**Response to Reviewer # 1**

Reviewer's comments are in blue. Author responses are in black.

This is an interesting new investigation into the response of global sulfur deposition to a sulfate geoengineering scenario. Three central questions are addressed: the net global and regional response of sulfur deposition rates to a specific sulfate geoengineering scenario; the quantitative and mechanistic differences between two models (with and without dynamic feedbacks) in their calculated responses; and the role of the quasi-biennial oscillation (QBO) in understanding the response. Feedbacks via modification of the QBO are not investigated. The authors conclude that, although the models are in agreement regarding background sulfur deposition, significant inter-model differences exist between the deposition patterns predicted for a 4 TgS/yr geoengineering scenario. However, the effect of the QBO is broadly consistent between the two models. During the QBO W phase (E shear), longer lifetimes are observed for the injected aerosols, but the overall AOD achieved is maximized during the E phase (W shear).
The central questions of this paper are mixed in terms of the level of interest, but the methods used are appropriate. The data produced by the paper fully support the conclusions, which have been appropriately caveated to take into account the limited scope. I particularly appreciate the fact that the paper is trying to isolate the effect of the QBO on sulfate geoengineering in the absence of feedbacks, which provides insight which might be lost or obscured in a model with a fully interactive QBO.

We would like to thank the reviewer for taking the time to thoroughly read the paper, and for his generally positive comments.

However, the paper is misrepresented by its title. It promises only a rerun of the work by Kravitz et al (2009), which already quantified sulfur deposition under a geoengineering scenario almost identical to that presented here. This is a shame, because the authors present a detailed and interesting investigation of the mechanisms by which sulfate geoengineering might increase sulfur deposition rates, with a deep dive into the role that might be played by the QBO which I find to be deep and insightful. I would strongly advise that the authors consider a new title which highlights their work on mechanisms, model intercomparison, and the role of the QBO.

We thank the reviewer for his insightful analyses. We will most definitively modify the title to shift the focus more towards the mechanisms that tie stratospheric dynamics and sulfur deposition. We believe the new title could be "Sulfur deposition changes under sulfate geoengineering conditions: QBO effects on transport and lifetime of stratospheric aerosols". The abstract will also be largely adjusted to highlight these aspects.

Such an intercomparison should include a thorough analysis of the differences and similarities between the results from this study and those from Kravitz et al's original analysis. The paper should also be restructured to bring their thorough work on mechanisms to the fore, rather than the already-explored net impact of geoengineering on deposition. If such changes are made, and if the other comments below are addressed, I believe that the results shown would be appropriate for publication in ACP.

We thank the reviewer again. In the revised manuscript, we will move the focus more towards what is now Section 3 (different effects of E,W QBO regimes on the injected stratospheric sulfate) and in that part of Section 4 dealing with QBO effects on strat-trop aerosol exchange and surface deposition. We will make it clear how a coupling of stratospheric transport oscillations (i.e., QBO) and aerosol microphysics may produce significant effects in sulfate aerosol transport, size distribution, lifetime, cross-tropopause fluxes and finally surface deposition.

As for many studies of geoengineering, the authors make comparisons to results from studies of volcanic eruptions. However, the comparisons often seem superficial, such as the paragraph beginning on line 16 of page 24, where a study of Tambora by Marshall et al (2017) is invoked without any serious quantitative comparison. The Marshall et al paper in particular is heavily referenced in spite of not having passed peer review at time of submission. I recommend that the authors make their comparison to volcanic eruptions more quantitative. The differences between a volcanic and SG scenario should also be made clearer and more quantitative prior to any comparison, including differences in aerosol size evolution, lifetime, and distribution.

Regarding the Marshall et al. (2017) paper (now in pre-print), we didn't make the discussion quantitative because we were aware that it would not have been completely scientifically sound, as we did for example regarding the baseline deposition with the Vet et al. (2014) paper. We will try to make it clearer in the revised version what are the limits of comparing volcanic eruptions and SG, as the reviewer suggests, by discussing specific points, such as the size distribution and e-folding time evolution and the impact of both geographical location of the eruption and timing (in relation to the QBO phase). The reference to Marshall et al. (2017) will be made significantly lighter in the revised manuscript. However, we believe that the use of sulfate deposition measurements from ice cores in Greenland and Antarctica may be seen as an added value of our study, although with the above caveats.

The authors may want to consider reporting their results normalized by the injection rate. This would allow a more direct comparison to other work, including that of Kravitz et al, and is already implied by (eg) the discussion regarding linearity on page 30.

This is a very interesting suggestion, and one we most definitely follow. Thank you. Part of the discussion on surface deposition will be changed by normalizing the results to the injection rate.

Minor comments and suggestions

Table 1: The GEOS-Chem vertical grid should read "hybrid pressure-sigma", not "hybrid pressur-sigma"

Corrected

Page 6, line 20: "with a 11.5% due to" should be "with 11.5% due to"

Corrected

Page 10, line 17: "aerosol firsts by looking" should be "aerosol first by looking"

Changed

Page 10, line 22: The opening of this sentence appears to be missing.

We corrected this oversight on our part. The words “The ULAQ-CCM” are missing.

Page 21, line 11: I assume this should be “1-2” days. The use of a division sign instead of a dash happens elsewhere too (also page 24, line 13).

Corrected everywhere.

Page 24, line 4: “pointing out to a” should be “pointing to a”

Corrected

Figures 13, 14, 15, and 16: it is not clear to me why the points are joined by a line. This implies continuity between data points along an axis where none exists, as each point represents a distinct region.

Fig. 13 will be modified accordingly (local deposition is closely related to local tropospheric emissions of sulfur with short lifetime and to the surface area of any region; so that there is no rationale in connecting points for different geographical regions). Fig. 16 does not exist. Figure 14 and 15 will be merged in a single six-panel figure with the first two panels in absolute units and no connection of points (absolute deposition changes are again a function of the surface area of any region). The subsequent four panels will be those of Fig. 15 (percent changes) and we believe they might stand as they are now. Here, in fact, we would like to emphasize the interannual variability driven by the QBO with respect the total variability, which also includes monthly changes. Both land and ocean regions are ordered from South to North, so that the latitude-dependent relative weight of the QBO-driven variability on percent deposition changes is better readable and the use of shaded areas helps in showing the two variabilities at the same time.