

Interactive
Comment

***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 #1

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General comments:

This paper discusses the role of phytoplankton group speciation for the DMS to chlorophyll ratio, as determined along several cruise tracks. The authors use the PHYSAT method in order to determine phytoplankton dominance in the different ocean basins and chlorophyll from the SeaWiFS satellite, and pair these with ship based measurements of DMS. They find that the DMS:chl ratio varies widely across basins and meridionally, but not consistently so with dominant PHYSAT group. The authors conclude that oceanic DMS surface concentrations may not primarily be controlled by phyto-

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plankton speciation, and that it will be difficult to find a way to determine DMS concentrations from space using satellite measurements.

While this study is new, original and certainly worth publication in Biogeosciences, the paper is unfortunately poorly written and will require major corrections. The English used here is almost incomprehensible, to the point that understanding is a major obstacle to judging this piece of work. Sentences are far too long and complicated, and in consequence full of grammatical mistakes and the logic and flow of the arguments suffers from this fact. The authors must have this publication proof read by a native English speaker. In addition, there are structural issues that need to be addressed and methodological issues that need clarification (see specific comments), as many of the sections are poorly structured, confusing and far too lengthy.

Despite the impressive contributions of some of the co-authors to the field of DMS science, I was surprised to find that this paper is poorly cited – important works are not referred to, several conclusions based on studies cited in this paper are wrong, many aspects of the complexity of the pathways leading to DMS production and destruction are ignored. Some references seem arbitrarily chosen and often, only review articles are cited instead of the original studies. For example, there is no reference at all to the original work of Keller et al. 1989, which is highly relevant for this study. In addition, while some of the sources processes for DMS are discussed here, the sink processes, which are known to have an important effect on the timing of DMS accumulation are entirely neglected. I have some major scientific concerns related to this (e.g. the role of bacteria), which I will express below in the specific comment section.

An additional point worth noting here is that the manuscript lacks quantitative information in almost all its sections. Neither are the errors of the PHYSAT method (percentage of false detection) taken into account, nor is the behaviour of the DMS:chl ratio explored in terms of statistical quantities, such as giving the ranges/min/max/std of DMS:chl for all phytoplankton groups. A table summarizing these quantities or a bar chart would help, along with an estimate of the error caused by the different sensitivities of the

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PHYSAT method for individual plankton groups, and the effect this error will have on the reliability of the results in this study. The reader also lacks information on the error of the DMS measurements (a few percent), the detection limit of the individual techniques used (hopefully a fraction of a nM) etc. Needless to say that there is no mention of the error of SeaWiFS chlorophyll (ca. 30 %), which will have a large impact in regions of low chlorophyll, where DMS:chl is very sensitive to small fluctuations in chl. Most importantly, there is no statistical information proving that there is no relationship between group and DMS:chl level, except for Figures 5-9, which the reader is supposed to judge by the eye?

All in all, I think that this study is valuable information for the members of the DMS community, but the impact of this work would double, were it more understandable and quantitative. The authors need to work hard on improving the presentation of their work.

Specific Comments:

1. Temporal and spatial resolution of data used in this study

The authors use satellite data with a min. resolution of ca. 9 km and an unspecified penetration depth and match this with point measurements of DMS. Furthermore they use a monthly climatology for the dominance patterns, and daily chlorophyll. Given that DMS concentrations can increase exponentially over a few days, I have serious doubts that patterns should be expected to emerge when the plankton community is studied on monthly time scales. Rather, daily maps of PHYSAT-derived plankton groups should have been used, despite the “data gaps”. The authors might have had less data points, but the likelihood that relationships between dominant groups and DMS concentrations are completely masked in the temporal averaging process would have decreased. In bloom situations, which are expected to have been prevalent during some of the cruises in spring in the high latitudes, the species succession is rapid and DMS maxima will be temporally delayed with respect to the chlorophyll maxima. I doubt

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that any of this would be captured in this analysis. Furthermore, the problem of depth resolution is never addressed: Whereas DMS concentrations are supposedly “surface samples” (depths of measurements are not indicated in the Methods section!), the satellite sees chlorophyll in a depth integrated layer. Given that the penetration depth of the satellite is not discussed, it is not obvious to the readers that the match makes sense. Apples and pears should not be compared and the authors do not sufficiently explain why they think that the different data sets they use should be compatible.

