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# *Interactive comment on* "Basin-scale variability of microbial methanol uptake in the Atlantic Ocean" *by* Stephanie L. Sargeant et al.

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This is a valuable study that adds significantly to our understanding of methanol oxidation in the oceans. The authors report seawater methanol oxidation rates obtained with 14C tracer methods, and microbial diversity measurements, from a latitudinal transect between 40S and 50N. They find that methanol oxidation rates are correlated with SAR11 relative abudance. Overall, the reported rates of methanol oxidation are in good agreement with previous measurements, but this study is exceptional in geographical scope and exploration of variables such as community composition and depth. Interestingly, the manuscript reports an inverse correlation between bacterial production estimated by the 3H leucine method and methanol oxidation. Although there have been a number of reports previously on methanol cycling in the oceans, I see the sub-



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ject coming of age with this report, which confirms what we knew and also shows us new trends that could only have been observed by making extensive measurements across a latitudinal transect.

A couple of comments follow about aspects of the paper that could be improved.

1. I recommend commenting on the abundance of methylophaga and OM43 in the 454 data, or indicate they were not detected if that is the case. It may be that the relatively low coverage obtained in this study (386 seqs/sample) led to these taxa being undetectable. If this is the case, that should be explained so that readers new to this topic understand the issues. OM43 is not mentioned at all, but perhaps it should be, since it has been shown to be an obligate methylotroph, is one of the dominant taxa in some coastal environments, and has been shown to be a source of abundant XoxF peptides in a coastal ocean metaproteome.

2. Amplicon ratios are not as powerful as cell numbers for identifying correlations between taxa and rates, although they are much easier to obtain. So, the correlations with SAR11 are not with SAR11 cells per unit volume, which would be best, but rather a correlation between the relative success of SAR11 in the community and rates of MeOH oxidation. I suggest the authors revisit the manuscript and choose wording that conveys these issues to oceanographer readers, who often misunderstand this aspect of relative abundance data.

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