Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-891-RC1, 2021


# Comment on acp-2021-891

Anonymous Referee #1

Referee comment on "Oceanic emissions of dimethyl sulfide and methanethiol and their contribution to sulfur dioxide production in the marine atmosphere" by Gordon A. Novak et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-891-RC1, 2021

Interactive comment on 'Oceanic emissions of dimethyl sulfide and methanethiol and their contribution to sulfur dioxide production in the marine atmosphere' by Novak et al.

This paper report simultaneous flux measurement of DMS ($CH_3SCH_3$) and MeSH ($CH_3SH$), which are important sulfur compounds as oceanic sulfur emission and subsequent aerosol formation and climatic effect, from a coastal ocean site. The result shows a linear relationship between DMS and MeSH over the campaign, and this suggests that both have the same source (i.e. oceanic emission). Besides, another important finding is that no other volatile sulfur compounds like DMSO, DMSO2, MSAM, and DMDS were below the detection limit.

The authors then investigated how the MeSH emission is important for the budget of sulfur dioxide ($SO_2$) in the marine environment. Based on a chemical box model implemented with newly compiled MeSH oxidation mechanisms, 30% of total $SO_2$ production in the marine boundary layers is from MeSH oxidation. Because this large additional source of marine $SO_2$ has not been considered in the global modeling studies, the finding for this study has a large impact not only on chemical kinetics but also on climate implications including the elucidation of particle formation/growth and/or CCN activity.

The paper is very well-written and organized. The background of the study, experimental setup, data analysis, and parameters for the modeling are all well described. The discussion of the results is expedient and understandable. I recommend this paper be published after minor/technical corrections. Although I have tried my best to find some problems to provide constructive comments for this study, I could not find any fatal problems that may help this manuscript. For this reason, the followings are my minor

comments.

Minor comments

Line18: I guess these values are also interquartile range? If so, please write so.

Line 140: You described both DMS and MeSH in this paragraph and different from the section title of 1.3. From this sentence, you can start another section "1.4 Purpose of this study" or something like that.

Line243: It is hard to understand which parameters are Meteorological inputs from NOAA data. Please specify what you cited.

Line321: The ranges of the flux of DMS and MeSH are different from Figure 3 as far as I read. As written, 0.53 to 1.61 are the interquartile range, however, the flux is varied over the hour of Day. I do not understand why the author used an interquartile range for the flux data, but the standard deviation is added in Figure 3. To improve the readability, I hope the authors modify the value in the text or the presentation in Figure 3. The same comments are also for MeSH.

Line372: It is very hard to understand "The poorer model performance at night" in this context. When I saw Figure 6, the difference between model and observation seems larger in the daytime for DMS.

Line374: Similarly, I do not understand "The model shows generally good performance during daytime" here. What does it mean "good performance" here? The difference between observation and model was larger in the daytime for DMS (Figure 6a).

Line 374: Please add an explanation why you use 1 ppt for BrO. I think you need some citations to choose this value for BrO concentration for your model.

Line 384: "reproduce observed DMS and MeSH" are hard to be convinced because of the comments above.

Line398: For me (perhaps also for readers), "a 43% increase in total SO2 production" has a larger impact compared to 30% of the overall SO2 production. I suggest adding this 43% increase in your abstract in addition to a 30% description.

Line424: SI S3? I think you described HPMTF for SI S5.

Recommendation

It is not easy to follow these complicated chemical networks for sulfur oxidation. If you prepare a Figure to describe the chemical scheme in addition to Table S1, the reader will be able to understand the reaction mechanisms for DMS and MeSH oxidation to SO2 more easily. I suggest drawing something similar to Chen et al. 2018 (Atmos. Chem. Phys., 18, 13617–13637, 2018 https://doi.org/10.5194/acp-18-13617-2018).

Technical correction

$F_{DMS}$ should be $F_{\mathrm{DMS}}$ in the main text, and the $F$ (Flux) for the caption in Figures 3,4,5, and 8 should be italic. Likewise, all variables should be italic throughout the manuscript.

Overall, I enjoyed reading your manuscript. Thank you very much.