Besides, in this manuscript it is often impossible to understand which temporal and spatial resolution was used for which part of the analysis. The authors must make more effort to convey this information to the reader.

2. Lack of exploration of ancillary/additional cruise data

Given that the DMS(P) content of marine phytoplankton can vary by several orders of magnitude, it is likely that the dominant plankton group does not account for the majority of DMS production in any given pixel of the ocean. This problem is not sufficiently discussed in the Discussion section, nor is any attempt made to address this problem using ship measurements as an independent source of information. For example, cruise data containing HPLC pigment measurements or microscopic counts could have been used to test the hypothesis “the dominant plankton group is responsible for the majority of DMS production”. Most likely, this hypothesis would have had to be rejected. In general, I feel that cruise data, which provides some ground-truthing for the PHYSAT method, is insufficiently used in the present analysis. I bet that most cruises that are cited here also measured chlorophyll-a, and several of them also estimated HPLC pigments. Some fewer might have cell counts available that could give a better insight in the plankton community structure. To rely only on satellite data (with a false detection rate of nearly 50% in the case of picophytoplankton!) to verify the hypothesis that DMS concentrations are not controlled by plankton community structure seems daring.

As a test for their hypothesis, I suggest that the authors have a look at Keller et al. 1989,

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and use the cell quota indicated there to calculate how much DMS could potentially be measured if the dominant algae were to account for it all. With a chlorophyll:carbon ratio and the DMSP:carbon ratio for each PHYSAT group you could estimate how much DMS you expect from the contribution of the dominant phytoplankton group in any pixel, were it all to originate from this group and turned over rapidly enough to show up instantaneously. The authors will find that it is almost impossible to allocate a significant fraction of DMS to their limited number of groups.

3. Temporal lag between DMS and chlorophyll

In some regions of the ocean, in particular between 40N and 40S, DMS lags chlorophyll by a few months. The summer paradox has been widely discussed in the literature during the last few years, and several modelling and experimental studies have tried to find the cause of this decoupling of DMS and chlorophyll. Hence, we know already that DMS and chlorophyll are anti-correlated or “out of phase” in large regions of the ocean – and that any measures that are a function of chlorophyll are unlikely to capture this phenomenon. The PHYSAT method, however, relies strongly on chlorophyll concentration through its use of nLw_{ref} (λ , Chl). Hence, implicitly, when you distinguish between PHYSAT groups you make this decision based on chlorophyll levels. And we know already that there is no significant relationship between DMS in chlorophyll in the stress regime. I think you should discuss this caveat in your paper, that the way you determine your groups may unavoidably lead to a poor correspondence of DMS:chl and groups.

In the stress regime, which several of the cruises used here cross (CN-169, CN-149, CN-139 etc) I would thus expect that the DMS present in the water column potentially originated from chlorophyll of a few months ago, and that it does not relate well to the in situ chlorophyll the satellite gives you for the month of DMS measurements. Hence, the authors should investigate how the DMS:chl ratio relates to groups that have been present a few months ago (lagged correlations in a wider sense), or discuss why they have chosen not to do this despite the fact that we know that DMS peaks several

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months after chlorophyll in those ocean regions

4. Role of bacterial degradation of DMS and DMSP, neglect of other sink processes

Another important point that the authors fail to discuss sufficiently in their Discussion section is the role of the sink processes for DMS accumulation patterns. The authors pick one process arbitrarily, photolysis, which they discuss fleetingly in a few sentences. However, no mention is made anywhere of the bacterial processes that lead to DMSP and DMS degradation. These processes are, in my opinion, most likely to control DMS levels. I encourage the authors to justify a) why they chose to examine only the sources here b) why they think that DMS patterns should be controlled by algae only (despite the fact that most DMS is likely to be of bacterial origin), and c) why they haven't, to give just one example, used (C)DOM estimates from space/cruise data as a proxy for bacterial biomass to verify that it's not the bacteria that counts. I know that CDOM and bacterial activity are probably poorly related, but still, an attempt should have been made to tackle the sink processes in a creative way. I repeat that the source processes are unlikely to fully account for observed DMS accumulation patterns, and this is really no news in the DMS community.

Minor comments:

Abstract:

Poor use of the English language, poorly written. Not clear what the major outcome of your paper is. Not quantitative enough. Please rewrite completely and be more concise.

