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Biogeosciences Discussions

Interactive comment on "The use of algorithms to predict surface seawater dimethyl sulphide concentrations in the SE Pacific, a region of steep gradients in primary productivity, biomass and mixed layer depth" *by* A. J. Hind et al.

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We thank the two reviewers for their very useful comments and suggestions and we shall use them to improve the quality of this work. In most cases, we are in complete agreement and will make the suggested changes. Some are outside of the scope of this manuscript which we intended to be an analysis of the existing DMS algorithms using this high resolution dataset. We included examples of the gradients observed both over small (3° longitude) and larger (14° longitude) scales to be illustrative of the relationships (or lack there of) between measured parameters and seawater DMS and

to highlight the difference between the algorithms. We completely agree a more in-depth study of the impact of the regional oceanography on DMS is worthwhile and are currently preparing a companion paper examining physical features; it is because of this 2nd manuscript, currently in its final stages, that we respectfully request certain changes, as indicated below, are not included in this submitted manuscript as they are not necessary to convey its main conclusion.

The major changes we shall make to this manuscript are to: 1) Improve the description of the physical and chemical conditions and to more clearly display the DMS and chlorophyll data. 2) Revise the text pertaining to the algorithm details and their application, making it more succinct and easier to follow. 3) Apply algorithm BE04 to SeaWiFS and in situ chl data and compare those results to those in the existing manuscript.

Response to issues identified by both reviewers

Both reviewers requested a more detailed description of the study area. We propose that we replace Fig. 1 with figures containing surface plots of chlorophyll and DMS. Additionally, we shall include a contour plot of sea surface height anomalies and mark on this figure the positions of eddies and fronts, with a corresponding description of conditions.

We agree that the detail relating to the algorithms is unclear so will shorten the descriptions and condense the text from the introduction, methods and results sections to improve this.

Response to the comments of Dr. Belviso

Samples for algal community composition were collected during the VOCALS 2008 campaign. Small volumes were taken for species identification and cell counts by scientists from the Peruvian Institute of Marine Research (IMARPE) but not for HPLC. However, the data are not yet available for publication and hence, unfortunately, the AU02 parameterisation cannot be investigated at this point. Additionally, the frequency

of these phytoplankton composition samples was comparatively low compared to the DMS, light or MLD data so the algorithm results produced from them would not be very comparable. The suggestion to examine the pCO2 data, which is of a high resolution, is interesting and will be considered in the companion paper.

Dr. Belviso asks that further investigation of DMS in relation to other geochemical parameters be made; however, as with the plankton composition samples, the nutrient and DMSP measurements are too few and thus the potential to resolve the finer detail is lost, again making the results incomparable to those produced from the higher resolution data. However, we will now include some macronutrient and DMSP data as it will be useful to help describe the conditions in SE Pacific during the period of study.

"Therefore, I would like to see a better assessment of the specific aspects of the DMS distribution in the whole investigation area with fine horizontal resolution (e.g. location of fronts and eddies) not just along a transect between 79 and 82 °W (figure 3). There are also huge gradients in DMS south of Arica or around 76°W"

The 79°W to 82°W transect displayed in Fig. 3 was selected as a special case of a particularly sharp transition from "typical" oligotrophic ocean DMS concentrations to high values, actually the highest we measured in the region. These higher values (> 14 nM) are higher than reported in oligotrophic regions across five AMT cruises (Bell et al., 2006) suggesting this is either an unusual or usually overlooked feature. The figure highlights the heterogeneity in the region not shown by the DMS climatologies. Also, it exemplifies a situation where chlorophyll and DMS are correlated partially explaining why AN01 and AT04, which use chl as an input variable, perform better than VS07 and MI09, which do not.

We did not plot or discuss the DMS peak at 76°W as it is smaller (6-7 nM) and the ship's track did not pass across the low DMS values, through high values and back to low values, indicating we likely did not transect the feature completely. Also, the DMS hotspot south of Arica is notable, yet less remarkable, as high concentrations

have been reported near shore previously (e.g. Andreae, 1986) so we do not think this warrants a detailed description or figure here.

We agree BE04 could be applied in this area both using data from both SeaWiFS and the underway chl and will include this in the revised manuscript.

We thank Dr Belviso for highlighting the misunderstandings of his and his colleagues' work and will correct the manuscript accordingly.

Response to the specific points of Anonymous referee #2

"It could also be useful to have a comparison with other in situ DMS measurements to show if there is a general behavior in that region."

In the region similar datasets were collected in 2006 and 2007. However, these are not accompanied by either in situ chlorophyll, MLD or hyperspectral light so for the most part the algorithms can not be tested using this older data but we will include a brief discussion of the similarities and differences between the datasets.

"The inclusion of calculated DMS flux emissions. . ."

The DMS flux emissions have been calculated and published in Yang et al., 2009. They were not included in this manuscript as this study concerns the surface seawater concentration only. We shall reduce the emphasis on the flux data in the introduction.

"On p.5338 Table 2 gives "a summary of predicted DMS values and the relation of these to the measured values". It could be much more transparent if this table was translated into a graphic representation in order to see the most appropriate algorithms and variables. Again a proper two dimensional graphic with the result that fits better with the in situ measurements could be very useful to compare with the values obtained during the track of the cruise, that should also be included, as least with the DMS concentration values."

Agreed

“Minor comments”

We thank the anonymous referee for the suggestions to increase the clarity of Figs. 3 & 4 and for the detection of the reference error, dead link and typos. In Fig. 3 we shall move the DMS axis to the left and the chlorophyll and MLD axes to the right as in Fig. 4. Additionally in Fig. 3 the MLD data shall be inverted to correspond with Fig 4. The scale of Fig. 4 will be adjusted to that used in Fig. 3.

Reference

Yang, M., Blomquist, B. W., and Huebert, B. J.: Constraining the concentration of the hydroxyl radical in a stratocumulus topped marine boundary layer from sea-to-air eddy covariance flux measurements of dimethylsulfide, Atmospheric Chemistry and Physics 9, 9225–9236, 2009.

Interactive comment on Biogeosciences Discuss., 7, 5305, 2010.