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Comment on acp-2021-813

Anonymous Referee #1

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-RC1, 2021

Review: https://doi.org/10.5194/acp-2021-81, A novel approach to sulfate geoengineering with surface emissions of carbonyl sulfide by Quaglia et al.

Overview: This paper proposes using carbonyl sulfide (COS) released at the surface for increasing stratospheric sulfate aerosol. This proposal has the advantage of not requiring distribution by stratospheric aircraft. The paper gives a great amount of detail on the differences between the three scenarios examined, a background, a geoengineering scenario using SO2 and one using COS. It is generally well written. I recommend publication after addressing the comments below, and most importantly, comment #1.

1) Crutzen (2006) https://link.springer.com/article/10.1007/s10584-006-9101-y states "An alternative may be to release a S-containing gas at the earth's surface, or better from balloons, in the tropical stratosphere. A gas one might think of is COS, which may be the main source of the stratospheric sulfate layer during low activity volcanic periods (Crutzen, 1976), although this is debated (Chin and Davis, 1993). However, about 75% of the COS emitted will be taken up by plants, with unknown long-term ecological consequences, 22% is removed by reaction with OH, mostly in the troposphere, and only 5% reaches the stratosphere to produce SO2 and sulfate particles (Chin and Davis, 1993). Consequently, releasing COS at the ground is not recommended."

Based on this, first, this paper should note that this has been proposed before (and therefore may not be so novel) , and discounted due to potential ecological damage. There is discussion at the end of the paper that this sort of thing should be looked at. That discussion should be at the start of the paper. Second, is the balance between global warming due to COS vs cooling due to aerosols produced taken into account in this study? Bruhl et al. 2012 state "Further, using a chemical radiative convective model and recent spectra, we compute that the direct radiative forcing efficiency by 1 kg of COS is 724 times that of 1 kg CO2. Considering an anthropogenic fraction of 30 % (derived from ice core data), this translates into an overall direct radiative forcing by COS of 0.003 W m−2 . The direct global warming potentials of COS over time horizons of 20 and 100 yr are GWP(20 yr) = 97 and GWP(100 yr) = 27, respectively (by mass). Furthermore, stratospheric aerosol particles produced by the photolysis of COS (chemical feedback) contribute to a negative direct solar radiative forcing, which in the CCM amounts to −0.007 W m−2 at the top of the atmosphere for the anthropogenic fraction, more than two times the direct warming forcing of COS. Considering that the lifetime of COS is twice that of stratospheric aerosols the warming and cooling tendencies approximately cancel." They also say "Therefore, if we account for indirect chemical effects in GWP calculations, also customary for gases such as methane (IPCC, 2007), it follows that COS has almost no net climate impact." So, if the warming and cooling tendencies cancel, is there an actual advantage to increasing COS emissions? At line 280 this paper states that the COS case produces an RF of .17 W/m2. What is the RF due to the increased sulfate aerosol produced by the COS? How do these results compare to the conclusions of Bruhl et al. 2012?

2) The text makes the statement "it is not a toxic gas for humans or ecosystems: negative effects have been found only when concentration exceed 50 ppm (100,000 times more than the background mixing ratio (Kilburn and Warshaw, 1995; Bartholomaeus and Haritos, 2006)." The Bartholomaeus and Haritos paper does not have that statement, and the Kilburn and Warshaw paper is about H2S and not COS. Different references are needed to say that there are no negative effects on plants and ecosystems.

3) The Budyko, 1978 reference link (Budyko, M. I.: The Climate of the Future, American Geophysical Union, https://doi.org/10.1002/9781118665251.ch7, 1978.) is broken (it says the doi cannot be found, this is apparently an error on the AGU web page.). I believe the publication date is actually 1977, and you can get to the full book at https://agupubs.onlinelibrary.wiley.com/doi/book/10.1029/SP010. The reference should be to the full book, since chapter 7 doesn't address the point being made. And, in support of the text (actually proposing SRM) NAS 1992 (National Academy of Sciences (NAS): 1992, Policy Implications of Greenhouse Warming: Mitigation, Adaptation, and the Science Base, Panel on Policy Implications of Greenhouse Warming, Committee on Science, Engineering, and Public Policy, National Academy Press, Washington DC, 918 pp, https://www.nap.edu/catalog/1605/policy-implications-of-greenhouse-warming-mitigation-adaptation-and-the-science should be added.

4) Line 72: It should be noted at the beginning of the paper how the increase of 40 Tg-S/yr compares to current emissions. I finally found the factor of 400 in the conclusions.

5) Line 105-106 says "This means an increase of 0.8 ppbv with respect to background condition, that would produce a direct RF negligible if compared to other well mixed greenhouse gases." First, change to "a direct RF that is negligible compared to other well mixed greenhouse gases." Second, is the RF negligible to the negative forcing caused by the increased stratospheric aerosols?

6) And, another experiment that could be run. Instead of emitting a large amount of OCS at the surface, what about emitting at the tropical tropopause? It would require significantly less material, and may produce similar results, with a higher aerosol layer

and similar latitudinal distribution. It would also avoid any issues with ecological damage due to increasing surface COS amounts.