

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

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General comments: The study by Sarah J. Lawson et al. “Methanethiol, dimethyl sulfide and acetone over biologically productive waters in the SW Pacific Ocean” is original, interesting, well written and an important contribution to the current knowledge of marine trace gases. The authors present an high resolution dataset in an undersampled remote marine region and explain the variation of the trace gases in the context of biogeochemical parameters. Furthermore, they present an intercomparison of DMS measurements performed with three different independent analytical systems. I recommend to publish the paper with only a few minor corrections: Abstract Page 1, Line

C1

21: You talked about “A positive flux of MeSH. . .”. Can you write instead of positive or negative flux the direction of the flux? Into or out of the ocean.

Results and Discussion Page7 Line34: You mention “atmospheric stability”. Can you explain shortly what do you mean with atmospheric stability in the text? Page 9 Line 2: You give the ratio between MeSH and DMS. Can you also present the average? Page 9 Line10: You say:”MeSH levels during B1 were substantially higher than. . .” Can give an actual number how high the level was. The reader will then be able to compare it with the literature value you present in this sentence.

Table 4: Can you present errors of the ppt values? In the figure caption please say: “nocturnal buildup method (NBL)” and say what EC stands for.

A general question: Why do you had different intake heights for you CIMS and your PTR-MS during the cruise? Isn't it esier to have the inlet of both instruments at the same location for comparison? Can you discuss this in the text?

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

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