

Interactive comment on “Short-term changes in anaerobic oxidation of methane in response to varying methane and sulfate fluxes” by G. Wegener and A. Boetius

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page 3071 line 7. aerobic methane oxidation is certainly better understood, but I don't know that it's well understood. There is increasing evidence that aerobic methanotrophs are better described as hypoxic methanotrophs-

AC: This sentence was revised/ deleted, line 45 to 50

page 3071, line 15-18. You should include methane oxidation rates from other seeps as well, to indicate to the reader that there is variation in AOM and SR rates among seeps. For example, AOM rates from the GoM or Monterey Bay would be appropriate (Joye lab work, or the Girguis papers you've already cited).

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AC: examples of GoM and Monterey Bay were included- $\approx 0.03 \mu\text{mol g day}^{-1}$ in coastal sediments of Monterey Bay. In advective deep-sea systems maximum methane oxidation rates of $0.2 \mu\text{mol cm}^{-3} \text{ day}^{-1}$ (Gulf of Mexico, low seepage rates (Joye, et al., 2004) but also rates up to $3 \mu\text{mol cm}^{-3} \text{ day}^{-1}$ (cold seeps of Hydrate Ridge (Treude et al., 2003) were determined. In methanotrophic microbial mats of the Black Sea a methane consumption of $10 \mu\text{mol cm}^{-3} \text{ day}^{-1}$ was determined (Treude et al., 2007). (line 74 -81)

Page 3076, line 22. You implicate other electron acceptors as the explanation for the differences in stoichiometry. I would seriously look into having your gasses analyzed. It's not expensive or time consuming. Even if it's a different bottle (since I presume you've expended the bottles used in this project), it will give you some quantitative sense of how much contamination is in your gas. Also, don't forget that your sediments may contain organics that can fuel sulfate reduction independent of AOM.

AC: The H₂ and higher hydrocarbon content of the commercial gas used as methane source was $<500\text{ppmv}$ and $<3000\text{ppmv}$, respectively. Taking solubilities into account, both potential electron donors for SR together could explain $0.012 \mu\text{mol (g dw)}^{-1}$ or ca 8-10 % of the observed sulfide production in the experiment. The background sulfide production in these sediments was around 6-8% of the total sulfide production, hence both background sulfide production terms together could explain ca. 15-20% of the total sulfide production. We also calculated the mass balance between sulfide produced and methane consumed based on measurement of the reservoir medium at the beginning and end of the experiment 1 (constant conditions), and found the same pattern, of 10% higher SR compared to AOM. Hence, we conclude that the background SR from sediments and impurities in the gas explain the observed offset, but that the main variation observed and discussed in the MS is due to the methane driven sulfide production. We have revised the manuscript accordingly.

Page 3077, line 25-28. It's OK to show just one dataset (since showing all of

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them may lead the reader to question natural variation in treatments). However, you need to use non-parametric statistics to insure the reader that your treatments are in fact comparable.

AC: A new table (Table 3) was added summarizing the results of all experiments

Page 3080. It's good to calculate $K(m)$, but please put some effort into outlining the potential limitations of this approach using the data you have in hand

AC: Whole paragraph was changed to outline limitations and to better explain the offset between SR and AOM:

In the absence of methane, a background sulfide production of about $0.02 \pm 0.01 \mu\text{mol gdw}^{-1} \text{d}^{-1}$ was determined. Sulfide production increased to $0.17 \mu\text{mol gdw}^{-1} \text{d}^{-1}$ at 1.35 mM CH_4 and to $0.21 \mu\text{mol gdw}^{-1} \text{day}^{-1}$ at 2.3 mM CH_4 . Methane oxidation followed the trend of sulfide production with rates of 0.02, 0.10 and $0.17 \mu\text{mol gdw}^{-1} \text{day}^{-1}$ at 0.33 mM, 1.35 mM and 2.3 mM CH_4 , respectively (Table 3). The high excess of sulfide production (up to the double of methane consumption) in this experiment is most likely due to contamination with alternative organic electron acceptors for sulfate reduction (acetate etc.) after medium exchange. However, this would not affect the methane budgets, which show linear relation between methane concentration and methane consumption in the whole tested methane range. This indicates that substrate saturation of the metabolic activity was not reached. Hence we conclude that the apparent K_M -value for methane in AOM is above the tested range, i.e. $>2 \text{ mM}$. This is in agreement with Nauhaus et al. (2002), who observed a linear relationship between methane oxidation and sulfide production at methane pressures below 0.1 MPa (about 1.5 mM), and suggested a high methane K_M in the range of several mM for AOM and methane-fueled SR. (line 343 to 360)

Additionally we added an outlook outlining the possibilities high-pressure incubations:

Further experiments with ANME-enrichments in high-pressure flow-through

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systems are needed for the determination of half saturation constants of sulfate and methane in AOM. (line 391 & 393)

Figure 2. Your Y-axis is labelled AOM/SR. Is this correct? That symbol implies division, which makes me think that your data are ratios (though your figure legend clarifies this point). Just say "AOM and SR rates".

AC: Changed as recommended

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