Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-856-RC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Methanethiol, dimethyl sulfide and acetone over biologically productive waters in the SW Pacific Ocean" *by* Sarah J. Lawson et al.

Anonymous Referee #2

Received and published: 12 December 2019

Review "Methanethiol, dimethyl sulfide and acetone over biologically productive waters in the SW Pacific Ocean" by Lawson et al.

The study by Lawson et al. reports atmospheric measurements of three trace gases relevant for atmospheric chemistry from a heavily undersampled region, and relates them to environmental parameters. A major finding is the varying but significant efflux of methanethiol to the atmosphere, indicating an oceanic source larger than previously thought. In addition, the paper contributes an intercalibration of DMS measurements, which is important for data comparability and helps to improve quality of DMS data compilation in larger datasets. Hence, the data reported is very valuable. The study

Printer-friendly version

Discussion paper

is well presented, timely and great care is given to details of how measurements were obtained and how the results were derived from the data. The study is very well suited for publication in Atmospheric Chemistry and Physics after some minor issues have been addressed:

General comments:

- The study remains rather descriptive. Did you have a clear hypothesis for the atmospheric concentration of these gases in the three regimes, e.g. before and during the blooms, and related to the bloom dominating species? If yes, you could state this more clearly in the introduction, and refer to it in the conclusion. Maybe you could emphasize the gap of knowledge that your study addresses more clearly at the end of the introduction.

- On page 11, l. 13 you state that an inherent assumption of the nocturnal accumulation method is a well mixed boundary layer, but in section 3.1 you state that part of the differences between measurement systems for DMS comes from the different intake heights. Isn't that a contradiction? Is it possible to take the information of concentration difference of the two inlets (i.e. concentration gradient) into account for the nocturnal accumulation method?

Specific comments:

Abstract:

p.1, l. 21: can you say "local time" after 16:00 hrs or say how many hours after local noon in order to make it more clear?

Introduction:

p. 2, l. 11: Could you provide a reference for the 17%?

Could you add a short sentence to the last paragraph about what was the aim of the study and what was the motivation to conduct this study in this area?

Methods:

p. 5, l. 23: Could you state whether you expect losses for acetone and MeSH, or why not (since they were not tested)? I am wondering if there are other studies showing the stability, which would support your results.

Results and discussion:

p. 8, l. 9ff you state that prior to day 47, the difference between the two measurement systems comes from calibration or undefined other differences. Can you further specify these differences? Or could it be that there are large uncertainties in the Smith equation, given that the trend in the differences between the two inlets decreased over time, but the absolute magnitude of the Smith-correction is not enough (but maybe carries uncertainties that cover the remaining differences?).

p. 10, l. 38ff: Could you discuss the physical control of the atmospheric concentration, e.g. would a breakdown of the boundary layer and an intrusion of free tropospheric air carrying less DMS/MeSH/acetone influence your measured concentration and your diurnal cycle as well?

p. 10, l 39ff: The finding of the differences in diel cycles between DMS and MeSH is interesting, since you state that both of them are removed by oxidation with OH. Do you attribute the remaining differences (e.g. increase in concentration during early morning for DMS but not for MeSH) to different production pathways, or to other additional sinks or physical processes that differ between those gases?

p. 12, section 3.4: Often you report potential explanations for your correlations, but you do not discuss them in detail (e.g. l. 22ff, l. 34ff). Could you be more specific here or derive further hypothesis/what needs to be tested specifically to prove whether this suggestion is likely/unlikely?

Implications and conclusion

My comments to this section is mainly reflected in the first general comment. Can

you use your data or the ratio of MeSH and DMS to derive a hypothesis under which environmental conditions the reaction pathway from DMSP favours DMS or MeSH formation? Or wouldn't that be reflected in the atmospheric data?

Figures

I think figure 6 would benefit from combining all diel cycles in one yy-axis plot (DMS and acetone on one axis and MeSH on the other) – so that one can compare the diel cycles together, as this is an aim of the study (p. 4, l. 6).

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-856, 2019.

Printer-friendly version

Discussion paper