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## ***Interactive comment on “Coexisting methane and oxygen excesses in nitrate-limited polar water (Fram Strait) during ongoing sea ice melting” by E. Damm et al.***

### **Anonymous Referee #1**

Received and published: 13 July 2011

The manuscript by Damm et al. presents data of hydrographic, nutrient, oxygen, DMSP, and methane along section at 79N across Fram strait, including two open water regions surrounding an area with some ice coverage, that coincidences with the boundary between the northward flowing AW in the east and less saline PWS in the west. The two water masses are biogeochemically characterized by different nutrient limitations, with Redfield-like P/N limitation in the east and N-limitation in the west.

The paper is well organized, crisp, and a good read. That said, it basically narrating two stories, which are only loosely linked. The dependence on methane production from the primary producing community, which again is linked to the N/P ratio, or the

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shift from new to regenerated production. This finding is based on experimental data A theoretical consideration of the limits for the establishment of anaerobic conditions in a oxygen-consuming cells, where oxygen transport is limited by O<sub>2</sub> permeability through the cell membrane, which is later “tuned” for *Roseobacter*, as they were shown to be involved in DMSP-methane transformation in earlier work by the authors. The link is the observation of the methane maximum at relatively high oxygen levels.

I suggest some minor changes / improvements and have to ask some questions of understanding. The paper can be ACCEPTED AFTER MINOR REVISIONS. Listed all below according to the paper, the more important questions/recommendations to be addressed are Figure 1 158 173 199-200 282 And the suggestion of adding a new Figure 3ab \_\_\_\_\_

Title: the Title of the paper is only pointing at the first of the two stories. The last is something like “ model constrains on the formation of anoxic environments within bacteria dwelling in oxygenated waters” but the authors surely could do a better job then me and might want to consider to point to this aspect of the paper by changing the title.

Figure 1 needs more information in the Figure as well as the caption. Explain ice color scheme; why not introducing the schematic northward flow, label the Polynya, give names to the surrounding land masses; this would help the reader not specialized in Arctic research, and the figure should be understandable without consultation of Spreen et al. 2008.

Chapter 3 : Rename “Sampling procedure and applied methods” or so

Line 95 specify used oxygen sensor; has the sensor be Winckler verified during the cruise?

Line 98 “towed in trough” ... Do not understand the expression. Might be clear for every marine biologist, though.

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Line 129 ...preserved in ice covered as well as in ice-free ... ?????

144 In the ice-covered regions light transmission was only reduced in the upper 20 m  
????

145 crated different - WHAT-WORDS MISSING – under ice ...

154 CUT “ increasing” : correlated with oxygen concentrations

158 following: cannot see the stringence of the argument. If you start with N-limitation and continue with Redfield-conform PP, this will further drive the remaining nutrients away from the Redfield ratio. Please strengthen – or revise your statement concerning the need for additional nitrogen sources

Lines 173 following: the paragraph is very vague. DMSP concentrations close to those in the ice free AW bloom are explained with ice algae, and the fact that PSW waters are DMSP poor even close to the ice edge is explained is eaten up by bact. consumption. Neither the one nor the other effect in vicinity of ice is really constrained by the data, and also not really important for the stories of the paper. Consider to remove, or make stronger.

184 ... concentration in equilibrium with the atmospheric partial pressure (between 3 and 3.5 nM, depending on T and S).

204 inverse correlation: well, if this is clearly present in the data, pls. show as property property plot. This cannot be seen from the sections presented in section II.

Lines 199 to 200: here, the authors directly translate the enhanced methane concentration into a hotspot of methane production. This needs justification. The high concentrations occur under the partly ice-covered region, i.e. in the area of reduced air-sea exchange. On top of that, it is the only area with strongest upper water column stratification (see Fig 2A). Both tend to diminish escape of the methane produced. I do not argue that the authors are right, but this has to be considered in the argumentation.

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Line 237 in connection to line 263: is this not the diffusion coefficient of water in the cell which has to be considered here? Which is not per se the same than in the surrounding water (off course of minor importance for the argumentation)

Line 228 AND 257 I strongly recommend to add a Figure 3ab showing in 3a a conceptual scetch of the setting, illustrating the concept, boundaries etc., and in 3b a graph  $C(r)$  vs  $r$  illustrating the solution of equation (6). This would make it easier for the non-modeller.

278 Though I see no better way the using Spalding and Portis either, I suggest to add a sentence on the limitations of this approach. Membranes can be highly specific in their transport characteristics, so equation 8 cannot be taken for granted at all.

282 I understand that it is tempting, but given the uncertainties in the – nice – approach, it is mere coincidence that you end up with the  $C_{max}$  matching the observed data. Also, I would like a sentence on the surrounding surrounding the bacteria (i.e. Roseobacter). Wouldn't the microbes be mostly bound to particles rather than free floating, which again mains that the oxygen concentration  $c_0=c_a$  they would “see” might be lower than the oxygen concentration in the bulk seawater (i.e. Jorgensen 1977).

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