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Discussions

Interactive comment on "Underway seawater and atmospheric measurements of volatile organic compounds in the Southern Ocean" *by* Charel Wohl et al.

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This is a valuable study and generally well-written. I have a few minor usage corrections which I'll list at the end. Here are some general comments for the authors to consider.

I wonder about sampling artifacts in the atmospheric measurements using the long inlet, described only as 90 meter, 9.5mm OD teflon tube. This inlet is probably OK for DMS but may contribute measurement bias for the more soluble species. Can the authors cite evidence to address this concern? Diverting sample flow to a Pt-catalyst combustion furnace provides an instrument blank for the PTRMS, but the authors don't mention doing a zero-air or standard injection at the inlet tip to characterize sampling

artifacts from, for example, marine aerosols accumulating inside the tubing. I'll note the inlet used by Kim (2016,2017) was much shorter, sheathed and heated for its entire length, and used impactors to limit aerosol contamination. The inlet for the Yang 2014 AMT cruise was 25m and shielded from light to prevent photochemical conversion.

Are the atmospheric concentration units (nmol/L) in Table 1 correct? Elsewhere the air values are quoted as ppbv (nmol/mol, nL/L). Many values reported in the text would fall well below the DL if nmol/L values in Table 1 are converted to ppbv. If air units on Table 1 are in fact ppbv, then project mean concentrations for all species but DMS appear to be right at the DL. I'll assume this is the case, but the authors should clarify.

There are many places in the text where the authors state their measurements 'compare well' with prior studies, but specific values from the literature are not always given. A comparison with published studies in the Southern Ocean (SO) and other regions is important but would be easier to digest if this information were removed from the various results sections, organized, and presented in Table format. A discussion of the these should be provided in a Discussion Section following the Results.

We know more about DMS than the other species in this study, and the surface ocean is unambiguously a source of DMS to the atmosphere over all seasons. Assuming the seawater concentrations and estimated fluxes observed in low-productivity areas are generally representative of fall/winter conditions over the entire SO, and the mean values from the entire cruise are typical of summer, it would be interesting to compute the estimated annual DMS emission over the entire SO region and compare this with prior estimates. Do we think the results from this cruise are representative of the SO in general? Have we now reached a reasonable consensus on annual DMS emissions from the SO?

This project is valuable because the SO is a unique marine environment, isolated from anthropogenic and continental emission sources. The cruise covered a broad swath of the SO, encountered a range of conditions relating to primary productivity, and con-

ducted the first survey of air/sea concentrations for methanol, acetone and acetaldehyde. Readers will inevitably speculate on the broader geochemical significance of the results, so it seems to me the authors could strengthen their concluding remarks and provide their own perspective, suggesting hypotheses that emerge from this study.

For example, from what I've gathered in my brief reading: 1. The SO is supersaturated with isoprene, even in low productivity areas, implying a continuous source to the marine atmosphere, perhaps over all seasons. 2. Methanol, acetone (and acetaldehyde?) are undersaturated in the surface ocean except during episodic cases of enhanced productivity, and it is therefore likely the undersaturated condition persists throughout the fall/winter seasons when productivity is low. Thus, the SO represents a sink in the global atmospheric budgets for methanol and acetone. 3. The observed relationships to fCO2 provide a crude way to estimate localized emissions of these gas species and their impact on atmospheric oxidative capacity and aerosol production/growth. We hypothesize these atmospheric impacts are restricted to upwelling regions of high productivity.

Are these appropriate? I'd like to hear the author's thoughts.

Minor comments:

line 32: 'Dimethyl sulfide is a key source of secondary organic aerosol' - suggest you omit 'organic' since the major contribution is from inorganic sulfate, although MSA is also produced.

line 39: I wouldn't call PAN a 'pollutant' since it's a natural component of photochemical cycles in the background (unpolluted) atmosphere.

line 228: do you mean 'H is the dimensionless liquid over gas' form of H?

Finally, it seems like the Appendix and related plots on solubility could be moved to the supplemental material.

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