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Biogeosciences Discussions

Interactive comment on "Spatial and temporal variability of the dimethylsulfide to chlorophyll ratio in the surface ocean: an assessment in the light of phytoplankton composition determined from space" *by* I. Masotti et al.

Anonymous Referee #2

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General comments

Few years ago, Alvain and colleagues developed an algorithm (PHYSAT) allowing determining the distribution of six key Phytoplankton Functional Types (PFT) from space where and when one PFT is dominating the assemblage and biomass is not exceeding 4 mg m^{-3}. This was an important contribution to 'space' oceanography given that several ocean processes are heavily influenced by phytoplankton types. One of these processes is the production of DMS, a biogenic gas contributing to the global climate regulation. As mentioned by the authors, DMS distribution at the surface of the ocean

is generally not correlated with total algal biomass indices such as chlorophyll a. The production of DMS is dependent on the presence of strong DMSP (the algal precursor of DMS) or DMS producers in the community, and of algal DMSP-lyase, the enzyme responsible for the cleavage of DMSP into DMS. High intracellular DMSP concentrations and DMSP-lyases are found mostly in prymnesiophytes and dinoflagellates, with diatom being poor DMSP producers and having no DMSP-lyase. In addition, several abiotic and other biotic processes contribute to the net DMS production. For these reasons, attends to related DMS levels to the chlorophyll a signal from space have not been successful so far.

In this paper, the authors assessed the influence of phytoplankton speciation on the variability of the DMS:Chl ratio at the global scale. Different data sets were used by the authors in order to explore the existence of linear relationship between DMS and Chl in water masses with different PFTs. To do so, they used satellite imagery for Chl and PHYSAT, and DMS values from the PMLE global data base (plus some unpublished cruises data). In their analysis, there are thus three widely different data sets: 1) 10-year climatology for PHYSAT, 2) space measurements for Chl, and 3) in situ measurements for DMS. It is difficult to appreciate how combining these three different data sets may have impacted on the conclusions. Nevertheless, it is important for the reader to keep this limitation in mind when going through the results and discussion.

The major conclusion of the manuscript is that large scale variations in DMS:Chl ratios are not consistent within phytoplankton group. The authors should be prudent here since PHYSAT only indentifies dominant groups. As mentioned several times in the discussion, non-dominant PFTs may be responsible for most of the DMS signal in many instances. I am thus concerned by their conclusion stating: 'Our results suggest that the species composition is not a first order controller of the global DMS dynamics' (page 3633, line 2). That could be true, but the results presented in this paper do not support this conclusion. They also found a particularly clear decoupling between DMS and Chl in the Equatorial Divergence. The decoupling between DMS and Chl has been reported before and called the 'Sumer DMS Paradox' by Simó and Pedrós-Alió (1999). I was surprised to find no reference to this previous study here.

With that said, this paper is relevant to BG and includes new ideas and an in depth analysis of an important data set. The question addressed is important and them methodological approach unique. Such analysis was never attended at this scale. I thus consider that this community paper represents an important contribution to the field and should be published following some revisions.

ABSTRACT

The DMS:Chl ratio varies widely in the surface ocean also because DMS is ventilated and Chl is not.

INTRODUCTION

Page 3608, line 20: The authors should acknowledge that Chl and DMS vary on different time scales, days and hours respectively. This also contributes to the poor correlation between the two variables. As mentioned previously, ventilation which only affects DMS levels should also be mentioned.

Page 3610, line 11: It is important to specify that PHYSAT detects the main dominant phytoplankton group since this dominant group may not be responsible for most of the DMS signal under certain circumstances. This is done here, but this limitation is somewhat lost in the discussion.

I suspect that Chl have been measured along DMS during most of the cruises cited. I am wondering why the authors did not attempt to compare in situ DMS:Chl ratios with DMS:Chl ratios estimated from space?

METHODS

Page 3610, line 18: Information regarding the effect of the pump on DMSP should be removed since not relevant to this work.

Page 3614, line 1: ... According to this validation... are validated. What to you mean?

Page 3614, line 8: The poor performance of PHYSAT to detect dominance of Phaeocystis and coccolithophores is certainly not good news in the context of the present study.

RESULTS

Page 3616, line 19: These results are interesting by their own, but are not all relevant with the study. As done in the previous paragraph, I would terminate this paragraph (and the other similar ones of the results section) by a short sentence stating where the DMS surveys have been done. In that case, something like: The DMS survey covered the NANO and mixed SYN-PRO zones. This will help the reader to keep in mind the information relevant to this study.

Page 3616, line 26: Idem – I would add the following sentence: 'The DMS survey was carried out mostly in the mixed SYN-PRO zone'.

Page 3616, line 9: Need a sentence here describing where the DMS surveys took place.

Page 3616, line 9: As for the Pacific section, I suggest to begin a new paragraph for each survey (In summer...). The match between Chl and PHYSAT is obviously poor here, Fig. 3c showing more productive zones than PHYSAT. This is not clearly discussed. I am also concerned by the poor representation of the coccolithophores and a potential overestimation of the contribution of diatoms in the assemblage. Reference to the papers by Scarratt et al. (2002, 2007) which include DMS:Chl a ratios and phytoplankton composition over extended transects in the North-western Atlantic will be relevant to the discussion of these data.

