27 et seq.) raises the question, why was not more methane pumped into the system. Was there a reason for choosing a 1mM concentration for the methane-rich solution? Could you please calculate the methane solubility in situ in contrast to your experiment and use the value to explain the low methane flux. What maximum methane solubility would have been possible to achieve in your experimental setup?

Author Reply: The SLOT system operated at atmospheric pressure (1.07 bar). The solubility of methane at the experimental pressure, temperature (10° C) and salinity (35 PSU) is around 1.5 mmol l-1, calculated after Yamamoto et al. 1976. Unfortunately we were not able to achieve this theoretical concentration, for reasons unknown (see extensive discussion in the original method publication by Steeb et al. 2014). The highest and relatively stable methane concentration we archived was around 1 mmol l-1 (965±180 μmol l-1). We highlighted the system's limitations in the method part (2.5) and referred to Steeb et al. 2014 for more details on the advantages and disadvantages of the method. We hope this information is sufficient, as we would like to avoid to repeat this discussion.

2. The different efficiencies of AOM (page 16056, line 19 et seq.) could also be due to different transport processes in the experimental setup in contrast to the natural environment. In the experiment the solutes are transported by diffusion, but in the natural environment fractures of different sizes might play a more dominant role (Mau et al., 2006). This thought is missing in the discussion and could be included.

Author Reply: In the experiments, the solutes were not transported by diffusion but by advection (fluid flow). But the reviewer is absolutely right that fractures in the core can enable faster transport. We observed such a possible transport mechanism in the low

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flow core (bromide and methane in the outflow) and discussed it (see 4.1)

3. From the conducted experiment one can state that methane ascending at a rate of up to _3 mmol m-2 d-1 is completely consumed by AOM. Is this coherent with published data? It would be nice if a table with methane fluxes and AOM-rates that are mentioned throughout the manuscript would provide a quick overview of what values were measured, modeled and how the experimental derived data fits to those.

Author Reply: We appreciate this suggestion and added an overview of fluid flow rates, benthic methane emissions, methane fluxes, and ex situ rates of AOM in comparison with previous studies at cold seep sites covered with sulfur bacteria mats. See new Table 5. We hope this overview puts our data into a better perspective. The areal rates of methane oxidation at seep sides vary over a broad range. At hot spots like Hydrate Ridge (Treude et al. 2003) or Mound 11 (Krause et al. 2014) AOM rates of more than 100 mmol m-2 d-1 were observed.

4. It should be clearly indicated that only one value for the response time was derived from the experiment and that further studies need to validate this value. Also, on page 16058, line 6, you write 171 days whereas on page 16060, line 8 you provide a range 150-170 days. Please clarify this contradiction.

Author Reply: After 171 days no further methane decrease was detected in the HFC, therefore no further change in the efficiency of AOM was expected. However, final efficiency might have established at an earlier time point (between the 105 and 171d measurement). From the fit of methane concentrations in the outflow, we estimated a response time between 150 and 171 days. But the reviewer is right, we do not actually know what happens between measurements and therefore changed the response time in the conclusion to ca. 170 d. We also highlighted that more measurements are required to validate our results.

5. The method section needs more details. At what temperature were the cores for the experiments transported from the cruise to the lab?

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Author Reply: The sediment cores were transported at 4°C and stored at 0°C. Information added to 2.5

At what temperature was the experiment performed?

Author Reply: The experiments were performed at 10°C. Information added to 2.5

These temperatures should be included in the manuscript as the temperature influences the solubility of methane.

Why were both media, also the resembling seawater media kept anoxic? There is still sufficient O2 in the water although located in the OMZ (page 16041, line 23). Is it because you liked to focus on the anaerobic methane consumption, then it should be clearly stated.

Author Reply: Yes, we focused only on AOM, as it is the most important process for removing methane from the sediment. We added this information to 2.5. But more importantly, oxic conditions in the overlying water would have compromised our methane budgeting, as we would not have been able to exclude aerobic methane oxidation in the supernatant, which could be powerful when kept in containment without dilution through currents. Furthermore, adjusting the seawater medium in the system to a constant (in situ) concentration of 6 μmol l-1 for over one year would have been a completely new challenge.

6. Tables and figures: The order of the tables in the text is not consecutive (Table 1, Table 4, and then Table 2). Table 3 is not mentioned in the text.

Author Reply: Thank you for noticing. We corrected/added all Table citations.

Fig. 5 is missing in the text, but should be included on page 16051, line 14 and 26.
Author Reply: Thank you for noticing. We added the citation to 3.2.2

Figure references in text include supplements, e.g., Fig. 3a-u, but these supplements are not shown in the figures.

Author Reply: Unfortunately, we are not sure what the reviewer meant. Fig. 3 has supplements from a-u.

It would be better to use the same scale for sulfide in all plots of Fig. 3 and 4 (possibly log-plots are better?) otherwise mention it in the figure caption that sulfide concentrations are plotted on different scales.

Author Reply: We agree that one scale for one chemical species is usually better. Unfortunately with the same scale it would not be possible to see sulfide values at the lower concentration range. A log scale is also not possible as many concentrations are zero. We would therefore like to keep the scale as is.

The supplements of Fig. 5, e.g., Fig. 5 a and b, do not match with the figure itself. Methane concentration in the outflow is shown in A and D, but not in A and B.

Author Reply: Thank you for noticing. We changed the supplement ID accordingly.

7. Minor changes: Some references are not in brackets, page 16037, line 8, page 16038, line 24, page 16040, line 22.

Author Reply: The brackets were actually in the submitted manuscript. They must have been lost when the discussion paper was produced.

Delete 'huge' on page 16037, line 12.

Author Reply: Done

Add year of sampling in method section

Author Reply: Done

Change 'controls samples' to 'control samples' page 16040, line 13

Author Reply: Done

Change 'several month' to 'several months' page 16040, line 24

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Author Reply: Done

The sentence: ‘Further details on the SLOT sampling procedure..’ page 16045, line 14, can be deleted as it was mentioned before.

Author Reply: This citation refers only to the sampling procedure and should remain here.

I found it confusing to talk about a moderate flow rate but call the experiment low flow, page 16042, line 3-5.

Author Reply: Its true that it sounds strange within the broader perspective, but compared to the high flow setup it was a low flow.

I suggest to delete ‘than expected’ on page 16056, line 16, as it otherwise sounds as if you did not know how much methane was pumped into your experimental system.

Author Reply: We deleted "expected"

Rephrase sentences on page 16057, line16 to ‘We assume that at most 80% of the sulfate reduction . . .can be related to.. Most likely, this ratio is less, because ex situ radiotracer incubations. . .’. In addition, I suggest to rewrite the last sentence of the paragraph page 16057, line 20 to clarify that the organic matter degradation is higher near the seafloor and decreasing with sediment depth.

Author Reply: Done

Interactive comment on Biogeosciences Discuss., 11, 16033, 2014.