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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 #1

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The authors present results on the utilization of methanol as a carbon or energy source by the microbial community in the NE Atlantic Ocean. The use of methanol by the microbial community was already demonstrated by the same authors, and the results presented here cover depth profiles ranging from the surface microlayer to 1000 m and thus complement previous publications on this issue. Overall, the paper is clearly written, but the presentation of the results could be more synthetic. I suggest the data presented in some of the figures should be presented in Tables. The discussion is too much focused on methanol utilization rates presented here and in previous studies, I suggest the authors include other aspects into their discussion (see some comments below). As previously demonstrated, methanol is predominantly respired and only a

small fraction is transformed to particulate organic matter. This pattern appears to be independent of depth. How does this pattern compare to other compounds, or for example to bacterial growth efficiency in general? The authors have made an effort in looking at fine scale variability of oxidation and uptake rates, and these results are original. I suggest, however, the authors expand their discussion on these results to what has been reported on other measures of microbial activity in the top layers of the ocean. One surprising finding of the present data set is that the range of the methanol oxidation rates and the uptake into the particulate phase does not substantially change with depth. Even though there are only a few data for the depth layers below 100m, they suggest similar oxidation and uptake rates as in surface waters. From leucine or thymidine incorporation we know that bacterial heterotrophic activity is substantially lower in deep waters compared to surface waters. I suggest the authors discuss their results in the context of a generally observed decrease in bacterial heterotrophic activity as observed by many other studies. In their previous publications methanol concentrations are presented together with methanol utilization rates. Do the authors have concurrent measurements of methanol concentrations in the different depth layers? If not I suggest the authors mention methanol concentrations determined previously in the study region.

Interactive comment on Biogeosciences Discuss., 9, 4513, 2012.