L3: "Although..." - replace DMS in this sentence by fucoxanthine, to see that this sentence makes no sense. Even though DMS is an algal by-product DMS and chlorophyll do not necessarily have to have any relation whatsoever. Remove.

L4-7: "This is because..." Only partially true. Chlorophyll varies, too. Rewrite.

L6-7: "as well... than": Grammatically incorrect. Rewrite.

C1151

BGD

7, C1146–C1166, 2010

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L10: “Effect” instead of “Affect”

L10: “Meridional...” This is only a part of your analysis and does not lead to “Hence...” on L13.

L15: “as well as...”: too long and grammatically incorrect sentence, rewrite.

L20: replace “that” by “those”

L23: “This is...” What? Don’t you show the opposite? Rephrase.

L25-26: “is not consistent within” - poor English, rewrite

L27: Replace “So” by “In consequence...”

Introduction:

Badly cited. Many important citations missing. Several wrong conclusion drawn from cited articles. Need to show that you have fully understood the DMS cycle.

P3608,L10: “These average concentrations...” Average concentrations of what. In addition: No, this statement is not true. More and more models actually use prognostic DMS modules, see e.g. Kloster et al. 2007.

P3608,L12: Cite Kettle et al. 1999 and Kettle and Andreae 2000 here rather than only referring to the website.

P3608,L12-15: This is not true, either. A large part of the uncertainty is due to the gas transfer (a factor of two, actually). See the works by P. Vlahos. And it’s not the variability which is the problem, but the limited amount of data we have.

P3608, L14: Add “at present” before “Surface seawater DMS concentrations...”, what is “from space”?

P3608,L16: “If the ratio”: It is not and we know it already, cite some references.

P3608,L8-19: This paragraph lacks a lot of important citations.

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P3608,L23: “most important controls”... This sentence is very controversial. Some might say that this is bacterial degradation of DMSP and subsequent conversion to DMS, read Kiene et al. 2000 and mention this. I think we do not know what the most important controls are, as of yet.

P3608,L23-27: Why is this important here? What are your references for microzooplankton grazing and for the DMSP-lyase activity in picoplankton. Cite!

P3609,L2: “stimulates DMS production”: Please support your claim with a reference.

P3609,L6: “DMSP cell content”. Use original reference to support this statement.

P3609,L6-11: Disagree – this is not so simple!!! The difficulty lies not only in the scarcity of phytoplankton data, but to a large part in the complexity of the different production and degradation pathways of DMS and our poor understanding of the physiological role of DMS in marine algae. You need a team of several scientist to measure all parameters relevant for the DMS cycle, need to know about species composition, bacterial production, environmental conditions, growth limitations, grazing rates etcetc. Hence, phytoplankton speciation is just one small part of the problem.

P3609,L12: Replace “the main” by the number of PFTs PHYSAT is actually able to detect (5.5, actually, if you count COC as 0.5). For example, the detection of dinoflagellates is not possible, but they are definitely a main player in some coastal areas.

P3609,L19-27: This should be part of the methods section, does not belong in an Introduction, so remove or rewrite. Write here what you are going to do, why and how. The aim of the study should be clearly stated, and the structure of the paper explained. In particular, the cruise description should be improved in the Methods section, so move your description of the cruises there. Avoid referring to “some” and “any”, and replace “affect” by “effect” everywhere.

P3609,L27-P3610,L3: Remove, has nothing to do with your work.

Methods:

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The subsections here are very confusingly structured, and the data used is poorly described. No quantitative measure of error is given for the different techniques, the experimental set-up are poorly described and there are some serious issues with “forcing a calibration curve through zero”. Please improve this section drastically, as it is nearly impossible to understand what you’ve done and why.

P3610,L6: “contrastING”

P3610,L7-9: What about the other cruises? In Table 1 I count 11 cruises, and on Figure 1 I count seven. Please number your cruises and refer to the numbering throughout this publication, as it is nearly impossible to distinguish them in your current manuscript.

P3610,L10-14: “PMEL group... UCI, UCB experiments”: Which cruise are you referring to? Be specific! Give detection limits, error estimates and say which methods were used where before contrasting bottle and pump measurements. The reader does not know which is which.

P3610,L15: And this description is for which cruise? KEOPS?

