

## ***Interactive comment on “Vertical profile of atmospheric dimethyl sulfide in the Arctic Spring and Summer” by Roghayeh Ghahremaninezhad et al.***

### **Anonymous Referee #3**

Received and published: 9 March 2017

This is an interesting study of DMS profiles obtained at different times of the year and at different locations. The introduction is a nicely written summary of Arctic DMS emissions, chemistry, and potential impacts on climate, etc.

The overall methodology is probably good but the description of the DMS measurements is confusing and would benefit from rewriting and reordering some sentences. Why did the sample collection times vary so much? Is there a reason for this? It seems like different collection times will result in different amounts of sample collected resulting in different limits of detection. Please comment on this and clarify. The paragraph beginning on page 5 line 23 is particularly confusing. It seems that this paragraph was intended to describe the calibration methodology but this is not obvious. It is stated

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that “Three Tenax tubes were injected with standard DMS along with one blank Tenax tube for each test period. . .”, Why? What is the meaning of this? This is followed by a statement about calibrating the GC-SCD with 1 and 50 ppmv gas DMS standards. Where did the laboratory get these standards. Were they certified standards etc. Collection and analysis were referenced to Sharma and Rempillo after the collection was already briefly described prior to this statement. It is stated that the uncertainty is  $\pm 12\%$  with this method but is that somehow independent of the amount of sample collected AND the mixing ratio of the sample that was collected? Please clarify and add a brief description of the Sharma and Rempillo methods and how the 12% uncertainty is determined.

The Tenax storage test shown in Figure 3 needs further discussion. The authors prepared a 1 pptv sample which is impressive. Would like details on how they did that. It is not clear what the standard deviation for each test represents. How many times were the samples analyzed etc.? And does the test have meaning given the uncertainty in the measurements? What is the LOD of the measurements?

DMS measurements and discussion – the decline in DMS mixing ratios with height in July is essentially what is expected and the pattern been seen in a number of previous studies. As stated, it results from primarily from fast photochemical destruction in the absence of deep convection as the lifetime of DMS is fairly short in July ( $\tau \approx 1$  day). The data points above the surface (1 and 3 km) could be interesting but it would be instructive to know/understand the confidence that the authors have in these measurements with respect to LODs etc. Also related to that, I agree with other reviewers that the authors should include more discussion of the vertical structure of the atmosphere in section 3.1 It is important to know if there evidence of atmospheric stability or of convection and mixing into the free troposphere.

The April results are definitely quite interesting. I am surprised at both the surface measurement and aloft mixing ratios since this time of year is early for substantial biological productivity I would think. The authors didn't mention previous ob-

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servations from the NASA DC-8 during ARCTAS (<https://www-air.larc.nasa.gov/cgi-bin/ArcView/arctas>). The results in paper contrast with the ARCTAS data in spring where lower DMS mixing ratios were observed (below detection limit to a few pptv and a max of 1 pptv in the free troposphere). It would be interesting to describe leads observed etc. in the region of sampling.

I am curious about the DMS emission source inventories used in the model and where these came from during springtime.

In summary, the paper presents some interesting data. I am a little concerned about drawing too firm conclusions from a relatively sparse data set. But if the authors can give further evidence of the robustness of their technique including limits of detection and a better description of blanks and uncertainties etc., providing a firm defense of their data, then the conclusions and/or hypotheses given in the paper can be given more weight and in that case the results should be published because the observations, if they hold, up are of interest to the community and relevant to atmospheric chemistry with possible climate implications.

Minor things:

P3 line 8 – describe CLAW hypothesis

P4 line 13 add s to altitudes to make it plural

P4 line 20 – suggest replacing “act” with “appear”

P5: As suggested above rewrite paragraph beginning on line 17

P8 line 25 replace “higher present” with “a higher presence”

P8 line 26 – make Cloud plural – “Clouds”

P10 line 5 eliminate comma after mixing ratios

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