

## ***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

[Printer-friendly version](#)

[Discussion paper](#)



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?

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

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

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?

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

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

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.

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

Line 379: Typo, remove of

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?

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.

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

[Printer-friendly version](#)

[Discussion paper](#)



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?

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.

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

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

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

Line 576: Typo, extra space between study and comma

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

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.

Figure 1: Caption – GD should be GL

Figure 2: No description of red dots.

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-337>, 2017.

Printer-friendly version

Discussion paper