P3610,L21: Now you come back to the pump effect.... a) explain the reader what a pump effect is before mentioning that “indeed” there is one. Remove “indeed”, as nobody challenged the fact that the two methods have different results. What are “pump samples”?

P3610,L3: Discuss the paper by Kiene and Slezak (2000?), showing that DMSPd might always be over-estimated in the context of your analysis. Where does filtering occur in the two methods you describe?

P3611,L1: “This...” What is “this”?

P3611,L7: Give slope, intercept, R^2 of the curve. Forcing it to go through zero is an absolute no go. Don’t force the curve to go through anything, as both methods will have an offset, resulting from different detection limits, residual DMS in pipes, different materials used to channel DMS etc. Don’t tell us the offset is zero. It’s not. In addition,

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I think a proper analysis should include an uncertainty analysis of the intercept and the slope of such a calibration curve, see e.g. Vogt et al. 2008.

P3609-P3611: Discuss the depths at which the different measurements are taken, give error estimates, clearly identify each cruise you describe, give methods for each cruise.

P3611,L22: Cite the PHYSAT method (Alvain et al. 2005,6,8).

P3611,L27: Correct formula, star is superscript

P3612,L1: “nLw”: Do you mean “nLw(λ)” here?, define nLw, λ .

P3612,L4: replace “nLw” by “nLw(λ)”

P3612,L8: move citation to the end of the sentence

P3612,L12: What is “dominant PFT monthly maps”? Rewrite.

P3612,L19: “As for...” rewrite this sentence, as it is possible, but tedious.

P3612,L12-P3613,23: This section needs to be rewritten. It is confusing, too verbose and mixes data description with validation issues. Information on the regridding methods used to get PHYSAT, chlorophyll and DMS data on one grid is missing. How were PHYSAT groups, chlorophyll and DMS matched in the spatial and temporal domain? It appears you are comparing data of a resolution of $\frac{1}{4}$ degree (ca 30km at Equator) to point data, and that chlorophyll has a 9km resolution. So you are using at least 3 different scales. Will the regridding change concentration means and ratios? How do we know that no information was lost in the temporal and spatial regridding procedure?

P3613,L1: “According...” Remove, this sentence means nothing.

P3613,L3: “a few wrong identifications”: Quantify! This is not at all true for *Synechococcus* and *Prochlorococcus* assemblages, where the false detection rate is almost 50%. And please don't call 50% wrong identifications “a few”. Summarize Alvain et al. 2008 here, give us numbers.

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P3613,L6: “major groups” - which groups?

P3613,L6: “also investigated”: Where? Cite.

P3613,L7: “good agreement”: Quantify.

P3613,L9: replace “not been” by “not yet been”

P3613,L13: “Hence...” So? What does that mean for your analysis? Which cruises are most likely to be affected by the lack of validation for the PHYSAT method? What do you conclude?

P3613,L16-18: “there could be times”... be specific! How often do you expect this to happen? Quantify!

P3613,L20: “fill the spatial gaps if necessary” How often is it necessary? In how many cases? Quantify!

P3613,L18: You mention “also” here. However, in your figures it appears that you always use the monthly climatology instead of using the daily dominance patterns. Clarify.

P3613,L15-23: I have strong concerns that the use of monthly climatological phytoplankton groups is not a good choice here. Phytoplankton succession is rapid, and phytoplankton community structure is highly variable (see e.g. Steinberg et al. 2001 for a description of the community structure at BATS). Can you show us that it would have been impossible to use daily data? Quantitatively, please?

Results:

P3613,L25-28: Be precise! How was the data extracted? Did you have to average? Which are “these datasets”, how were they selected “based on 3 criteria”??? Rewrite this section. And isn’t this part of the method section?

P3614,L1: What do you mean by “high horizontal resolution”? What is horizontal on a

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sphere – longitude/latitude?

P3614,L1-2: I don't understand criterium 3, reformulate. Why do you write this here and not in the methods section???

P3614,L5: "relatively homogeneous": Not enough information, what do you mean?

P3614,L6: What is a "physiological shift" in this context?

P3614,L3-12: I don't understand this section. What do you mean, how are these regions characterised? And how can you be sure that the DMS content of the cells is also "homogeneous". Cells could be light-stressed in this region, couldn't they? Most likely, intracellular DMSP concentrations were enhanced. If you detail the physiological conditions to explain chlorophyll levels, then you should also explain what these condition mean for DMS(P) levels.

