

Interactive comment on “Different methanotrophic potentials in stratified polar fjord waters (Storfjorden, Spitsbergen) identified by using a combination of methane oxidation techniques” by S. Mau et al.

Anonymous Referee #1

Received and published: 20 May 2013

This manuscript gives the results of a study evaluating the distribution of CH₄ oxidizing activity in a polar fjord using radioactive tracers, stable isotopes and molecular approaches. The authors correctly point out that measurements of CH₄ oxidation in ocean waters are comparatively rare, especially in high latitude environments. The study is also noteworthy from the perspective that multiple approaches are used to evaluate CH₄ oxidation.

In my view the title of the manuscript is a bit confusing and perhaps should be changed. I interpret CH₄ oxidation potential to indicate the maximum rate of CH₄ oxidation, that

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is the zero order (substrate saturated) rate in the Michaelis sense. As in most studies of CH₄ oxidation in low CH₄ systems, a true “tracer” experiment cannot be performed, where the added isotope does not significantly affect the substrate pool (exception: Pack et al. cited herein). The additions here significantly increased the concentration of available substrate forcing back-calculation of the rate at the ambient substrate concentration from first order rate constants. However, radioactive CH₄ additions were not sufficient to elicit a zero order response; half-saturation constants for CH₄ oxidation are generally several μM , compared with the $<500\text{nM}$ levels observed here, even after. Thus, the measured rates are not potential rates, nor are they an estimate of rates at the in situ CH₄ concentration, until adjusted via the first order rate constant.

The authors give great detail in the Methods about recovering respired CH₄ and CH₄ incorporated into biomass or released as dissolved organic matter. However, it is not made clear whether the reported rates are for total CH₄ consumed or simply the fraction recovered as CO₂, i.e. respired. On a similar note, since the assimilated CH₄ was fractionated, it may be useful and insightful to report the fraction incorporated into microbial biomass.

The manuscript would benefit by placing the results in the context of other marine and even freshwater studies. The authors give a wonderful compendium of measured CH₄ oxidation rates in Table 1 (referenced only in the Introduction). This could be referenced again in a rate comparison in the Discussion.

Interactive comment on Biogeosciences Discuss., 10, 6461, 2013.

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