

## ***Interactive comment on* “Summertime sources of dimethyl sulfide in the Canadian Arctic Archipelago and Baffin Bay” by E. L. Mungall et al.**

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Received and published: 19 January 2016

This paper describes a recent set of DMS measurements (mostly atmospheric, some seawater) from a cruise in the Canadian Arctic. The authors used trajectory analysis and GEOS-Chem model to predict the atmospheric DMS mixing ratios, which were compared to shipboard measurements. Differences between the model and measurements were then attributed to oceanic as well as non-marine DMS sources, such as terrestrial plants, tundra, and melt ponds.

The authors were thorough in examining all the possible DMS sources. The use of trajectory/chemical transport modeling for this analysis is appropriate. However, the “end member” emission rates from most of these sources (e.g. melt ponds, biomass

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burning) are highly uncertain – often times only a single emission value from literature is available. Such large uncertainties mean that the work is less about “attribution” and more a case of sensitivity study. Given the many possible DMS sources, the degree of freedom is high.

My biggest concern of this paper lies in the fact that there were so few seawater DMS measurements, which are needed to compute the DMS flux along the cruise track and form the principle input parameters for the GEOS-Chem model. A significantly biased seawater DMS field would render most of the analysis on model-measurement comparison in atmospheric DMS moot. Furthermore, the paper seems to implicitly assume that the model gets all the atmospheric transport and the DMS sinks right. The comment “wind speeds in our GEOS-Chem simulations are generally within a factor of 2 of the observed wind speeds along the ship track time series” is worrying. A factor of 2 error in wind speeds means approximately a factor of 2 error in DMS flux from the ocean (since the kDMS vs U relationship is largely linear). Could it be that the measurements and model predictions (with oceanic DMS only) already agree within the uncertainties?

A comparison of another independent variable might be helpful as verification for the model. For example, the paper shows sea salt, MEK, and CO from GEOS-Chem. Were there other tracers measured on the cruise that could be used to indicate biomass burning (e.g. potassium, acetonitrile), terrestrial biogenic emissions (e.g. methanol, acetone, isoprene, terpenes), pollution (CO, black carbon) etc? The Tof-CIMS with benzene source should be able to detect compounds such as acetone, isoprene, terpenes.

Specific comments:

p. 35549. Line 7. DMS emits from the surface ocean to the atmosphere not really because it is “relatively insoluble”, but because there’s a large air-sea concentration gradient in DMS.

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Line 15-19 suggest rewrite and add references. For example “more hygroscopic” instead of “more water soluble”

Line 22-23. Need reference

p. 35553. Last line. State the stability/variability of the one-point calibrations. For future deployments, the use of an internal isotopic standard is highly recommended (e.g. see <http://www.atmos-meas-tech.net/3/1/2010/> for DMS and <http://www.atmos-chem-phys.net/14/7499/2014/> for methanol and acetone)

Also, the authors may want to consider the use of gold beads to blank for DMS or the use of Pt/Pd catalyst to blank for organics in general. It's advantageous to use these methods to blank instead of zero air because doing so doesn't significantly change the water vapor content in the sampled air.

p. 35554. It's highly unlikely that the moving ship itself will cause enhanced DMS mixing ratio at the foremast. The vast majority of atmospheric DMS just above water level and immediately in front of the ship will blow past the ship before even reaching the sampling height of 16 m.

p. 35556. A description of the seawater DMS measurement system?

There is a mistake in Eq. 2. KH should be associated with the  $k_a$  term in this formulation.

p. 35558. Bottom of page. Air-sea transfer of DMS is primarily (>~90%) waterside controlled, so the choice of the airside transfer velocity ( $k_a$ ) shouldn't make much difference to the predicted sea-to-air flux. If you want to test the sensitivity to  $k_a$ , you should include the most recent, and one of the only field measurement based parameterizations from Yang et al 2013 ([www.pnas.org/cgi/doi/10.1073/pnas.1317840110](http://www.pnas.org/cgi/doi/10.1073/pnas.1317840110)).

p. 35559, line 18. I assume the waterside transfer velocities have been adjusted to the ambient seawater Schmidt number of DMS?

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p. 35560, p. 10. Another high latitude cruise with DMS measurements is Yang et al. 2011 (<http://onlinelibrary.wiley.com/doi/10.1029/2010JC006526/abstract>) from near the South Georgia Islands.

p. 35561, line 12. The Liss and Merlivat 1986 parameterization consists of three piecewise linear terms, rather than a single linear wind speed dependence.

p. 35564. Line 22, east of Baffin Bay

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35547, 2015.

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