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Comment on acp-2021-782

Anonymous Referee #2

Referee comment on "Exploring dimethyl sulfide (DMS) oxidation and implications for global aerosol radiative forcing" by Ka Ming Fung et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-782-RC2, 2021

Review of Fung et al. Exploring DMS oxidation and implications for global aerosol radiative forcing, ACP

Fung et al present a model study of the global DMS oxidation system, extending their chemistry scheme to take into account new insights around HPMTF. The revised chemistry scheme also includes MSA chemistry, knowledge of which is somewhat established but seldom included in models. A comparison of the revised model output to recent measurements is also presented.

The updated chemistry is used in calculating a revised aerosol indirect radiative forcing, arriving at a value of -2.3 Wm-2. This value is similar to previous estimates from this model, but the updated chemistry reveals significantly altered contributions from gas- and aqueous-phase oxidation pathways, and associated spatial differences.

The manuscript is well written, and considers a range of uncertainties in the reaction rates etc, including their potential impacts.

The supplemental material contains additional valuable figures and information. Inclusion of this information in the main manuscript could easily be justified (the supplementary material is extensively referred to throughout the manuscript), but would make the manuscript considerably longer and would likely distract from the main points of the study.

I have two major comments, and recommend publication when the points below are addressed.

Major comments

DMS emissions are suggested to be too high, leading to too high concentrations being simulated (when compared to observations). A reduced DMS flux simulation is provided in the supplementary material, leading to better agreement with observed DMS. What impact does the reduced DMS flux have on MSA, HPMTF, IRF etc? Presumably the burdens of these species are significantly altered. Interpretation of Figure S5 might also differ. All of this suggesting there would be consequences for aerosol IRF if [DMS]a was more accurately simulated.

Has any evaluation been conducted with respect to aerosol number concentrations, e.g. CCN? How well are these quantities constrained? It is not currently possible to assess the ability of the model to reproduce fundamental aerosol parameters, which limits confidence in the conclusions. Please provide, at least, surface maps of CCN so that the reader can determine whether the model has an ability to simulate CCN effectively (and therefore CRE).

Minor comments

There are several references to IPCC AR5, which itself refers to quite old literature. It would be better to refer to AR6. And better still, to the literature referred to therein.

Is there any vertical distribution of the sulfur (or other) emissions? Or are all emissions injected at the surface?

Fig.1 caption: it would be useful to have an indication of what is considered long-lived