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Biogeosciences Discussions

Interactive comment on "Sulphur compounds, methane, and phytoplankton: interactions along a north-south transit in the western Pacific Ocean" *by* C. Zindler et al.

Anonymous Referee #1

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Summary comments

This manuscript presents results from a large oceanographic survey of methylated sulfur compounds and CH_4 across the Western Pacific. The aim of these measurements is to; a) examine the spatial distribution of these compounds in a poorly sampled ocean region; b) assess the relationship between different sulfur compounds, total algal biomass (Chl a) and the abundance of particular algal taxa, and c) gather evidence linking sulfur compounds as a source of CH_4 in oxygenated surface ocean waters.

Overall, I think that the data are valuable and will contribute useful information to our understanding of global DMS/P/O distributions. In addition, there are some interesting

correlations presented, which provide potential insight into the factors driving S and CH_4 accumulation in surface waters. That being said, I had a number of concerns related to the presentation of methods used and the description of the results. I believe that both sections could be significantly improved and have provided a detailed list of suggestions below. Finally, I think the authors need to be a little more cautious in deriving strong conclusions from correlations alone.

Specific comments.

Abstract: The last sentence here is a pretty bold statement. I'm not sure that the (relatively weak) correlation of CH_4 with various S compounds justifies this conclusions. I'd suggest toning down the language here.

Methods:

In general, I think more details are needed here, as I was unsure about a number of things. For example, the S analyses (other than DMSO) were apparently run immediately, but it's not clear exactly what the analysis sequence was. I presume that the authors collected water, ran a DMS analysis, then added NaOH and ran a follow up DMSP analysis and then stored the high pH samples for subsequent laboratory DMSO analysis. Is this correct? What about filtrations? How were these conducted? Did the authors use gentle gravity filtration to address the issue of cell lysis during filtration as discussed by Kiene?

How were the calibrations done? Were these gas phase calibrations of the GC system ($e.g.$ using a permeation tube with dilution gas), or did they authors produce liquid calibration standards to calibrate the entire analytical system ($i.e.$ both purge and trap and detector components)?

What do the error terms ±represent? Std. dev, std. err? Are these mean values derived from triplicate analyses?

There is no mention of poisoning the CH_4 samples prior to storage. I presume that

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samples were indeed poisoned and the details should be given.

Can you please add a reference for the quantitative filter pad technique.

I am not familiar with the pigment analysis methods used by the authors to extract quantitative information on phytoplankton taxonomic composition. Is there a reason why they chose not to use the CHEMTAX program which has been widely used across the oceanographic community?

It's not clear how the different size classes were determined – using size fractionated filtration? I don't see how this could be done on the basis of pigment concentrations alone since there are, for example, dinoflagellates and diatoms with very different sizes.

A transformation is mentioned for non-Gaussian data, but there are no details on what that transformation actuallyis.

It seems to me that the authors should use a Type II regression since both the x and y variables are measured with error.

Results and Discussion:

p. 15019 – I'm not sure that I agree that the clusters reflect Longhurst's provinces. It seems to me that clusters 2, 3 and 4 are all present across the two main provinces sampled. I think only cluster 1 shows a distributions that is linked to one of the biogeographic provinces.

Top of p. 15020. I think it could be made a little clearer that the author's are comparing their observations with climatological predictions from Lana.

p. 15020. I'm not sure how relevant it is to compare the W. Pacific sulfur data to measurements of the E. China Sea. Would we expect the numbers to be similar? If so, why is the comparison valid?

Section 3.4 I don't think it makes that much sense to compute an overall mean for each expedition. There is very good reason to believe that different regions of the cruise

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track represent different systems, so why lump them all together. It would make a lot more sense to me to compile the data into (for example) 1 degree temperature bins. In the same section, I'd be a little cautious about over-interpreting the 'positive trend' in DMSP:DMSO with temperature at SST <5 C. This is really based only on a couple of low points. Perhaps the temperature binning approach would provide more data points to help fill in the plot.

Bottom p. 15021. I don't really follow the logic of the coccolithophore argument.

Overall, I found that the section describing various correlations was rather convoluted. It seemed that the authors took the approach of correlating everything to everything else. While this approach did yield some significant correlations, which are discussed in further detail, the significant results were, in my view, somewhat 'diluted' by the large number of correlations that were presented. Moreover, it seems to me that the authors could have included a number of other variables that could have significant explanatory power. For example, why not include Chl a in the multiple regression as opposed to running a separate analysis. Also, what about other physical variables such as surface PAR, mixed layer depth, wind-speed etc. Might it be possible to construct a more general step-wise regression attempting to product the best empirical description of S compound distributions? I think it would be ok to include some of the pair-wise correlations ($e.g.$ DMSO and DMSP) if they are used to highlight a specific significant result.

Related to the point above, I think the authors sometimes overstep their interpretation of causality based on correlative evidence. I would suggest a slight change of wording in a number of places where conclusions are drawn based on the regression results.

p. 15028, first para: The discussion is focused on DMSO and DMSP, but then seems to 'backtrack' somewhat to revisit arguments already presented for DMS. Towards the end of the paragraph, it seems that photo-oxidation could be mentioned.

Section 3.6.4.

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Interactive Comment

I think it would be useful to present %saturation values for methane in addition to concentrations.

Fig. 7. It seems to me that the regression of CH_4 vs. Chl a is heavily weighted by two points at high Chl a.

Bottom p. 15029. The idea that DMSO and DMSP serve as substrates for methanogenesis is certainly consistent with the correlations observed, but I think more direct evidence would be needed to draw the kind of firm conclusion presented in the text. I'd suggest toning down the language a bit.

Tables:

Table 1: Errors (±) are only given for some variables. Why? What do the errors represent – std. dev., std. err.?

Table 2: I found the layout of this very hard to read. In particular, the placement of letters and of individual outputs seemed rather random. The first letter to appear is 'a', then 'd', then 'i' etc. What about b,c,e,f,g, etc. Also, wouldn't it make more sense to group all of the full data set results together at the top, then group the cluster 2 results and finally the cluster 4 results?

Table 3. I would make the same comment with respect to organization of entries in the table. I think the results could be presented in a more logical arrangement. Note also that one r^2 value is missing (model l).

Figures

Fig. 1, I presume that the position of the lines on the figure correspond to the positions of the colorbars ($i.e.$ leftmost data corresponds to leftmost colorbar). This is not explicitly stated and should be. The font size seems very small to me.

Fig. 2. I found this plot very hard to read and basically useless. I think the presentation would be more effective as line plot with symbols. If necessary, you could have a series

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of sub-plots to avoid crowding the figure too much.

Fig. 3. I found details hard to see because most of the data were compressed at the bottom of the axis. Perhaps you can have a two panel figure showing the total chl data on top and the other group specific info in the bottom panel. Such a figure would likely make Fig. 2 redundant.

Fig. 4 The color scheme was a bit hard to see here. In particular, the blue color for cluster 3 could not be readily distinguished from the green of cluster 2. Perhaps the use of different symbol types would help.

Fig. 7. I think it's worth emphasizing in the figure legend that there is a second y axis for the 19-but data.

Interactive comment on Biogeosciences Discuss., 9, 15011, 2012.