P3614,L13-20: Rewrite, incomprehensible. What are "unfavourable situations"?

P3614,L25: Do you mean the "western North Pacific", as "North and Equatorial Pacific" is the title of this section?

P3615,L4: Replace "play also" by "also play"

P3615,L6-8: Poorly written, please reformulate

P3615,L8: "where the dominance of PRO and SYN alternates"

P3615,L10: move "In the central Pacific" after 35deg N

P3615,L12-13: "is not the same .. or..." Reformulate.

P3615,L15: define "hot spot", not sure if it is good to use this word when you are considering climatological means

P3615,L16: Omit "indeed"

P3615,L17: replace "ones" by "waters"

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P3615,L23: reformulate “is well represented”, use plural form of verb for DIA, NANO everywhere, as they are defined in the plural on page 3612

P3615,L25: “will be defined afterwards”: Indicate in which section this will happen

P3616,L1: From here on you suddenly transition from “group dominance” to “group signal”. Why have you chosen to change the terminology?

P3616,L12: “confined TO”

P3616,L13-17: This is important and the implications of the fact that coccolithophores are poorly detected should be discussed in much greater detail in the Discussion section. Given that COC are not really seen AND contain a lot of DMS, there is always a chance that the false detection of another dominant group totally biases your DMS:chl story. This should be most important in the North Atlantic. Try to associate the measured DMS using the dominant group and conversion factors from chl:carbon and DMSP to carbon and see whether or not your conclusions may be biased, in particular in the North Atlantic. Check microscopic counts and or HPLC measurements – were COC blooms detected during the cruises that you are studying? If so, consider excluding these cruises or modifying your conclusions.

P3616,L27: “some general features can be drawn” - What does this mean? Reformulate.

P3617,L2: Explain “island effects”

P3617,L7: Explain why you haven’t compared chlorophyll from SeaWiFS with ship-based chlorophyll measurements. Or why you wouldn’t use cruise data from the start. I think you could considerably improve your DMS:chl ratio estimates. Chlorophyll from SeaWiFS has an uncertainty of 30%, whereas ship-based chlorophyll should have an error of less than 10%. Assuming you get a measurement error of ca. 5% for DMS measurements, you could substantially reduce the combined error for your ratio. Perhaps your argument is that you want to use larger scale mean values for groups and

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chlorophyll. Well, still your DMS measurements remain point data if you restrict them to individual cruise data, hence you would have to use e.g. all measurements within one pixel of the NOAA database to get a larger scale DMS estimate. In addition, you're comparing depth integrated to single depth measurements. I think I discuss this problem in the general comments. In all cases, resolution is one large source of uncertainty. Please explain your choices. But don't say that it was too inconvenient to organise ship-based chlorophyll for those 8 cruises. Btw, we don't know anything about the repeatability/reproducibility of your DMS data – add this information in the Methods section.

P3617,L7: “species composition” - this should be dominance patterns I assume, as you don't have any information on species using the PHYSAT methodology.

P3617,L8: and let us know whether you use the daily, monthly, or monthly climatological PHYSAT estimates here

P3617,L22: Why is the “equatorial divergence” abbreviated with EU instead of ED?

P3617,L20: “were present” - should this be “were dominant”?

P3617,L25-P3618,L22: The description of the physical environment in this section is way too long. Please shorten, as the title of sections 3.2 is “DMS:chl ratios” and not “How to detect the equatorial convergence zone”

P3618,L9: “DMS accumulates” reformulate, you don't know this, as you have not measured source and sink rates

P3618,L18: “PRO is more typical” reformulate, unprecise. How many percent of pixels show a PRO, how many show a SYN dominance signal?

P3618,L19: Add year after “November”.

P3618,L20: “NANO was rare” - quantify, how rare?

P3618,L1-22: I cannot understand what you want to tell me when you describe the

results of Behrenfeld and Boss, 2006. An area cannot be “homogeneous with respect to phytoplankton physiology” in a general sense. All plankton groups have different nutrient and light requirements, and temperature dependences and you cannot convince me with what you write here that you checked the limitations for all groups and all limiting factors. Please reformulate the whole section.

