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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

Anonymous Referee #2

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General comments: This study combines measurements of methanol uptake/oxidation along several depth profiles and during a diel experiment in North Atlantic waters. In contrast to the traditional view of highly specialized methylotrophs growing solely on C1 compounds, the recent finding that members of the ubiquitous SAR11 clade are able to obtain energy from these substrates suggests that this process may be more widespread than commonly assumed. Thus, quantifying the relevance of C1 compounds metabolism in seawater is of great interest. 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 corre-

lation was found for the same parameters across the environmental gradient sampled. Therefore, in my view, this finding should receive less attention in the discussion. By contrast, the correlation between bacterial heterotrophic production and methanol incorporation into biomass seems more robust and suggests that methanol uptake is related to general heterotrophic activity. 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.

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

Specific comments:

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.

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).

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

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