Biogeosciences Discuss., 11, C9434–C9438, 2015
www.biogeosciences-discuss.net/11/C9434/2015/

Biogeosciences
Discussions

BGD
11, C9434–C9438, 2015

Interactive Comment

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

P. Steeb et al.

ttreude@g.ucla.edu

Received and published: 15 August 2015

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 #1 Received and published: 17 February 2015 The manuscript by Steeb et al. consists of two parts. One explores the current in situ methane geochemistry at two sites in the Quepos Slide (offshore Costa Rica) whilst the other uses sediments from these sites to simulate the effects of changing fluid flow conditions on the sedimentary biogeochemistry using a flow through reactor. The first part concludes that the benthic filter at these sites is highly efficient, with AOM serving as an effective barrier for methane seepage into the water column. Seepage velocities are also extracted from the numerical model. The second part concludes that, under the conditions of the flow-through experiment, the benthic filter can cope with a wide range of fluid flows (0.5-5 microL/min delivering 0.28-2.8 mmol m-2 d-1 methane, respectively) for up to 316 days, with a change in the flow regime at 260 days. The paper is well written and contains interesting insights. I think, however, that the manuscript could benefit from additional discussions and more emphasis on the assumptions behind both the numerical model and the experimental setup. The following summarizes some criticisms of specific sections:

Introduction: The introduction centered mainly on seeps and AOM, but only tangentially discusses the scientific question or the aims that the manuscript wants to address/ achieve. Background information is good and important, but it is not until the last sentence in the introduction that the authors tacitly frame their research question. Further information that should go either in the introduction or in the Methods section should be the reason for the given approaches and how these approaches complement each other.

Author Reply: We would like to thank the reviewer for this helpful comment. We restructured and edited the last section of the introduction to provide a better overview of our goals and applied methods.

Numerical Model: The modelling exercise was performed in order to determine the site-specific areal AOM rates and fluid velocities. In general the model parameters are highly unconstrained, for example, what determines the lower boundary of the model? (i.e. What evidence exists for hydrates at 50-80 cmbsf?).

Author Reply: We found no evidences for gas hydrates, such as visible hydrates or porewater dilution, in the multicorer cores (44 cm length), which was not surprising as the study site was above the gas hydrate stability zone. We used the equations by Tishenko et al. 2005 to calculate the sediment methane concentration at the lower boundary.

Table 4 shows over 13 parameters are fitted, what procedure was used to determine a best fit? Some of the fitted values seem exceptional and would thus require additional justification (i.e. 80 yr-1 non local mixing). Is the entire core length the mixing depth?)

Author Reply: The reviewer asserts that the model is highly unconstrained. The model (like all models) simplifies the biogeochemistry and physics of natural settings. However, at seeps, the major biogeochemical processes occurring in surface sediments are limited to sulfate reduction, AOM, and precipitation of sulfide. The measured geochemical profiles contain all the information (diffusive gradients etc.) needed to constrain these rates and parameterizations quite well, as well as the upward fluid flow velocities. We did not use an optimization procedure to parameterize the model since the modeled profiles are quite sensitive to the parameters given in Table 4 (now Table 2). In other words, the entire set of data allows the parameters to be well constrained. Yet, the burial velocity is more unconstrained than other parameters. This is not of major concern at our site because the solute transport is dominated by fluid advection and non-local transport. The lower boundary conditions of the model (fixed concentrations) are simply determined from the measured data. (The non-local transport of 80 yr-1 is a maximum value at the sediment surface, which attenuates toward zero at 2 cm depth).

What evidence exists for steady state conditions?

Author Reply: This is a necessary assumption to derive the background rates of AOM. We acknowledge the possibility that the geochemical profiles are not in steady state. We have no temporal data to investigate this further.

In the rate-fitting simulations, only AOM was taken into account while SRR was ignored. Justification for these assumptions and further clarifications are required in order to correctly interpret the results of the numerical model.

BGD

11, C9434–C9438, 2015

Interactive Comment

Author Reply: As our model did not consider organoclastic sulfate reduction, sulfate reduction rates equaled AOM rates. We therefore refrained from showing the modeled sulfate reduction, as now new information would be gained.

SLOT experiment: Maybe I missed it, but the dimensions of the SLOT cores should be given. I can infer them from the porosity data and the pore water residence time, but this does not allow for an independent assessment of the residence time. Consequently, it is also difficult to tell how much pore water was removed during the extractions with respect to the total volume of pore water. This is important to determine how the pore water concentrations may shift during the rhizon extractions. It would also help to establish to what extent the SBTZ movement is due to AOM vs. fluid displacement.

Author Reply: Diameter of the SLOT liners (6 cm) was added (see 2.5). The exact technical drawings of the liners can be found in Steeb et al 2014. 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 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.

A great deal of the discussion focuses on comparing the flux and AOM results of the SLOT experiment with those at other nearby sites. As the methane flux cannot be replicated due to pressure constraints, perhaps the authors could collect methane flux (both from the source and out of the sediment), AOM, fluid flow, and other environmental information from various seeps into a table to facilitate the comparison, especially as these results are at odds with the modelling and field observation of Karaca et al. (2012) and Bohrmann et al. (2002).

Author Reply: We thank the reviewer for this suggestion. We 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.

It appears to me, based on the information provided in the manuscript, that the limitations of the experiment would make it impossible to extrapolate the results to field conditions, especially since at seeps sites methane is often found to bypass the anaerobic zone and supply energy for many aerobic communities (Boetius and Wenzhöfer, Nature Geoscience, 6, 725–734, 2013). I thus feel that better context of the experiment, such as the different methane to sulfate ratios possible in the SLOT experiment in comparison to field sites, warrants further scrutiny.

Author Reply: We are well aware of the limitations of the SLOT system, which have been broadly discussed in the original publication of this method (Steeb et al. 2014). The main reason for applying this method is to study the response of AOM and the SMTZ to different fluid flow regimes. But the reviewer is right that we should emphasis this point stronger in the present publication. We therefore highlighted the methodological limitations of the SLOT system in the method part (2.5).

Minor revisions: Page 16037 line 9 Parenthesis missing.

Author Reply: Sorry, we did not find a parenthesis missing in this line.

Page 16039 line 15 remove “by”.

Author Reply: Done

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