Comment on manuscript egusphere-2024-1975

August 28, 2024

General comment

Based on field measurements the authors present an assessment of how much the HPMTF loss to cloud affect the yields of SO2 and OCS from DMS oxidation. The manuscript is well written and conclusions sound. The analysis looks valid given the limited of knowledge about aqueous-phase oxidation of organo-sulfur compounds in the real atmosphere. I only have some remarks concerning how heterogeneous losses have been accounted for.

Major comments

- **Loss to aerosols**
  In Eq. 1 the expression for the loss frequency, k, to aerosols is given in a specific form which might not be familiar to every reader. It took me a while to find the proper reference [1] and assure myself it is correct. Please add that reference. The equation for k is normally used with α instead of γ. For aerosol uptake the latter is more commonly used in an expression with only the second term as the cited DMS-relevant works [2, 3]. Why did the authors not used the usual form $1/4\ v\ \gamma\ A$? A short explanation would be help the reader.

- **Loss to cloud droplets**
  It has been reported that the DMS loss due to aqueous-phase oxidation by O3 is not neglibible [4]. If considered by the authors, this would imply larger cloud loss of HPMTF in order to match the observed DMS/HPMTF ratios. Moreover, as it could be seen by the *gamma* values higher than for HPMTF, loss of DMSO, DMSO2, MSIA and MSA to clouds is likely affecting the SO2-yield as well. I understand the model used by the authors lacks explicit cloud chemistry but at least it would be good to have these points in the discussion of results.

Minor comments

- p. 3, l. 75: Upon O2 addition to the CH3S radical CH3SOO is produced and not CH3SO2. The latter comes later from CH3SOO isomerization and the T-dependent branching ratio between channels yielding either SO2 or MSA are for reactions involving CH3SO2. However, the CH3SOO isomerization is not included neither in MCM nor in the updated F0AM model as one can learn from Table S1. The author should consider how much of an impact it has on the overall SO2 conversion from DMS oxidation in their model.

- p. 18, l. 448: Saying that the results of this work "align" with the OCS-yield determined in the experiments by [5] is misleading. Those experiments were done in a chamber under non-atmospheric conditions and surely without liquid water. I would rather say it is a fortuitous coincidence that the two OCS-yields meant here are similar.

References

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