Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-615

Anonymous Referee #1

Referee comment on "Marine gas-phase sulfur emissions during an induced phytoplankton bloom" by Delaney B. Kilgour et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-615-RC1, 2021

Review of acp_2021_615

Marine gas-phase sulfur emissions during an induced phytoplankton bloom

by Kilgour et al.

This manuscript describes CI-TOF-MS measurements of gaseous sulfur compounds emitted during a controlled, induced bloom experiment. The authors sought to determine if additional sulfur compounds, besides DMS, are emitted to the atmosphere, as well as the relative amounts. This is important for aerosol and cloud formation in the marine atmosphere. The manuscript reads well and the results are robust. The manuscript should be published after the following comments have been addressed.

Specific comments:

Lines 95-96: Missing current Lennartz references. For example (this alone might be good enough to reference):
Lennartz, S. T., Marandino, C. A., von Hobe, M., Andreae, M. O., Aranami, K., Atlas, E., Berkelhammer, M., Bingemer, H., Booge, D., Cutter, G., Cortes, P., Kremser, S., Law, C. S., Marriner, A., Simó, R., Quack, B., Uher, G., Xie, H., and Xu, X.: Marine carbonyl sulfide (OCS) and carbon disulfide (CS_2): a compilation of measurements in seawater and the marine boundary layer, Earth Syst. Sci. Data, 12, 591–609, https://doi.org/10.5194/essd-12-591-2020, 2020.

Section 2.3: is there a reason why the authors chose not to use an equilibrator?

Figure 1b: are the individual peak shapes determined by injection of individual standards?

Line 191: how were LODs determined?

Line 201: why does MeSH have a water vapor dependence, but DMS does not? Have the authors considered an isotope dilution method to solve this sensitivity problem during the measurement?

Lines 212-220: Did the authors try using Teflon fittings? And Sulfinert® coated metals?

Line 284: I am not sure why this should be surprising if you don't have oxidation products – even if MSAM is outgassing, if no DMSO2 is formed then why would the authors expect a correlation?

Lines 304-306: this is only true if the atmospheric loss is the same or negligible. Is it true here that it is assumed to be negligible?

Line 363: why would the authors assume the lifetimes to be the same?

Lines 366-370: how does this explanation account for bacterial cleavage?

Line 426: when I look at Figure 5, it does not seem like bacterial sulfur demand is the most important determinant.

Supplemental material, paragraph starting at line 109: since this correlation looks so good, I wonder if there is anything with which salinity correlates that could be interesting for the ratio (but was not tested against the ratio).