P3618,L20-22: Cannot understand sentence, remove or reformulate.

P3618,L24: “increased but not steadily” - reformulate, what do you mean?

P3618,L26: “the steady latitudinal decrease” as above, reformulate

P3620,L1: “where” instead of “were”

P3620,L11: “northeastern sector” of what?

P3620,L13: What do you mean with “were represented in this region”? Reformulate.

P3620,L13: “COC and PHAEO signals” - please write “dominance” where you mean “dominance”

P3621,L7: “factor 25” and “factor 40”: Cannot understand what these refer to. Reformulate sentence.

P3621,L14: “species composition” - you mean “dominance patterns”?

P3621,L16+22: Describe the eddy first before discussing its DMS:chl signature

P3621,L26: “A third hydrological structure” - do you mean “a third eddy”?

P3622,L19-25: Now here you attempt some kind of a comparison between in situ and satellite chlorophyll. But instead of giving us some statistical information about the match between these two, all we have is a supplementary figure. Do a proper comparison, estimate the deviation, and do this for all cruises that you are using here. From the eye, I'd say that your ratios will be massively impacted by which chlorophyll you choose.

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P3622,L19: “is”: present or past tense ?

P3623,L26: “since the ship did not...” incorrect grammar

P3623,L26,28: “November, December”: add year

P3623,L24-P3624,L2: Very confusing, please reformulate, in particular “the amplitude of the growth” and “The comparison is not allowed....”

P3624,L16-18: Why PHYSAT groups spelled out here?

P3624, L19: In general, the results section should contain more quantitative information about the change of the DMS:chl ratio, i.e. mean, max, stdev for each group, as a function of latitude, see my general comment above.

Discussion:

This section should somehow related your measurements of DMS:chl to other measurements. For example, I think it would be very important to use the Keller et al. 1989 data and see where your calculated values lie. For many groups, a chl:carbon ratio is available, so that conversions can be made. At present, I don't have any means of relating your ratios to other work, the values lack context. You discuss some studies measuring the origin of DMS from phytoplanktonic sources, but you stay very qualitative. See general comment on page 1.

In addition, this section must address bacterial and other sink processes, see general comment on page 1. This is a major caveat of your work, that you cannot estimate the bacterial contributions with your method.

Furthermore, you must absolutely relate your results quantitatively to the error estimates for the different basins and groups, as detailed in Alvain et al. 2008. Does the uncertainty in group detection influence you interpretation of your results? How about the uncertainty in DMS, chl, the DMS:chl ratio? Do you have species composition from independent sources that you can relate your results to? HPLC pigments? Etc.

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En plus, you should have a paragraph where you discuss the caveats of your study and show what consequences they have for the outcome of your results, and their interpretation. All the issues with scale, sinks etc. must be discussed somewhere.

Last but not least, the language issues in this paper lead to very long, verbose sentence. You repeat a lot of information several times in different places, touch briefly on far too many side issues and come to the point only a few paragraphs later. Cut down all unnecessary text.

P3624,L23-27: Sentence way too long. Use a subject-verb-object structure.

P3625,L9: “according to culture work” - please cite the original sources here

P3625,L18: “The distribution...” - Style! Start with the subject, use active voice, etc etc.

P3625,L24-26: What? Above you say that you do not see COC except around Iceland. Clarify.

P3626,L7-12: Not clear, reformulate.

P3626,L8: “well-known physiological adaptation” What do you mean? Please cite original sources.

P3626,L11: “in a more systematic way than Colomb et al. (2009)” How? In which way? Why would you mention this here if you don't explain more about this?

P3626,L21: “when entering” - bad English

P3628,L4-6: “It is known that..” What relevance does this have to your work?

P3628,L9-14: What relevance does this have to your work? Photolysis is by no means the only DMS sink (~ 80% of DMS degradation goes through the microbial loop). Why do you pick this sink and not the others? Discuss sink processes in a more systematic way.

P3628,L15-17: You cannot say this, as you haven't done the appropriate measure-

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ments to prove this. Quantify and justify or omit.

P3628,L21: “in the latter case”

P3628,L29: “Assuming that...” And this I think you have shown that you cannot assume, and you should not. Rewrite.

P3629,L2-4: “Consequently.” Confusing. Firstly, I cannot see why this would follow from the previous sentence, and secondly I don’t understand “neither in absolute nor in negative”.

