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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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The aim of this study is to assess the performances of several published algorithms to ultimately predict the marine emissions of DMS to the atmosphere in the SE Pacific region and their impact on the radiative budget of the area because DMS is a source of aerosols and of cloud condensation nuclei with potential strong interactions with stratocumulus clouds. Overall, the longitudinal variations of seawater DMS concentrations along 20°S were best reproduced by the algorithm proposed by AN01 (Anderson et al., 2001).

The authors closely followed the computation schemes presented by AN01, SD02, AT04, VS07 and MI09. AU02 was not tested as the supporting measurements to determine the Fp-ratio were not made (p5317, lines 8-9). This is rather surprising since Hind et al. 2009 (AGU Fall meeting 2009, abstract # A51D-0147) examined algal community composition during the VOCALS field campaigns. The algal community composition allows the characterization of the proportion of microphytoplankton within the whole phytoplankton community and the assessment of the Fp ratio. Please be more precise and specify whether or not high-performance liquid chromatography (HPLC) analysis of accessory pigments was used to assess the algal community composition along the W-E transects. Nevertheless, the authors missed an excellent opportunity to critically evaluate BE04 since this algorithm makes use of nonlinear parameterizations to relate DMS concentration solely to Chl a (see the content of the next paragraph). The authors also missed an excellent opportunity to look for a "home-made" relationship using the different biogeochemical parameters measured during the VOCALS campaign (e.g. Chl a, nutrients, underway pCO2 or a combination of the different parameters, http://www.eol.ucar.edu/projects/vocals/documentation/Ship-Based_Final1.pdf). An illustration of the high value of pCO2 at constant temperature to document the origin of DMS within and in the vicinity of coastal upwellings can be found in Belviso et al. (DSR I, 50, 543-555, 2003). Moreover, it is likely that the spatial distribution of DMS off Chile is also highly sensitive to the upper ocean dynamics (eddies, fronts, upwellings). 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 which likely can provide valuable information on how DMS is produced in the Pacific Ocean. What about DMSP measurements too?

The authors missed an excellent opportunity to critically evaluate BE04 for wrong reasons. The following paragraph (p5310, lines 8-17) contains a series of wrong statements (in italic): "In this case the Fp ratio is estimated using Seaviewing Wide-Field-

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of-view Sensor (SeaWiFS) chlorophyll concentrations and a relationship derived from Atlantic Ocean measurements. However, their empirically derived relationship did not reproduce the substantial variability of Fp at low chlorophyll concentrations. A similar problem was found for DMSPp which displayed chlorophyll independent variability so ultimately the predictions of DMSP at low chlorophyll were (very nearly) constant, again losing the natural variation. These factors combined resulted in poor resolution of DMS in oligotrophic regions and, for example the equatorial Pacific seasonality that is found in K00 data is not seen. More generally, BE04 underestimated seawater DMS."

1- Sentence reproduced from Belviso et al. (CJFAS, 61, 804-816, 2004): "The amount of available data is thus crucial in this approach to ensure the validity of the relationships at the global scale. We used pigment measurements performed during five cruises carried out in the Atlantic Ocean, the Mediterranean Sea, and the Indian sector of the Southern Ocean (Belviso et al. 2001) to relate the Fp ratio to the Chl a concentration."

2- There are no discontinuities in the Fp vs Chl a relationship (see Figure 1a, Belviso et al., CJFAS, 61, 804-816, 2004). Therefore, the relationship did reproduce the substantial variability of Fp at low Chl concentrations.

3- Nano-DMSPp is predicted from nano-Chl a using the same kind of "broken-stick" regression used by AN01. Both AN01 and BE04 relationships are losing some of the natural variation but BE04 simulates the general increase in DMSPp-to-Chl a ratio in oligotrophic systems. Remember that DMSP is an important intermediate in DMS production and that you can evaluate BE04 also against DMSP measurements.

4- BE04 did not result in poor resolution of DMS in oligotrophic regions (e.g. Fig. 3b and Fig. 5i in Belviso et al., CJFAS, 61, 804-816, 2004; or Fig. 5c in Belviso et al., GBC, 18, 2004). Data of K00 in PEQD is reproduced in Fig. 3c in Belviso et al., CJFAS, 61, 804-816, 2004. There is no clear seasonality in DMS in the Equatorial Pacific, nothing comparable with the seasonality in the northern high latitudes of the Atlantic Ocean.

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I also disagree with the following statement: “BE04 also was not used as it is described as performing poorly in the VOCALS region” (cf. p5317, lines 9-10). Indeed BE04 performed poorly in PEQD but PEQD is only a small part of the VOCALS region. The present paper mainly investigates the variations in DMS along 20°S (Figures 3-5), an area that was not considered in the different validation exercises carried out by Belviso et al. in their papers dated 2004.

Finally, I also disagree with the statement: “Belviso et al. (2004a) also found that AN01 performed well globally it reproduced DMS concentrations best of those they tested.” (cf. p5324, lines 16-17). In the discussion section of the GBC paper dated 2004 (co-authored by Dr. Anderson) we wrote: “Our recommendation for those studying the atmospheric DMS cycle over the Pacific Ocean would be to use either the climatology of Simo and Dachs [2002] or that from Chu et al. [2003], because both accurately track the observations in the equatorial Pacific. The Anderson et al. [2001] climatology also provides the right order for annual mean concentrations, but it does not resolve the variations in sea-surface DMS concentration. Anderson et al. state that the simplicity of their algorithm makes it suitable for use in global studies, although it may not capture local variability in DMS. Unfortunately, none of these climatologies adequately resolve the highs in DMS found in the North Pacific (Figure 7c).”

I look forward seeing those sentences rewritten or removed in the revised manuscript.

All in all, I think that this study is valuable information for the members of the DMS community but the authors need to work hard on improving the cruise description and the presentation of DMS data (discuss the role of eddies, fronts and upwellings with the help of physical (T, S, excess density and MLD) and biogeochemical data. Add new figures). Make your own guess about the processes responsible for the variations in sea surface DMS along 20°S but also along the latitudinal transect at 85°W. Shorten the presentation and the discussion of the algorithms predicting surface seawater DMS concentrations but include BE04 in the exercise and AU02 if you have access to HPLC data or cell counts.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C2405/2010/bgd-7-C2405-2010-supplement.pdf

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