

Interactive comment on
**“Dimethylsulfoniopropionate (DMSP) and
dimethylsulfide (DMS) cycling across contrasting
biological hotspots of the New Zealand
Subtropical Front” by Martine Lizotte et al.**

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This is a very well written paper about DMS(P) concentrations and dynamics in one rather unexplored oceanic site, the frontal region above the Chatam Rise east of NZ. The authors measured [DMS] and [DMSP] concentrations and microbial DMSP turnovers over a relatively short period of time, sampling 3 different blooms of DMSP-producing phytoplankton. Due to the non-commercial nature of 35S-DMSP, measurements of microbial DMSP turnover, fates and DMS yields are rare in the literature. This paper provides useful data for DMSP modelers since it covers a poorly characterized

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zone although very active in terms of use of reduced sulfur in the ocean.

The methodology used is correct and well described. As a general comment, the only weakness detected on this study is that not all pools of DMS(P) cycling were covered since no measurements of DMSO were performed (particulate and dissolved) which hampers a more extended discussion on the fate of metabolized DMS in seawater.

It is really appreciated negative results of influence of light preincubations on DMSP dynamics. I think it is not stressed enough in the discussion of the paper. One thinks it is a pity than in such DMSP-active zone more specific experiments to test still open questions of the cycle, mainly related to the different physiological and ecological roles of DMSP in the upper ocean could have been tested (for instance, the relative role of non-DMSP-producers algae as sink of DMSP, algal DMS production, new in situ production of DMSP by heterotrophic bacteria, chemotaxis, etc). Rather than a weakness, I hope the paper will encourage the DMSP community to sample in the described area.

I only have minor comments on the manuscript. line 38: there is more than only 2 fates of consumed DMSP, excretion as an oxidized form but not incorporated into cell structure is missed. line 45. "measured in this study" can be deleted. line 59: Since no aerosols were measured, I wouldn't mention it in the abstract of the paper line 70: Quinn and Bates 2011 should be also cited since evidence for climate regulation though DMS still needs to be proven. line 92: misplacement of the (line 149: the sentence should read "...the potential climatic relevant gas..." line 218: were the samples fixed with any fixative? P+G? line 221: Dinoflagellate abundance was determined? lines 314-325: Very interesting results that can be more discussed after Ruiz-Gonzalez et al. ISME Journal (2012) 6, 650–65, for instance. line 448: "Microbial affinity for DMSPd as indicated by" can be deleted line 651: I love Table 3 line 665: Could cyanobacteria be included? Were them measured by flow cytometry? It is a pity no taxonomical description of the communities could be performed. line 748: What about the role of algal oxidative stress? do you have any measurement indicating senescence of the bloom during the sampled period of time? line 789: "much needed" can be deleted.

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