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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 #2,

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 a reworking (and slight shortening) of our Discussion section in response to your points - we aim to be more cohesive and clear when placing our work in the context of other studies performed in the Arctic. 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 #2 Received and published: 15 October 2017 General comments. The study reports high spatial resolution measurements along a cruise track that passes through a number of distinct regions around the western Arctic. This is interesting on two counts, one is the high spatial resolution of the data that illustrates spatial gradients generally not observed using other approaches, and the second is the contribution to the comparatively few measurements of DMS/P that have been carried out in the Arctic in general and particularly in this region. These high resolution seawater measurements of DMS and DMSP are generated using MIMS and an OSSCAR system that is probably unique to this group and the two systems have seldom been applied simultaneously (e.g. Asher et al. 2015). This is an important data set and may well be useful to those trying to model DMS emissions in Arctic waters and the role that DMS may play in aerosol and cloud formation over the Arctic. Despite the uniqueness and quality of this data, in general, I think the authors fail to make full use of the high spatial resolution data and supporting information. For instance, much of the manuscript, including 3 tables, is dedicated to trying to identify the phytoplankton sources of DMSP and DMS from a limited dataset (9 stations along a 10,000 km transect) of pigment concentrations. It would be much more informative in my view, to concentrate on the unique high resolution data over the very long transect; especially what may be causing the large gradients neatly illustrated in Figure 4 and whether there are areas of particularly high or low DMS sea to air flux. I think the Discussion in particular needs to be more focused on the results from this dataset and what they might mean to DMS emissions in the regions.

Specific Comments Abstract. This could be tightened-up so that it really represents the finding in the main manuscript. At the moment it does little to convey the real relevance of the project.

>We feel that the abstract quite clearly conveys the main results of the paper. However, we have revised it to better reflect the broader significance of the work.

L21-22. What does the conclusion that a range in concentrations of DMS (âĹij1 nM to 18nM) and DMSP concentrations (âĹij1 nM to150 nM) was consistent with previous observations in the Arctic Ocean really mean? This would apply to almost any large stretch of ocean wouldn't it?

>Thank you, we agree and have removed this part of the sentence.

L23. The comment about Baffin Bay is interesting but I do not see a focus on it in the actual manuscript, maybe there should be?

>We agree that the Baffin Bay DMS concentrations are interesting, and wish to highlight them. We discuss the sharp increase in DMS Baffin Bay concentration from lines 347-357, and contrast them with the rest of the transect.

Introduction. L41. The uncertainty in the CLAW hypothesis should also be made clear.

>We have added a reference to the Quinn and Bates paper, stating that this mechanism remains the subject of debate.

L48. Stefels et al. 2007 is now 10 years old, it might be worth considering whether more recent studies have thrown new light on the topics?

>Though we agree the Stefels paper is older, it remains one of the most comprehensive reviews of the DMS cycle, which makes it very suitable for a general reference such as the one called for here. We cite more recent papers when discussing specific aspects of our findings.

L60. I don't think Zubkov et al. 2001 directly addresses stimulation of DMS production by grazing or viral lysis.

>We switched this reference to Evans 2007, who directly compares the relative significance of grazing and viral lysis in DMS production in an E. huxleyi culture study.

L68. Several modelling studies also suggest a limited role for DMS in cloud formation in the Arctic and should be mentioned (e.g. Carslaw et al 2012, Browse et al 2014).

>We have added a line stating this, and have cited the Browse 2014 study, as well as citing Carslaw's 2013 paper that highlights the importance of quantifying uncertainty natural aerosol contribution to climate forcing.

L80 It is not clear what the relevance of this comparison between Arctic and Antarctic measured DMS values is, both datasets are regionally and seasonally biased making it difficult to conclude anything from the comparison of the full datasets.

>We respectfully disagree - we feel that it is important to point out the different controls on DMS production in the two polar regions, which otherwise share a number of physical characteristics, includingseasonally varying sea ice cover and insolation. The difference between these two polar regions is critical to understanding potential drivers of DMS cycling in these regions, and this provides context for our work in the Arctic.

