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Biogeosciences
Discussions

Interactive comment on "Fine scale variability in methanol uptake and oxidation in the micro-layer and near-surface waters of the Atlantic" *by* J. L. Dixon and P. D. Nightingale

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Referee 2 (C1869) 6) Based on a significant correlation between methanol incorporation into biomass and the abundance of Prochlorococcus, the authors suggest that this cyanobacterial group is capable of methanol oxidation. While this observation has certain interest, it is not a direct proof, and indeed no correlation was found for the same parameters across the environmental gradient sampled. Therefore, in my view, this finding should receive less attention in the discussion.

This sentence has been edited to 'The statistically significant relationship observed between the uptake of methanol into cellular biomass and the numbers of Prochloro-

coccus during the diel experiment could suggest that, this abundant group of marine cyanobacteria, are capable of mixotrophy using methanol as a carbon source for growth, although such correlations are not direct proof.'

These authors do not agree that this finding should receive less attention in the Discussion, as we think it is important to indeed discuss why this correlation does not hold across all samples stationed. However on balance, and with the addition to the discussion of points raised by Reviewer 1 (see 2-4 of Respone to Reveiwer 1 - C1739), this sections is less dominant overall. Furthermore, the relationship with Pros has been removed from the Abstract and replaced with a sentence summarising methanol bacterial growth efficiencies.

7) The fact that methanol oxidation rates were not correlated with heterotrophic production, however, does not necessarily imply that other (non heterotrophic) microorganisms are involved in this process (as the authors suggest). In general, respiration of organic compounds is a much more stable process than biomass production, and these two processes are not always correlated in marine systems.

Yes, we thank the reviewer for this comment. A new section in the Discussion has been added;

'No statistically significant linear relationships were found between methanol oxidation and bacterial production. However this is perhaps not surprising given that respiration of organic compounds is a much more stable process than biomass production e.g. del Giorgio and Duarte, 2002, and these two processes are not always correlated in marine systems (e.g. Duarte et al., 2001). Indeed, perhaps an answer to the question posed in del Giorgio and Duarte, 2002 'what are the sources of organic matter that fuel these relatively high rates of respiration in the surface waters of oligotrophic oceans?' could be partially addressed by looking at respiration of methanol and other biologically reactive oxygenated volatile organic compounds, especially given the high nanomolar concentrations of methanol in the North Atlantic gyre (up to 429nM, Beale et al., 2011,

Dixon et al., 2011b). However what remains elusive is the source(s) of methanol in the North Atlantic gyre, given that the air to sea flux is estimated to be very low in these regions (Dixon et al., 2011b), and methanol oxidation rates are measurable below the mixed layer down to depths of 1000m.'

The sentence in the Conclusion stating that the '..lack of correlation between methanol oxidation and heterotrophic production implies that other micro-organisms are utilising methanol as an energy source' has been removed.

8) An interesting but not widely discussed point of the manuscript is the relatively high rates of methanol oxidation below the surface. It should be taken into account that prokaryotic abundance sharply decreases in mesopelagic waters, and therefore the specific activity of methanol oxidation (rates per cell) may be comparable for surface and deep waters. Could that be calculated?

In my opinion, to divide the methanol oxidation rates by the total number of prokaryotes would be a questionable calculation, as it would imply that all bacteria are capable of utilising methanol as an energy source. However the general decline in bacterial production with depth and relevant discussion has now been added into the Discussion section (please see Response to Reviewer 1 (C1739).

In addition we have only sampled 3 depths in the mesopelagic zone (500, 700 and 1000m) from one CTD cast, so we think it's a little premature to focus too much on the methanol results for these depths, given the sparse coverage. However, we have added a sentence pointing out that methanol oxidation rates were measurable at these depths (see response to 7 above).

9) Do the authors have estimates of methanol concentrations in deep samples? That would be a great addition to this dataset.

Unfortunately for this cruise we do not have any concentration measurements, see response to 5a and b for Reviewer 1 (C1739).

10) The fact that methanol oxidation/uptake was detectable below the mixed layer (and even down to 1000 meters) invites the question of what may be the sources of this compound in oceanic deep water masses.

Yes it does, however we do not know the major sources of methanol in the surface ocean either, as air-sea flux calculations suggest that the atmosphere is not a major source of methanol to the upper ocean. This has now been added to the Discussion as detailed in 7 above.

11) The authors may consider changing the title of the manuscript as it does not refer to the results of deep measurements of methanol uptake/oxidation or the diel variability.

Changed to 'Fine scale variability in methanol uptake and oxidation: from the micro-layer to 1000m'

12) Some of the statements made by the authors do not seem statistically significant (such as higher bacterial leucine activity in low Chlorophyll versus high Chlorophyll regions, page 4522).

Agreed. Page 4522 statement changed to 'Interestingly, some relatively high bacterial leucine activities up to 39 pmol L-1 h-1 were found in the lower chl surface waters.' Also in lines 7-10 and 23-25, a comment has been added that due to high sample variability the average differences are not statistically different.

Also the start of the Conclusion (p. 4527) has been changed to 'Average surface methanol oxidation rates range between 0.006-0.39 d-1 and agree with those previously published.' As methanol oxidation rates were not statistically different between the two Chl regions.

13) In page 4524 authors refer to Fig. 8a, while they should refer to Fig. 9a

Figure numbers have been changed as Figures 2 and 3 have been combined into a new Table (see response to Reviewer 1 C1739). All Figure and Table references have been checked.

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