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Biogeosciences Discussions

Interactive comment on "The distribution of methylated sulfur compounds, DMS and DMSP, in Canadian Subarctic and Arctic marine waters during summer, 2015" *by* Tereza Jarníková et al.

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Dear Anonymous Referee #1,

Thank you for your thoughtful and constructive criticism of our work. We have reread and revised our manuscript according to the insightful suggestions you provided, and have added some key references. The most substantial revision may consist of changing the DMS flux calculation used, and of correcting our wind speed to 10m height. This is not a problem, as the data are readily available. Please find responses to each of your points, below. We have done our best to address each statement carefully, and look forward to your responses. An updated manuscript is also available.

Best, Tereza Jarnikova PhD Candidate, UBC

Interactive comment on "The distribution of methylated sulfur compounds, DMS and DMSP, in Canadian Subarctic and Arctic marine waters during summer, 2015" by Tereza Jarníková et al. Anonymous Referee #1 Received and published: 4 October 2017 This manuscript presents DMS/P data measured in Canadian waters using two techniques, a MIMS and an automated GC-PFPD. The authors were able to use the fine resolution spatial distribution of sulfur compounds measured to examine the influence of frontal features and other small scale hydrographic changes on DMS/P. The authors provide a comprehensive introduction to DMS/P cycling and their importance in both the ocean and the atmosphere. They note that high latitude DMS emissions may be especially important for aerosol formation and polar climate. However, the number of measurements in these high latitude regions is scarce, compared to the mid- and low latitudes. The difference between findings in the Antarctic (high values of sulfur compounds) vs. the Arctic (moderate level of sulfur compounds) motivated this study and the authors are particularly interested in the role Arctic sea ice plays on regulating DMS/P distributions. This manuscript is an important contribution to the DMS/P database and should be published after the following minor changes have been made.

Specific comments: Lines 119-120: Is this Gabric reference the most updated reference on the feedback between ice albedo and DMS emissions?

>We have added a reference to a quite recent modelling study by Cameron-Smith et al, run for the Southern Ocean, that also demonstrates a remarkably strong DMS emission response to loss of sea-ice albedo. Becagli et al (2016) (http://dx.doi.org/10.1016/j.atmosenv.2016.04.002) also observed a robust correlation between DMS-sourced aerosol concentration and sea-ice melt, though we did not add this citation as the section focuses on modelling studies of future polar regions.

Lines 199-216: What is the LOD for the MIMS?

>A line has been added here - 2nM, reference Tortell 2005.

Line 211: Perm tubes are highly sensitive to constant temperature and flow conditions. How reliable are these as primary standards when taken to sea?

>Both these sensitivities are thoroughly addressed - the temperature of the perm tube is kept constant by use of a circuit-controlled heating pad, and the flow through it is kept constant via a flow gauge. We now explicitly state this in our manuscript.

Line 264: Why are your fluxes computed with N00, when more evidence is coming online that DMS k values should be linearly dependent on wind speed?

>We used this flux computation because it is consistent with the one used by the main global DMS climatology, Lana et al. However, we are aware that newer computations have been published, eg. Bell et al. It would be possible to use these.

Lines 269-270: What your wind speed corrected to 10 m height?

>This is an oversight on our part - the initial data were not corrected to 10m. However, our collaborators have provided us with this data, and we will revise this figure and the discussion as necessary.

Line 291: Do the authors mean Table 3 here instead of Table 4?

>Thank you! This has been fixed

Lines 305-306: The measured range reported is way below the LOD. The authors discuss this much later, but maybe here there should be a statement about 22% of these are below the LOD.

>For maximum clarity, a line has been added here briefly discussing this, and alluding to the more extensive discussion further down.

Line 329: Do the authors mean Figure 4 here instead of Figure 5?

>Thank you! This has been fixed

Line 379: Typo, remove of

>Thank you! This has been fixed

Lines 410s: Are there no possible scenarios in which the MIMS values are too low? E.g. peak resolution not achieved because MIMS is too slow?

>We are not aware of any reason that the MIMS should under-estimate DMS. We use a ∼30 second dwell time to ensure good peak resolution for DMS at m/z 62.

Lines 449-451: The top figure in this graph would be more instructive if we could see the comparison between this study and previous studies. The bottom figure helps with this, but does not give an idea of the spatial comparison. >The top panel in this figure does show a comparison between the current study and previous studies both in terms of concentration distribution and spatial distribution. We have changed the symbols to help clarify the presentation of different data sources.

Lines 455-rest of paragraph: Why is there no comparison to the Lana climatology here?

>The Lana climatology is based on the PMEL measurements, which we compare with directly. Any information present in the Lana climatology that is not present in the PMEL data results simply from interpolations, and thus (we feel) are not worth comparing. There is almost no data in the PMEL database in our observational region (the Canadian Arctic) - almost all PMEL measurements in the Boreal Polar Longhurst province come from the Atlantic sector. Therefore, according to Lana's methodology, the Lana climatology presents only a rough "first guess" of concentrations in the studied region.

Section 4.3: There is only one reference here (Tremblay et al., 2011) related to DMS/P and fronts. Are there no others to corroborate the authors' findings?

>Here we are focusing on the idea that frontal zones may be regions of enhanced productivity, and we then argue that this may lead to increased DMS production. This idea is well-established in the literature - eg Lutjeharms 1985 and others. We chose the Tremblay reference because it is from a similar region from a similar time of year.

Lines 533-535: There are no obvious trends in the data between MLD and sulfur compound concentrations. I am not sure that the following explanation is justified by the data.

>We have changed the wording here somewhat to reflect the lack of an overall statistical trend.

Line 537: There appears to be something wrong with the numbers here. The shallowest MLD is 2.1 m in Table 2.

>This was a typo; I have removed this sentence.

Lines 552-563: Are there no possible other explanations beside PFTS? Was there more bacterial activity? Or more cell lysis?

>We agree that these factors are important, though it is not possible for us to estimate them from our observational data. We have added a comment about these factors in this section.

Lines 565-566: Are there no citations for this sentence? Is this considered common knowledge?

>We now cite a review paper published by Levasseur in 2013, which gives a good overview of the relevant literature.

Line 576: Typo, extra space between study and comma

>Thank you, this has been fixed.

Line 585: What is 30a? Is this a citation typo?

>This has been corrected - this is a reference to the Galindo 2014 paper.

Lines 590-592: In Table 2, I can see the highest sulfur:chl for stations BB2 and CAA7 for DMS. BB3 and CAA6 are for DMSP only.

>We have changed this sentence to reflect DMSP only. In this way, the section makes

a better link between DMSP:chl ratios and sea ice cover.

Figure 1: Caption – GD should be GL

>Corrected – thank you.

Figure 2: No description of red dots.

>Corrected – thank you.

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