Fig. 4: CN-198 (as in Fig. 1) or CN-128 (as in Table 1)?

Page 3616, line 24: This area seems to be too broad to be representative of the ice edge. The authors should be more specific.

Page 3620, line 18: Obviously, the authors worked hard to find a way to present their data. However, I still find the interpretation of Figures 5, 6, 7 and 8 difficult (the small size of the panels as well as their poor resolution – on my computer at least – make their visual analysis very laborious. Each figure should occupy the full width of the page (ex. Fig. 7 and 8?). That would help. As expected, there is a lot of variability in DMS levels and DMS:Chl ratios and in some instance this variability if not properly acknowledge by the authors (see below for an example).

Page 3618, line 11: 'DMS concentrations were systematically higher'. This is not so evident in Fig. 5e.

Page 3610, line 15: 'This is apparently in contradiction. . . '. If so, what is the value of the rest of the data presented? I am a bit harsh here, but given the complexity of the data set presented and the wide variations observed; this is an honest but certainly not reinsuring sentence. . .

Page 3623, line 1-30: It is not clear why the data from Tortell and Long are only presented in the supplemental materials. This does not make the paper easier to work through.

DISCUSSION

The beginning of the discussion does not provide a complete and up to date view of the DMS literature. The authors referred to either old (albeit good) papers (ex. Turner et al. 1988) or their own recent papers (Marandino et al. 2008). Although ignored here, information on how the DMS:Chl ratios vary with species in the field can be found in the literature. By the way, the paper by Matrai and Vernet (1997) provides a very nice example of a diatom and a Phaecystis assemblages producing similar amount of DMS.

As mentioned before, the authors never mentioned the potential role of DMS ventilation on the DMS:Chl ratio. If they consider this abiotic DMS loss term not important, they should explain why.

The 'caveats' described in the first part of the Discussion section were predictable and are not a result of this study.

Other specific comments

Page 3608, line 10: Replace 'affect' by 'effect'.

Page 3608, line 27:. . . from space by PHYSAT.

Page 3610, line 21: . . . the effect. . .

Page 3624, line 23: It seems that 'laborious' or 'tedious' would be more appropriate than 'difficult'.

Page 3626, line 9: 'With these. . . '

Page 3626, line 11: 'way than Colomb et al'. This is the firs mention of this study, which seems very important in the context of the present one. If so, this study should be presented in the Introduction section.

Page 3628, line 6: 'Clearly, DMS accumulated there too. . . ' The authors do not provide explanation for the observed increase in DMS concentration. The reference to bird foraging is speculative and does not contribute to the interpretation of the data.

Page 3629, line 7+: Could be interesting to refer to the paper by Wong et al. (2006) which also explores some potential DMS ENSO cycle in the NE Pacific.

Page 3632, line 9: 'would trace that of the oligotrophy. . . 'Not clear what the authors mean here. Needs to be reworded.

Page 3632, line 14: 'from space of several phytoplankton functional. . . '. Should read: 'from space of the dominance of several. . . '

Page 3632, line 21: About the high and low DMS-producing phytoplankton. Given the complexity of the abiotic and biotic mechanisms responsible for DMS net production, it will be more accurate to mention the 'high and low DMSP-producing phytoplankton'.

Similarly, few if any DMS researcher would expect to see variations in DMS:Chl ratios as large as variations in DMSP:Chl ratios as mentioned in Page 3631, line 13. It is misleading to not make a clear distinction between DMSP and the gas DMS given their widely different dynamics.

Page 3632, line 26: 'is not consistent within DOMINANT phytoplankton group'.

Figures

General comment: Many figures as small and busy. They should be all scale up to the full journal page width.

Fig. 2. Indicate in the legend what panels a and b, b and c, and c and d mean.

Fig. 10. Y-axis should be the same for the two panels.

Reference cited in this comment

Matrai P, Vernet M, Wassmann P (2007) Relating temporal and spatial patterns of DMSP in the Barents Sea to phytoplankton biomass and productivity. Journal of Marine Systems 67: 83-101

Scarratt MG, Levasseur M, Michaud S, Cantin G, Gosselin M, deMora SJ (2002) Influence of phytoplancton taxonomic profile on the distribution of dimethylsulfide and dimethylsulfoniopropionate in the northwest Atlantic. Marine Ecology Progress Series 244: 49-61

Scarratt M, Levasseur M, Michaud S**, Roy S (2007). DMS and DMSP in the Northwest Atlantic: Late-summer distributions, production rates and sea-air fluxes. Aquat. Sci. DOI 10.1007/s00027-07-0886-

Simó, R., and C. Pedrós-Alió (1999), Role of vertical mixing in controlling the oceanic production of dimethyl sulphide, *Nature*, 402, 396-399

Wong CS, Wong SE, Peña A, Levasseur M. 2006. Climatic effect on DMS producers

in the NE sub-arctic Pacific. Tellus 58B: 319-326.

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