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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.

Anonymous Referee #2

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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.

L21-22. What does the conclusion that a range in concentrations of DMS (∼1 nM to 18nM) and DMSP concentrations (∼1 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?

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?

Introduction.

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

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?

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

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).

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.

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?

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?

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?

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.

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

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.

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

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.

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

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

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

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

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.

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?

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

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.

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?

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?

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?

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?

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?

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

Minor points

L144 - use CAA instead of Canadian. . .

L196 –should read 'convert DMSO to DMS'

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

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

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

Figure 3. please explain what the red dots are?

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.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-337/bg-2017-337-RC2-supplement.pdf

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Discussion paper