

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

**Martine Lizotte et al.**

martine.lizotte@qo.ulaval.ca

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Response Reviewer 1. os-2017-32 Lizotte et al.

1. 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.

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Answer ML. Indeed, DMSO measurements would have been a very appreciable addition to the paper. Unfortunately, they were not available, and we therefore cannot rule on the fate of certain pools.

2. 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.

Answer ML. We agree with the reviewer. We furthered the discussion by adding some information in the methodology section (lines 334-348), please see point#11 of this review for the full description of the added information.

3. 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.

Answer ML. To address this we changed the phrase: “This study focused on the two opposing fates of DMSP-S following its uptake by microbial organisms: either its conversion into DMS, or its assimilation into bacterial biomass.” by the following phrase: “This study focused on two opposing short-term fates of DMSP-S following its uptake by microbial organisms: either its conversion into DMS, or its assimilation into bacterial biomass.” Then we also added information about the third fate in the introduction section: “Another potential fate for DMSP is its transformation into dissolved non-volatile degradation products (DNVS), including sulfate (SO<sub>4</sub><sup>2-</sup>), however less is known of the molecular pathways involved in this process (Kiene et al. 2000; Reisch et al. 2011).”

4. line 45. "measured in this study" can be deleted.

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Answer ML. The words “. . .measured in this study. . .” have been deleted.

5. line 59: Since no aerosols were measured, I wouldn't mention it in the abstract of the paper

Answer ML. The following phrase: “The findings from this study provide crucial information on the distribution and cycling of DMS and DMSP in a critically under-sampled area of the global ocean, and they highlight the importance of oceanic fronts as hotspots of the production of marine biogenic S compounds and as potential sources of aerosols particularly in regions of low anthropogenic perturbations such as the frontal waters of the Southern Hemisphere.”, was changed to: “The findings from this study provide crucial information on the distribution and cycling of DMS and DMSP in a critically under-sampled area of the global ocean, and they highlight the importance of oceanic fronts as hotspots of the production of marine biogenic S compounds.”

6. line 70: Quinn and Bates 2011 should be also cited since evidence for climate regulation though DMS still needs to be proven.

Answer ML. The following phrase: “DMS has gained notoriety over several decades of research on the grounds of its potential role linking ocean biology and the climate (Andreae et al., 1985; Charlson et al., 1987; Lovelock et al., 1972).” Was changed to: “DMS has gained notoriety over several decades of research on the grounds of its potential role linking ocean biology and the climate (Andreae et al., 1985; Charlson et al., 1987; Lovelock et al., 1972), a role that is still under debate (Quinn and Bates 2011, Quinn et al. 2017).”

7. line 92: misplacement of the (

Answer ML. The parenthesis in the following phrase now conforms to the requirements: “These productive regions sometimes form unique biogeographic habitats of their own such as the Subtropical Convergence province proposed by Longhurst (2007).”

8. line 149: the sentence should read "...the potential climatic relevant gas..."

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Answer ML. The following phrase: “Depending on bacterial requirements for either S or C and the relative contribution of DMSP to the overall oceanic S pool (Kiene et al. 2000; Levasseur et al 1996; Pinhassi et al. 2005), at least two very different and competing outcomes are involved from the bacterial catabolism of DMSP: one producing DMS, the climatic relevant gas, the other producing methanethiol (MeSH), an important microbial substrate (Kiene and Linn, 2000b).”, was changed to: “Depending on bacterial requirements for either S or C and the relative contribution of DMSP to the overall oceanic S pool (Kiene et al. 2000; Levasseur et al 1996; Pinhassi et al. 2005), at least two very different and competing outcomes are involved from the bacterial catabolism of DMSP: one producing DMS, the potential climatic relevant gas, the other producing methanethiol (MeSH), an important microbial substrate (Kiene and Linn, 2000b).

9. line 218: were the samples fixed with any fixative? P+G?

Answer ML. No paraformaldehyde nor glutaraldehyde were used, rather the samples were snap-frozen in liquid N<sub>2</sub> and quickly analyzed upon return to land. The following phrase: “Bacterial samples were frozen in liquid nitrogen (Lebaron et al., 1998) and thawed immediately before counting by flow cytometry following the methods described in Safi et al. (2007).” Was changed to: Bacterial samples were snap-frozen in liquid nitrogen (Lebaron et al., 1998) and thawed immediately before counting by flow cytometry shortly after the cruise following the methods described in Safi et al. (2007).”, in order to make it clearer.

10. line 221: Dinoflagellate abundance was determined?

Answer ML. Dinoflagellate abundance was determined in surface samples for all stations but not systematically for the “near surface” samples from which the incubation experiments were derived in this paper. It is thus possible to provide some information about the overall “regional” conditions of phytoplankton dominance shown in Table 1 but not to discuss the specific near surface abundances. The phytoplankton speciation

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data will be discussed in a separate DMS/marine biogeochemistry paper. Nevertheless we modified the information by adding a phrase: "Coccolithophore abundance in near surface waters was determined using optical microscopy as described in Chang and Northcote (2016)." Dinoflagellate abundance was determined for surface waters (not for near surface waters) and is not shown here."

11. lines 314-325: Very interesting results that can be more discussed after Ruiz-Gonzalez et al. ISME Journal (2012) 6, 650–65, for instance.

