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Biogeosciences
Discussions

Interactive comment on "Methane production in aerobic oligotrophic surface water in the central Arctic Ocean" *by* E. Damm et al.

M. Scranton (Referee)

mscranton@notes.cc.sunysb.edu

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This is an interesting addition to the decades-long search for the source of excess methane in the surface ocean. The present study is intriguing and the data are very interesting. The combination of field samples and incubation data are quite convincing and I am impressed that the authors FINALLY have found a process that appears to result in significant rates of aerobic methane production in natural samples.

I have a few comments which might help strengthen the authors' presentation and clarify a few points of concern.

One of my biggest questions is related to whether or not methane from coastal waters (which the authors admit are likely to be very high in methane) could contribute to central Arctic methane excesses. At present the manuscript simply states “Offshore transports of dissolved methane are rapidly reduced by open ocean dispersion, sea to air flux and methane oxidation. . ..” To me this statement is relatively weak. Is there any specific information on how rapidly this happens in the Arctic? Sub-mixed layer transport over long distances has been seen in the open ocean (eg Scranton and Farrington 1977 study off Namibia), and while the high methane levels are seen here in the top 50 m of the water column, the sigma t plot shows some density structure in the water. The transport pathways of water in the study area are not discussed and considering the ice cover, one would expect air-sea loss in Arctic might be much slower than in open Atlantic. Perhaps a statement expanding slightly on why T.D. Lorenson (personal communication) believes that transport is not important would be helpful. How long is the likely transport time from methane rich shelf areas? What is the likely time constant for gas loss? (Usually gas equilibration for methane is estimated to be on the order of a few weeks so if coastal waters are extremely high, elevated methane levels caused by coastal supply could extend long distances.)

P10367 Line 20: Recently there has been a “buzz” about the Karl et al experiment which suggested methane production in phosphate depleted cultures. However these experiments were done in culture so I am not yet convinced that the process they studied is actually important in the open ocean. The present experiments, since they were done on whole water samples, are more compelling to me, although the details of the mechanism may not be well understood. No need here to be apologetic about getting a different result from Karl and colleagues, although of course it is worthwhile citing that work. I suggest a little more detail on the extent to which each of the two studies mimicked nature.

P10368 The authors provide a lot of detail in how they calculate ∆Go’. This is probably a good idea for a general audience, but I think the detail could be reduced somewhat by simply stating, for example, that temperature corrections have been made based on the van’t Hoff equation, and that the values have been corrected for dissolution of

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the gases in water using Henry's law constants with sources for the various constants provided. On the other hand, this brief section might be useful to others who have not done such calculations before. One question I have is whether it is really accurate to assume that HS- concentrations within the cell are 1 mM. Since this is a product of the reaction, the value for ΔG will be quite sensitive to choice of HS- and 10 mM is 100,000 times the value for methane concentration. Where does this number come from? Some references would be helpful to the more geochemical of the readers of the paper. Also the maximum methane concentration measured is apparently around 5 or 6 nM, not 10 nM. Why pick 10? Can a brief discussion of sensitivity of result to choice of concentration be presented?

P10372 The conclusions are written in a very positive manner, and I think are somewhat stronger than is justified by the results (especially lines 9-10). The authors have demonstrated that methane production and DMSP and nitrogen/phosphate ratios are somehow related, but I don't think they have PROVEN that the mechanism they propose is the controlling one. The text of the paper is written in a more qualified manner, but the conclusions should also reflect this (since many may read only abstract and conclusions and misunderstand the level of confidence involved.)

Technical (editorial) comments

P10356

Line 6: Replace "Both" with "The two"

Line 8: rephrase as ". . . phosphate is available as a P source."

Line 14: I suspect this should be "methylotrophic", not "methylothrophic". If methylothrophic means something specific in microbiology, it should be defined here as the word is not commonly used in the oceanographic community. Hyphen should be after l, not after y.

Line 19: The "methane paradox" so called was discussed in the earliest papers which saw excess methane in the surface waters. Not sure why Kiene now lays claim to the term.

P10357

Line 1: Karl's study was in lab culture, not in field samples.

Line 8: "requirement, to trigger" should be "requirement for triggering"

Line 12: Again "methylotrophic", not "methylothrophic"

P10358

Line 8: Probably delete hyphen in de-nitrification.

Line 20: should be "transports dissolved methane mainly along isopycnals into.."

P10359

Line 11: Consider use of L for liter.

Line 16: "glassfiber", not "glasfibre"

P10360

Line 18: "Belemnite" not "belemnit"

P10361

Line 1: " transferred from . . . bottles into sterile"

Line 2: I think "silicone" not "silicon"?

Line 5 and 6: "in" not "at"

Line 17: "Nuclepore" not "nucleopore"

Line 25: Delete "those,"

P10365

Line 12: “Preponderantly” is a very awkward word. How about “dominantly” or “primarily”?

P10369

Line 6: Should be van’t Hoff.

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