P3629,L17: “However, the DMS loss...” I don’t understand what you are trying to convey here. Which loss?

P3630,L12: Here we go again with “species composition”

P3630,L18: And which size spectrum does your HIAC counter cover? Which size range of organisms will it be possible to see? Which will it not see? And how do you know that the detected material accounts for the majority of all particles? Be more specific.

P3630,L19-25: Please cite the original measurements.

P3630,L23: “Marine CO2 levels...” How? Prove this.

P3630,L24: “variability..” of which ratios?

P3630,L25: ... and how does this argument with the age of the water hold for the Benguela??? Be careful, how do you know this?

P3631,L2-3: Don’t understand what you are saying. Reformulate.

P3631,L7: How common are “diatom blooms free of coccolithophores” outside the laboratory? There will always be other algae present that may also contain a little bit of DMSP?

P3631,L8: Please rewrite this paragraph. I think that the information on the meridional

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trends are very important, but this section is utterly confusing. Why refer to growth conditions, when you have actually not measured any limiting terms, half saturation constant. This section is very spongy. Stick to what you know, and cite the appropriate original publications.

P3631,L17: Which “ratios”?

P3631,L18-20: What “coherent information”? And I don’t think you can conclude this from what you show here. Show the evidence.

P3632,L1-4: Again, I think you go too far, as you haven’t measured the “physiological conditions”, and you are neglecting all DMS sinks.

Conclusion:

P3632:L23-24: “is not consistent within the SYN group..” Do you mean “SYN dominated waters”? Rewrite.

P3633,L2: “a control”, not “controller”

P3633,L4: But what about DMS measurements? It doesn’t help to only improve the PHYSAT resolution, when you have very few point data for DMS only.

P3633,L7: Effect, not affect

P3633,4-17: In general, it doesn’t help to only focus on PHYSAT here. What about the sinks of DMS? Shouldn’t we focus also on more ship based measurements of HPLC pigment data, as one example, to validate PHYSAT with and to have an independent way of matching plankton groups with DMS measurements. Add caveats of your method and discuss how they can be remedied.

Figures & Tables:

Table 1: Label your cruises here and stick to the labelling throughout the paper. I count and count, and find 11 cruises. However, in the text you mention only 8 cruises. Can

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you indicate more clearly how the different cruise legs belong together.

Figure 1: Here I count seven cruises...

Figure 2: You have panels a) – f) that are not described in your figure caption. UnitS is plural.

Figure 3: Again, describe panels a) – d)

Figure 4: As above for the labelling of the individual panels. What does “is intrinsic to the PHYSAT data treatment” mean?

Figure 5: Describe panels a) – f) in figure legend and systematically describe the individual panels. Describe all symbols on this plot. Axis labels are not clear. Must label the axis in the middle between “Western Pacific” panels and “Eastern Pacific” panels. Need to repeat abbreviations here in the figure legend. What is “T” in panel d)? Why not put curve labels in a legend, rather than having them cover some parts of the curves. It is really confusing to have both water mass acronyms and variable names on the same plot without visual separation. Labels are too small to read (dates).

Figure 6: Explain panels a) to c). Add longitude/latitude indications for “Atlantic basin” as otherwise the difference to Figure 7 is not clearly understandable. Choose good abbreviations for “Sargasso Sea” and “Benguela current” and plots these rather than spelling these terms out. Explain abbreviations in figure caption.

Figure 7: Not sure the reader needs to see salinities. Choose abbreviations for the different currents and regions, explain them in figure caption.

Figure 8: Explain panels a) to f) Now you use a and b instead of a) and b), be consistent. Label all axes, especially those in the middle between left and right panels.

Figures 5-8: Indicate which cruises contribute to these plots. Furthermore, “same as” does not apply in any of your captions, as none of the figures have exactly the same axes, i.e. sometimes you have date, sometimes longitude or latitude on the x axis, so

the plots are not the same.

Figure 10,11: Describe panels a) and b).

Figure S1: Pretty sure we don't need this figure. Especially if you force the curve fit through zero.

Figure S2-4: Not sure we need these figures. Biomass is not chlorophyll content. They are not the same, as the x-axis varies. Describe your panels labelled a) – c/f)

Figure S5: Pretty sure we don't need this figure.

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