

Interactive comment on “DMS oxidation and sulfur aerosol formation in the marine troposphere: a focus on reactive halogen and multiphase chemistry” by Qianjie Chen et al.

Anonymous Referee #2

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This is a good paper that has been well prepared. The goals and methods are clearly presented, and I have mostly textual suggestions that can be found in the attached pdf, along with one or two more serious issues.

I recommend publication when my points are properly addressed.

One main point comes at the end of the paper. The main subject is the importance of halogen chemistry for the DMS → sulphate oxidation pathway. New intermediates and reactions are included in a global chemistry-transport model.

This gives better validations of intermediates, like MSA and MSA/nssSO₄ ratios. Also, the main uncertainties in the reaction pathways are clarified. However, for the formation

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of sulphate aerosol the new pathways have limited impact, simply because this is the end-product. Only the speed of the DMS → sulphate conversion may be affected, and maybe some intermediates have efficient dry- or wet deposition pathways. Thus the phrase "...with a simplified DMS oxidation scheme (gas-phase oxidation by OH and NO₃ only) may overestimate sulfate abundances in the pre-industrial environment, potentially leading to underestimates in sulfur aerosol radiative forcing calculations in climate models." seems overstated. I expect no huge impact in sulphate forcing and some reflection on this is recommended.

For the rest of my comments, see annotated manuscript.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2018-410/acp-2018-410-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-410>, 2018.