L89-102. The relevance of the comparison of Arctic and Antarctic DMS concentrations and controls on that production is not clear at this point. This is not a component of either the Results or Discussion. Maybe this comparison would be more interesting and relevant as an aspect of the Discussion?

>See above - we believe that a brief discussion of the different DMS dynamics of the two polar regions provides context for our work in the Arctic. It is a fact that there has been a hugely disproportionate effort towards understanding DMS/P dynamics in south polar regions. The relative lack of data from the northern polar regions is a main motivating factor for our work.

Methods L159+ It would be useful to know why the data from the OSSCAR system does not cover the full transect, maybe I have missed that in the manuscript?

>We experienced technical difficulties with the OSSCAR system in the early part of the expedition. This is now explicitly mentioned in the manuscript.

L181. It would be useful to include the concentration of the point standard as this would provide context for the standard error of 0.55 nM that is deemed the level of precision. Was this not concentration dependent?

>We agree - this inline standard was 20 nM, and this has been noted in the text.

L266. Flux estimates: possibly understandably the authors use a fairly simplistic parameterization to compute DMS exchange rates, but it should be noted that the Nightingale 2000 parameterization has now consistently been shown to overestimate flux at higher wind speeds. At some point we as a community are going to have to start using a more realistic parameterization. Plus the scaling exponent (0.4) derived from Loose et al. 2009, requires more explanation. Does this account for flux through the ice or for enhanced exchange due to turbulence generated by the ice etc.? A short section, possibly in the Discussion, is required to make this uncertainty clear.

>We agree - this point about the Nightingale parameterization has been brought up by Reviewer 1 as well. We used it to be consistent with Lana et all, but are open to changing the parameterization (to Bell et al or another) as well. We will also add a comment on the Loose at al scaling exponent in the relevant section.

L269. Was the wind corrected to U10 as is generally used in the Nightingale 2000 parameterization and was it corrected for ship speed?

>This was an oversight on our part - the original ship data we used was not corrected to U10, but we have obtained U10 data from collaborators and will use it in the revised manuscript.

Results L364. It would be useful to have an indication of what distance the subjective 100 points refers to over which the gradients are calculated.

>In our dataset, a radius of 100 points corresponds to a mean radius of around 25 km;

this has been clarified in the revised text.

Figures. In general, the figure legends could be made more informative.

>We have added additional details to the legends for figures 1,2,5, and 6, but it wasn't clear what the reviewer was exactly looking for.

Discussion. L406. Could this be caused by carryover of NaOH from DMSP analysis to DMS analysis? High concentrations of NaOH are difficult to wash off with only MQ water, was this tested with DMSP standards at all, i.e. purging of a DMSP standard following a DMSP analysis with NaOH addition.

>Prior to the cruise, we did test the thoroughness of our rinse cycle by testing DMSP standards with no NaOH added, following a DMSP analysis with NaOH addition, and these blanks were clean – a note about this has been added to the text. Nevertheless we want to state this possibility.

L451. This comparison of DMS flux does not 'prove' anything really without access to the modeled information.

>We agree and have removed this sentence.

L462. Do you mean 'sulfur accumulation', or what does sulfur accumulation mean?

>The statement 'DMS and DMSP concentrations' is more exact here. We have rephrased this.

L477. It's not clear what point is being made here, I think the logic may be reversed?

>The purpose of this section is to discuss seasonal patterns in phytoplankton biomass, productivity and DMS/P concentrations, as observed by fellow researchers in the region. It serves to provide a context for the results we obtained in our study. We are not clear which logic the reviewer sees as reversed.

L481. Again, it is not clear what point is being made in this paragraph and whether it is necessary?

>See above comment.

L492. The Simo and Pedros-Alio (1999) study was based on experiments in the North Atlantic, so also not restricted to lower latitudes, and many studies have confirmed the 'summer paradox' seasonal pattern extends beyond low latitude waters.

