

Interactive comment on “Quantification of sulfur deposition changes under sulfate geoengineering conditions” by Daniele Visioni et al.

Anonymous Referee #1

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In the introduction, the authors already say that this question has been addressed by Kravitz et al. (2009), but they do not make a case for doing the same study again. What are the scientific questions that they are addressing that have not been answered before? Without this, I wonder why readers would want to wade their way through all the details in this paper of repeating the same experiment with two more models. Is there something about the current two models that would produce a more accurate simulation and have the potential to get a different result than before? If not, why do it? Yes, these models have an explicit nudged QBO, but would that be expected to produce anything fundamental that is different about sulfate deposition?

Therefore, I recommend this paper be rejected as it has no new scientific conclusions.

The abstract and conclusions use the metric of % of current sulfur deposition, but

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this relative value is not nearly as important as the total mass. Does the increased deposition in pristine regions represent a threat in terms of acid deposition on land or in the ocean?

The global average results they get can already be easily calculated because in equilibrium 8 Tg SO₂ into the stratosphere per year will produce the same surface deposition.

p. 3, lines 31-33: The authors say they did a G4 experiment, but G4 required 5 Tg SO₂ per year and not 8 Tg/year. This has to be corrected here and throughout the paper. G4 also had a 50-year emission and then a halt to emissions. The authors also have to explain why they chose to use 8 Tg/year.

Why was the ULAQ model run with such low horizontal resolution but high vertical resolution? Does this affect the results? For example, how well is tropospheric deposition really simulated, as I would think the precipitation would not be expected to be able to address issues like rainout and washout of sulfate aerosols, and distinguish between wet and dry deposition? How well does it do this for the current climate with not geo-engineering? Geos-Chem also seems to combine low horizontal resolution with high vertical resolution. Why?

p. 9, line 22, starts in the middle of a sentence. Something is missing.

How do you explain the longitudinal patterns of deposition changes in Fig. 11? Why are the depositions in the Northern Hemisphere much larger in the already polluted regions? Kravitz et al. (2009) also found this (their Fig. 2), at least for North America and Asia. Is this the region of maximum STE along tropopause folds and storminess, and just over the polluted regions by chance?

The authors would also need to address the 14 comments in the attached annotated manuscript.

Please also note the supplement to this comment:

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<https://www.atmos-chem-phys-discuss.net/acp-2017-987/acp-2017-987-RC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-987>, 2017.