

We thank the reviewer for the positive feedback and the constructive comments that we address point by point below. The review comments are repeated here in black, our replies are given in blue.

Major comments:

1. The figures all have low resolutions. It is difficult to read the superscripts in the axis legend. Figures of a better quality need to be provided.

The quality of all figures is improved in the revised manuscript.

2. Are there any losses of precursor or products to the chamber wall?

We do not expect significant loss of the precursors and products shown on the FEP walls of the chamber at the selected/observed concentrations on the time scales of the experiment, and we do not see any evidence for such deposition processes in our results.

3. In terms of the sulfur budget, were there any aerosol formations in these experiments?

Formation of sulfate aerosol during our experiments can't be ruled out. However, it is neither supported by the observed sulfur budget (Figure S6) nor expected under the experimental conditions. Note that during previous SAPHIR experiments, in the low concentration range used in our experiments, particle formation was never observed without the introduction of aerosol seeds (Carlsson et al., 2023; Brownwood et al., 2021).

4. In Figure 2, it seems that DMS started to decay before the UV light was turned on. Is the decay from DMS+O₃? The figure also suggests that HPMTF formation started before light was on. Does this indicate HPMTF can also be generated from DMS+O₃?

The observation that DMS starts to decay before turning on the UV lamps is, indeed, one of the mysteries in our experiment, and we have spent some time discussing this. The chemical model does expect DMS reactivity to be dominated by O₃ here, but the rate is too low to explain the observed decay. Even with an enhanced DMS + O₃ rate constant of $\sim 1 \times 10^{-18}$ observed at high relative humidities by Wang (2013), the expected DMS loss rate is about a factor of 10 slower than observed. Only if DMS + O₃ in the aqueous phase (kinetics were reported by Hoffmann et al., 2016) is considered, the observed DMS loss can be explained, e. g. in a water film forming on the cold chamber walls in the morning upon humidification. But this is really speculation and we would rather not include this in the paper.

With respect to HPMTF generation from DMS + O₃, we would not interpret the non-zero HPMTF levels as evidence for HPMTF presence or formation before the light is turned on, although we can also not exclude this. The difficulties of establishing a proper background for HPMTF and TPA are explained in some detail in the response to a comment by Reviewer #1 (re. line 181) and we can only report none-background corrected values. Therefore, it is not possible to establish a HPMTF formation from DMS and ozone from our observations.

Technical comments:

5. Line 546, should be “OCS production rates given in panel d”).

Thanks for spotting this, we correct it in all captions of Figures 2 – 4.

6. The OCS time series are quite fluctuated. Did the authors calculate OCS production rates using a longer period of time and what is the difference?

The fitting of the OCS production rates is explained in the figure captions. Fits were always made over 30-minute time periods as a compromise between time resolution and precision of the calculated rates. The instrument precision and, in case of Experiment I, gaps in the data acquisition, would not allow for meaningful results over shorter time periods. For experiments II – IV, fits over longer time periods yield essentially the same results as averaging the 30-minute rate values, as one would expect.

References:

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