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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.

Anonymous Referee #2

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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 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:

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. 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 Could you also indicate what changes in the concentrations can be expected if xx ml porewater are withdrawn from the experiment.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.

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?

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.

From the conducted experiment one can state that methane ascending at a rate of up to $\sim$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, modelled and how the experimental derived data fits to those.

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.

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

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.

Fig. 5 is missing in the text, but should be included on page 16051, line 14 and 26.

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

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.

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.

Minor changes:

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

Delete ‘huge’ on page 16037, line 12.

Add year of sampling in method section

Change ‘controls samples’ to ‘control samples’ page 16040, line 13

Change ‘several month’ to ‘several months’ page 16040, line 24

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

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

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.

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.

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