Answer ML. It is true that the absence of a significant difference between pre-incubation treatments is interesting in itself. We added some discussion on this, referring to Ruiz-González et al. (2012) and other publications (specifically related to the sulfur-relevant responses) but also more particularly to the review published by Ruiz-González et al in 2013 which clearly shows that the past 20 years of research on sunlight-bacteria interactions display a wide-range in responses (from negative to positive effects of natural sunlight on metabolic activity of heterotrophic bacteria) intimately linked with factors such as the phylogeny of bacterial groups under investigation, the light-history experienced by the natural populations, and many more. The added information is in the following section: "On the whole, the light conditions (dark and ambient) at which the cells were pre-acclimated for 6 h had no significant effect on the 35S-DMSPd metabolic rates measured. This result contrasts with findings from earlier studies (such as Galí et al., 2011; Ruiz-González et al., 2012a; Slezak et al., 2001, 2007; Toole et al., 2006) and could be related to a number of variables such as the timing and depth of sampling, the type of bacterial assemblages present and their previous light-history, as well as the different temporal and spatial scales at which exposure to solar radiation varies (Ruiz-González et al., 2013). Because of these wide-ranging and intricate light-bacteria interactions, natural solar radiation is believed to play a significant, yet challenging to predict, role in modulating bacterial dynamics and biogeochemical functions (Ruiz-González et al., 2013). In the current study, the sulfur-related metabolic activities of the marine biota sourced in the morning (between ca. 7h00 and 9h00; Table 1) from

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the highly irradiated near surface waters may have persisted in the dark within the time period of experimental pre-exposure (6 h), however the lack of information on the phylogeny of bacterial groups present, for example, hampers a more detailed discussion. We therefore present rate measurements made in dark-incubated samples that had been pre-exposed to ambient light conditions for 6 h."

12. line 448: "Microbial affinity for DMSPd as indicated by" can be deleted

Answer ML. Yes. The following phrase: "Microbial affinity for DMSPd, as indicated by the 35S-DMSPd loss rate constant ( $k_{DMSPd}$ ; Fig. 3a) varied between 0.4 and 3.4 d<sup>-1</sup>, with the exception of a higher value of 19.9 d<sup>-1</sup> measured in the B2 cluster at station 5." Was changed to: "The 35S-DMSPd loss rate constant ( $k_{DMSPd}$ ; Fig. 3a) varied between 0.4 and 3.4 d<sup>-1</sup>, with the exception of a higher value of 19.9 d<sup>-1</sup> measured in the B2 cluster at station 5."

13. line 651: I love Table 3

14. line 665: Could cyanobacteria be included? Were they measured by flow cytometry? It is a pity no taxonomical description of the communities could be performed.

Answer ML. Yes it is indeed a good idea to mention cyanobacteria here as they have been shown (particularly *Synechococcus* and *Prochlorococcus*) to participate in DMSP assimilation. The following phrase: "It has been suggested that loss rate constants of DMSPd, rather than being directly related to stocks of bacteria could be more related to bacterial community composition, and particularly the specific abundance of *Roseobacter*, a member of Alphaproteobacteria, and with Gammaproteobacteria (Royer et al., 2010), which are both significant contributors to DMSP metabolism (Malmstrom et al., 2004a, 2004b; Vila-Costa et al., 2007; Vila et al., 2004).", was changed to: "It has been suggested that loss rate constants of DMSPd, rather than being directly related to stocks of bacteria could be more related to bacterial community composition, and particularly certain members of Alphaproteobacteria, Gammaproteobacteria as well as cyanobacteria, that could all potentially represent significant

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contributors to DMSP metabolism (Malmstrom et al., 2004a, 2004b, 2005; Royer et al., 2010; Vila-Costa et al., 2007; Vila et al., 2004). The appropriate references were also added (Vila-Costa et al 2006a as well as Malmstrom et al. 2005). In reference to the other questions: we agree, it is highly unfortunate that no taxonomical description is available for the heterotrophic bacteria and picoplankton communities. This also limits our comprehension of the response of the biotic community under the different pre-incubation light exposure scenarios.

15. 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?

Answer ML: Measurements of photosynthetic efficiency ( $F_v/F_m$ ) would have indeed been appreciable here, but are unfortunately not available. However we modified the phrase to reflect this possibility. The following phrase: "Community DMS production may have included indirect processes such as zooplankton grazing, viral lysis, and senescence, as well as direct algal DMSP-lyase activity associated with the presence of certain species of dinoflagellates and coccolithophores (Niki et al., 2000; Wolfe and Steinke, 1996), ubiquitous in Subantarctic waters in early March.", was changed to: "Community DMS production may have included indirect processes such as zooplankton grazing, viral lysis, and senescence, as well as direct algal DMSP-lyase activity associated with the presence of certain species of dinoflagellates and coccolithophores (Niki et al., 2000; Wolfe and Steinke, 1996), ubiquitous in Subantarctic waters in early March, and potential algal oxidative stress associated to light or nutrient availability (Stefels et al., 2007; Sunda et al., 2002).

16. line 789: "much needed" can be deleted.

Answer ML: The phrase "Our study provides much needed information on both concentrations and cycling of dimethylated sulfur compounds within waters of the New Zealand biogeochemical province (NEWZ) and more specifically in an oceanic frontal region." Was changed to: "Our study provides information on both concentrations and

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cycling of dimethylated sulfur compounds within waters of the New Zealand biogeochemical province (NEWZ) and more specifically in an oceanic frontal region."

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