>We have rephrased this to reflect observations beyond the low latitudes.

L497. I think this section would be made more interesting if it was considered in terms of trying to develop a seasonal model that included DMS emissions. Is there sufficient information (Table 5 and this study) to start to develop such a model? If not, what is needed; seasonal studies in different regions of the Arctic or more transects throughout the year?

We believe that a seasonal model of DMS in the Arctic is being developed by our colleagues at University of Victoria, (Dr. Steiner's group). We have not focused on model construction in the Discussion as we feel that it is beyond the scope of the paper, but a comparison of seasonal studies in different regions of the Arctic would certainly be helpful in model construction.

L502.' decorrelation length scales' needs more explanation, especially as the authors then go on to point out co-occurring gradients.

>Decorrelation length scales provide information on the spatial scale of processes driving the majority of variability in DMS concentrations. We have added an explanatory line to the text. .

L510. I'm not sure whether the argument is consistent - why should high primary production drive increased DMS - as pointed out earlier in the text, high nutrient periods on a seasonal basis are associated with low DMS.

>We can appreciate the apparent contradiction with our earlier discussion of seasonal

changes, and have added an additional sentence to clarify this.

L523. I'm not sure where this paragraph goes, other than to highlight a different study by these authors? Are there regions identified in this region of the Arctic where understanding the processes would be particularly useful and how might that be achieved?

>We agree – as we are not conducting isotope-based experiments in this research, we have removed this paragraph.

L532/Section 4. This section starts off discussing areas of shallow mixed layer depth then merges into a discussion comparing DMSP:chl a ratios generally in the Arctic. What point(s) is being conveyed?

>Here, we aim to characterize the general structure of our observations - to state that we observed elevated DMSP:chl in shallow MLD regions with a mixed assemblage, under potential light stress. We also want to compare these observations with others made in the region, for context.

L552. Again, it is unclear what point is being made here - the range in DMSP:chl is > 3-fold, (52 - 182), why does the final sentence conclude low variability in DMSP:chl?

>We have rephrased this section to better reflect that we cannot draw conclusions on the role of taxonomy in controlling DMSP:Chl a ratios - though the variability in DMSP:chl is relatively high, the variability in taxonomy appears low.

L564. This section could be usefully focused. It would be more relevant to focus on what this dataset shows. The paragraph from L565 simply reviews previous studies and seems superfluous. As it stands, it is unclear what the authors conclude. The correlation between sea ice cover and DMS/P is negative but station-specific data suggests enhanced DMSP:chl ratios near the ice edge?

>We have re-written this paragraph to focus the ideas presented. We start with a brief overview of some previous results examining potential ice effects on DMS/P cycling, and use this to provide a context for our work. We have significantly shortened the

paragraph to focus on the most important messages.

L558. Does the MIMS data pick-up ice edge effects on DMS concentration, i.e. is DMS related to salinity when passing through marginal ice zones or ice edges, over shorter distances than the whole dataset?

>We have now clarified that the ice-edge effects on DMS were observed using MIMS.

L605 Again this section reads very much like a review, with little focus on what this particular study demonstrates.

>We agree and have removed this short section, as it does not speak to our specific results.

Minor points

L144 - use CAA instead of Canadian. . .

>Thank you, this has been corrected.

L196 –should read 'convert DMSO to DMS'

>Thank you, this has been corrected.

L266, more correctly A is proportion of sea ice cover, rather than percentage.

>Thank you, this has been corrected.

Figure 1. GL - Greenland not GD; and please define CAA

>Thank you, this has been corrected.

Figure 2.please explain what the red dots are on the DMS graph, possibly station mesurements? This would strengthen the suggestion made on L595+

>I have added the note that these are station measurements.

Figure 3. please explain what the red dots are?

>I have added the note that these are station measurements.

Figure 6. Some more information in the legend would be useful, for instance, how are the data compiled, what exactly is illustrated? The total number of points for each dataset would also be useful.

>We have updated this figure legend to reference the data sources and the total number of points.

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