Comment on acp-2021-813
Simone Tilmes (Referee)

Referee comment on "An approach to sulfate geoengineering with surface emissions of carbonyl sulfide" by Ilaria Quaglia et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-813-RC2, 2021

This study discusses a new potential approach for sulfate geoengineering using an enhancement of COS at the surface. The study is very well organized, clearly written, and nicely presented. The paper should be published as an important contribution to possible approaches for sulfate geoengineering and their effects. I am supporting the publication after the authors considered the following points listed below. While this is a minor revision in terms of workload, I am suggesting major additions to the discussion and the abstract.

Here are some major concerns considering this new approach that has not been clearly addressed in the paper:

- COS is more uniformly distributed in the troposphere and has a much longer lifetime than sulfate. This however suggests that there is much less control using COS than for example using SO$_2$ injections for sulfate geoengineering and limits the potential use of a feedback control algorithm to modulate the amount of cooling in different hemispheres. However, earlier studies by Kravitz et al. (2017) have shown the (game-changing) potential of using a feedback control algorithm to reduce side-effects for instance to reach surface temperature targets or other impact-relevant targets. This is a major drawback of this approach in addition to the lack of the rapid regulation of the injections in case of a large volcanic eruption as already pointed out in this paper.

- Due to its toxicity, there is seems to be a hard limit in using COS. This study increased surface COS concentrations to reach 35ppb, which is very close to the possible limits of 50ppb, as stated in the text. The first concern that needs to be at least mentioned for future work is to identify how reliable the studies are that estimate this toxicity considering potential long-term increases of COS. Secondly, the enhancement of COS reduces AOD by 0.08 with a radiative forcing of -1.3W/m2, which may be translated to less than 0.5 degrees of cooling. Due to the uncertainty in different models, the cooling could be less and may not be sufficient. Furthermore, due to the long phase-in time, it will take a long time to find out how much cooling can be achieved. What if more cooling is needed? Would COS be eventually replaced by SO2 injections? What is the point then of using it in the first place, while adding the danger of exposing humans and ecosystems to a toxic pollutant? To me, this is a major issue that may be a
showstopper for considering this method.
- It needs to be more clearly stated that surface UV is largely reduced with this method, and this can be harmful to humans and ecosystems. A more detailed discussion with the region and season would be helpful.

**Detailed comments:**

Abstract: The last sentence in the abstract stated that COS emissions are feasible. It may be technically feasible, but I think, the authors need to also point to the drawbacks of this approach in the abstract and conclusions, including the limitations compared to stratospheric SO2 injections.

Line 19: I don’t think, this type of intervention can be classified as a “short-term” intervention, earlier studies have shown, that even a “Peakshaving” scenario may require injections between 80-160 years (Tilmes et al., 2016, 2020).

Line 22: “optically active” is somewhat strange. You could maybe say, “the aerosol layer is thickened and therefore reflects more sunlight...“

Line 27: I don’t understand what you mean with “any proposed compound would quickly react to form sulfate aerosols”, only sulfur will form sulfate aerosols. Other components may be coded with sulfates that are in the atmosphere. Is that what you mean?

Line 38: Is this the tropospheric lifetime? What is the stratospheric lifetime.

Line 65: Since you are looking at UV, please also state what photolysis scheme is used in the model and if photolysis varies with aerosol concentrations.

Line 72: Is there a reason why increase COS emissions are placed at the same locations as the anthropogenic emission?

Line 101: Could you add an estimate of how much surface cooling one expects with an increase in 0.08 AOD? I would be probably less than half a degree of cooling considering the related GeoMIP experiments, is that correct?

Line 104: Didn’t you state that the lifetime is 35 years, not 3.8 years? What is different here?

Line 106-107. I don’t follow, what is meant here. 0.8ppbv of what, and why do you refer to the RF of other greenhouse gases? Do you mean, that after 20years, COS values have declined close enough to the background to not have a significant impact on the RF?

Line 230: The catalytic NOx cycle is decreased with more surface area density, which results in less ozone loss. Isn't the inhibition of denitrification more important in high latitudes and for cold temperatures, and less in other latitudes

Line 234: “its photodissociation” what is “its” referred to here?

Line 240: change “UVA decrease is everywhere negative in both SG experiments” to “UVA decreases everywhere in both SG experiments”

Table5 and Figure 8: You are nicely showing total column ozone variations with region and seasons, and it is very clear that there are differences in sign in the response. Illustrating
UV changes annually is not a very meaningful measure especially for high latitudes. I would strongly recommend expanding this figure and showing 4 seasons instead or in addition to the annual values. I would also expand the discussion on the UV impacts, and why UV-A and UV-B and shown separately. Not sure if there is any reference to Figure 8 in the text?

Line 269: reference should be Brühl et al. (2012)

Line 289: should be (Figures 9 and S6)