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Comment on acp-2022-215

Anonymous Referee #2

Referee comment on "Constraining the budget of atmospheric carbonyl sulfide using a 3-D chemical transport model" by Michael P. Cartwright et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-215-RC2, 2022

This paper presents forward simulation of OCS using the TOMCAT model. Two main simulations are presented: a control simulation and a simulation in which GPP from the JULES model is converted into an OCS flux using the LRU approach.

Results generally show an improvement with the surface observation network, and a favorable comparison with the ACE-FTS observations.

The paper is well written, with a clear structure. However, the results are rather thin in the sense that the field of OCS research is moving rapidly, and formal inversion systems are now in place. In that sense, the "hand-adjusted-flux" approach in this paper may be a bit outdated. The comparison with ACE-FTP and the use of a new model (TOMCAT) and biosphere model (JULES GPP) provides sufficient new information. The paper can might provide a valuable addition to the existing literature, after addressing some major issues.

First, when the results are described in the paper, often hand-waving argumentation is used to explain the deviations between model and observations. "Likely caused", "could be attributed". Here, we have to believe the judgement of the authors, since rarely additional arguments are presented. Likewise, the underestimation of modelled OCS in the tropical stratosphere is explained by too fast removal. These observations call for additional simulations to verify whether speculations hold true. Two suggestions here: (1) a simulation with tagged tracers to be used in a more detailed analysis of e.g. seasonal cycles (2) a simulation with reduced photochemical removal in the tropical stratosphere. Presentation of the results would give the paper more body.

Second, the LRU approach is defendable, and uses seasonal CO2 mixing ratios to convert GPP into a OCS flux. It remains unclear what is taken for the OCS mixing ratios here. In the recent papers of Ma et al. (quoted), and Kooijmans et al. (Biogeosciences, 2021, not quoted) it is clearly shown that OCS fluxes become substantially smaller in regions with low OCS abundance (F_OCS = -vd*OCS). I am a bit surprised that nothing is mentioned on how OCS mixing ratios are used to convert GPP to a OCS flux. If a constant value of e.g. 500 ppt is taken, the final OCS flux may become substantially smaller. Kooijmans et al., (2021) report a drop in SIB4 from 922 Gg S yr−1 in the original SiB4 to 753 Gg S yr−1 when accounting for varying OCS mixing ratios.

Finally, the field of OCS research is moving fast. The paper therefore misses quite some recent references that are relevant for the work. The authors should update the reference list (and discussions) with more recent papers.

Further comments are in the accompanying annotated pdf file.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2022-215/acp-2022-215-RC2-supplement.pdf