

Interactive
Comment

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 #2

Received and published: 27 July 2011

Damm and colleagues' work show oceanographic data (including nutrients and oxygen) along with climatically important tracers like methane and DMSP along a zonal transect at 79°N, in the Fram Strait in the Arctic Sea.

The oceanographic settings of the study area are very complex and probably unfamiliar to many oceanographers and biogeochemists, so it is difficult to figure out the oceanographic processes that occur when they are not well explained and the hydrography not included in detail.

Also, the study area appears to be very dynamic at an inter-annual scale, with a clear long term trend driven by global warming. This is of tremendous importance in the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



sense that that it is undergoing rapid changes. The concentration of gases may be very sensitive to these changes; if gases are being produced and exchanged with the atmosphere in this region, they act as a positive feedback mechanism for global warming.

As MS' title mentioned, results report a hot spot of CH₄ in nitrate limited and oxygen excess water during on-going sea ice melting. First of all, nitrate limitation is a concept that comes from the biological community. Neither physiological studies, nor biogeochemical community indices (such as N:P or P*, Chl-a) are included and analysed with sufficient depth. Let include some biological data

Secondly, oxygen excess is not an appropriate term. Usually surface water are super-saturated in O₂; here, no saturation percentage was estimated and due to the different salinity and temperature of the present water mass (i.e., PDW and ADW), I deduce that saturation percentage may be similar along the zonal transect. Author should indicate saturation percentage values.

The MS did not described data clearly, tables are not included, it is only possible to follow by observing one figure, but which lacks sufficient resolution.

Two main conclusions arise from the MS, not based on the results obtained, 1.- "methane production occurred during regenerated production in Pacific derived water" and 2.- methanogenesis takes place in bacterial cells.

In my view, it is very difficult to reach such conclusions based only on oceanographic data. Except for phytoplankton composition, neither biogeochemical rates nor microbial biomass and abundance are included.

This means that it is not possible to determine any causal relationships due to the lack of biological measurements (bacterial abundance by epifluorescence microscopy or flow cytometry and even molecular characterization). In the same sense, it is very speculative to assume that microbial cells (model approach) can support (perform)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

anaerobic pathways if there is no cell abundance available for the studied transect in order to associate them with high methane concentrations. I believe that the model is out of context.

On the other hand, the concept of new and regenerated production methods are based on the origin of the nitrogen nutrients, but not on carbon sources; so the statement that DMSP released during the melting of the ice in the PW potentially favours regenerated production is not correct or is not well explained.

Source of nutrients and its concentration may be due to water mass, in this case PW or AW.

The author convincingly introduced a mechanism of CH₄ production in the central Arctic (Damm et al. 2010). They discussed the potential role of DMSP (dimethylsulfoniopropionate) degradation products as precursors for methane formation and propose methylotrophic methanogenesis as the principal pathway, using direct (spike experiments) and indirect evidence; but this explanation is not explored in the MS, only an inverse relationship between DMSP and CH₄ concentration is mentioned (without any statistical support).

Other important physical mechanisms that should be considered, is the effect of sea ice melt. Gas concentration (i.e., O₂ or N₂O) in sea ice is lower than in the water column, so a CH₄ “enriched” or “poor” layer could be the effect of sea ice melting or formation, and gas distribution could be associated with the distribution of polynyas.

The MS was very difficult to understand and messy. The lack of clear structure becomes very hard to follow. I recommend the MS to be rewritten and analyses within an oceanographic context taking into account the oceanographic setting of the region, considering water mass composition and exploring other physical mechanisms. The authors can reinforce the MS by looking for more explicit biogeochemical correlations. I would like to see this MS published, but it must be reformulated.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper