Replies to comments by reviewer 1

Comment: The manuscript examines the ability of satellite occultation instruments to monitor potential geoengineering experiments using MAECHAM5-HAM simulations and retrievals with the SCIATRAN radiative transfer model. While the model experiments are straightforward and easy to interpret, I have two major concerns regarding the analyses presented in the paper.

Reply: We thank the reviewer for his/her constructive and helpful comments. We tried to answer every comment in an appropriate way.

Comment: First, the use of data from the initial two years of the simulation raises concerns about whether the model has reached an equilibrium state. It is crucial to ensure that the model's early-phase data are suitable for analysis. Could the authors provide insight into the model's performance during this period and demonstrate that it has stabilized?

Reply: We added the following figure to the appendix and explanatory text to the ECHAM section:

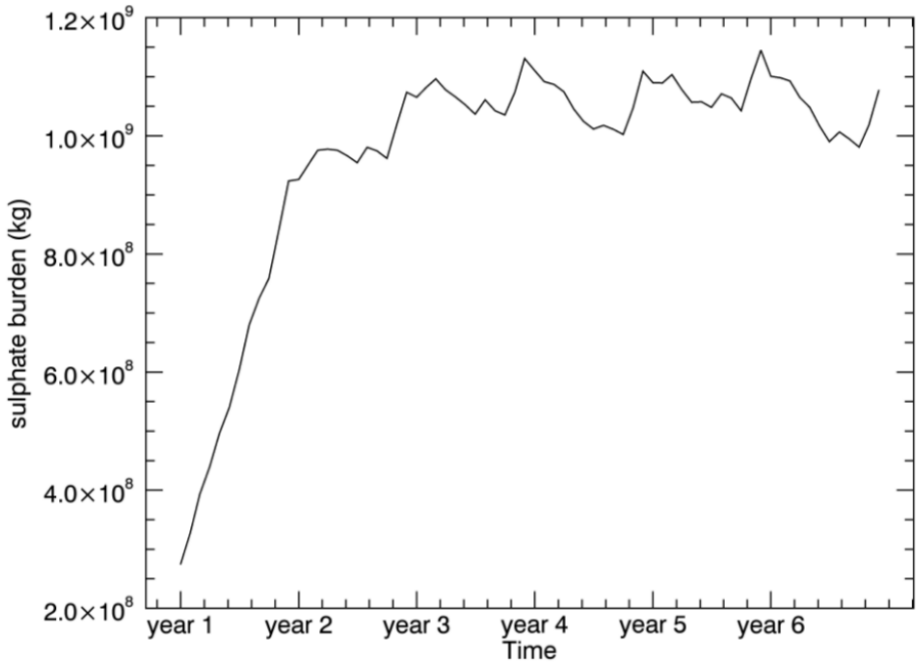

Figure A1. Monthly mean sulphate burden in kg over time (2005 – 2010) for 1 Tg S/y, showing the differences between the two-year initial phase and the quasi steady-state phase.

The time series of the global sulphate burden shows that the steady-state phase is reached after two years (compare Fig. A1). We used the early phase to include sulphate levels below the steady-state level to see if we could detect sulphate even earlier. At this point, the goal was not to use a stabilized result. The aim was to find a lower threshold at which detection would be possible.

Comment: Second, in line 214, the manuscript states, "The errors shown and discussed so far are based on one extinction profile per month." It would be beneficial for the authors to clarify their ratio-

nale for this choice and to compare averaged profiles with those currently presented in the manuscript.

Reply: Thank you for pointing that out. We were unclear at this point. The aerosol extinction profiles shown from the ECHAM simulations are monthly averages. To avoid further misunderstandings, we have removed the passage.

Comment: Beyond these major concerns, the manuscript is well-written and presents a novel approach by using a relatively small sulfur injection to assess the model's response. The study is valuable to the scientific community, and I have provided specific comments below that should be addressed before publication.
Comments:
1. Lines 71-73: How is the model's dynamics treated? Is it prescribed using a climatology? Please elaborate.

Reply: MAECHAM is a general circulation model which solves prognostic equations for temperature, surface pressure, vorticity, divergence and phases of water. The model simulates the related dynamical processes and generates the quasi biennial oscillation in the stratosphere. The model is not coupled to an ocean model. Imstead, sea surface temperature is prescribed and set to climatological values (Hurrell et al., 2008), averaged over the AMIP (Atmospheric Model Intercomparison Project) period 1950 to 2000, and does not change due to SAI. No nudging, relaxing the prognostic variables towards an atmospheric reference state to, e.g., ERA5 data, is applied.
We added these additional explanations to the MAECHAM-HAM section.

Hurrell, J. W., Hack, J. J., Shea, D., Caron, J. M., and Rosinski, J.: A New Sea Surface Temperature and Sea Ice Boundary Dataset for the Community Atmosphere Model, J.Climate, 21, 5145–5153, https://doi.org/10.1175/2008JCLI2292.1, 2008

Comment: 2. Lines 75-76: Could you clarify whether a single realization or an ensemble is used? If only one realization is run for 15 years, what is the spin-up time? If the first two years and the last three years are analyzed, is the model in equilibrium in the first two years (initial phase)? A time series showing equilibrium would be useful.

Reply: We performed a single simulation over several years. The injections for SAI ran for 15 years. For our study, we took three years at the end of these simulations and averaged them over time. This is similar to previous simulations and publications, e.g. Niemeier et al. (2020), Weisenstein et al. (2022), where we also used a three-year average. The sulphate burden for an injection of 1 Tg/y stabilises after two years (compare Fig. A1). See our previous answer above regarding the early phase. We added this explanations to the lines 75-76.
Niemeier, U., Richter, J. H., and Tilmes, S.: Differing responses of the quasi-biennial oscillation to artificial SO2 injections in two global models, Atmos. Chem. Phys., 20, 8975–8987, `https://doi.org/10.5194/acp-20-8975-2020`, 2020.
Weisenstein, D. K., Visioni, D., Franke, H., Niemeier, U., Vattioni, S., Chiodo, G., Peter, T., and

Keith, D. W.: An interactive stratospheric aerosol model intercomparison of solar geoengineering by stratospheric injection of SO2 or accumulation-mode sulfuric acid aerosols, Atmos. Chem. Phys., 22, 2955–2973, https://doi.org/10.5194/acp-22-2955-2022, 2022.

Comment: 3. Line 96: Correct the typo “trough” to “through.”

Reply: Changed.

Comment: 4. Lines 97-98: Are these climatological values equivalent to those used in the model if the dynamics are prescribed?

Reply: As described in the corresponding section of the preprint, we use the vertical profiles of pressure, temperature and trace gases from a climatological database implemented in SCIATRAN, which is based on a 3-D chemical transport model. These values will not be identical to those in ECHAM.
Possible errors in the derived aerosol extinction profiles due to inaccurate knowledge or assumptions in e.g. pressure and temperature are part of the sensitivity study we have performed.

Comment: 5. Lines 120-121: Is this statement based on observations or model experiments? Have you conducted a no-sulfur (0 Tg S/year) experiment to establish a background aerosol extinction coefficient? Please see my comment for Figure 7 (lines 187-188).

Reply: The lines say: ‘The retrieval was restricted to the altitude range of the aerosol extinction coefficients provided (see Sect. 2.1), i.e. from 10 to 27 km. Background aerosol coefficient profiles at 520 nm corresponding to the latitude and month were used as a priori information.’
The altitude restriction is based on the altitude range of the given aerosol extinction profiles from ECHAM, which is why we have referred to the ECHAM section (Section 2.1). The background profiles originate from the ECHAM simulations as the following line in the preprint says: ‘These profiles originate from the ECHAM5-HAM model simulations (compare Sect. 2.1)’. (lines 121 - 122).
The background profiles correspond to 0 Tg S/y. To prevent further misunderstandings, we have added this information to the sentences mentioned here.

Comment: 6. Line 120: Insert ”extinction” between ”aerosol” and ”coefficient” for clarity.

Reply: Changed.

Comment: 7. Line 124: Could you please define “quasi-steady state” and “initial phase” more explicitly, especially in relation to model equilibrium.

Reply: We added additional explanations (see above).

Comment: 8. Lines 127-130: What is the reasoning behind showing these figures? Are they meant to highlight differences between the initial and quasi-steady-state phases? If so, please clarify.

Reply: Yes, you are right. We have added this figure to the preprint to show the differences between the initial phase and the quasi steady-state phase.
We have now added this explanation.

Comment: 9. Lines 140-145: The sensitivity analysis is appreciated. Could you elaborate on why differences are larger below 15 km and improve above that altitude? Please do the same for the appendix figures.

Reply: Thank you for the question. We have addressed this point in the discussions: 'At this point, it should be noted that the relatively large errors at low altitudes (compare Figs. 3 and 7) are due to the low extinction coefficients at these altitudes (compare, e.g., Figs. 4 and 5).' (lines 212 – 213).

Comment: 10. Lines 149-155: Why return to the initial-phase data here? If the quasi-steady-state phase is used for analysis, maintaining consistency would be preferable. I doubt if the initial phase data and their analyses are relevant as the model may not have attained equilibrium state by then (first two years).

Reply: Thank you for the comment. Please see our previous detailed answers above (replies to the major concerns).

Comment: 11. First line of section 3.2: Line 149 mentions that retrievals are based on initial-phase data, but this section focuses on quasi-steady-state data. Why mix the two?

Reply: We think there is a misunderstanding here. Line 149 is not in the subsection on the quasi steady-state phase, but still in the general section on results and discussions (in subsection 3.1 Sensitivity study), which is why both phases are addressed in this section.

Comment: 12. Third line of section 3.2: Could you explain why total errors for 1 Tg S/y are greater than for 2 Tg S/y in Figure 3? This seems counterintuitive.

Reply: We have now added the explanations to line 158. It now says: 'Overall, the total errors for 1 Tg S/y are greater than for 2 Tg S/y, which is consistent with the expectations, as the signal is stronger at a higher injection rate such as 2 Tg S/y and the total errors are therefore smaller.'

Comment: 13. Line 162: Please clarify the term "true profiles." Does this refer to model simulation results?

Reply: Yes, you are right. The information is in lines 162 – 163. The entire passage says: 'The following Figs. 4 and 5 show the retrieved aerosol extinction profiles at 520 nm (black dashed lines)

for 1 Tg S/y (Fig. 4) and 2 Tg S/y (Fig. 5), both for January (left column) and July (right column) including the corresponding total errors (red dashed lines), the background profiles (black solid lines) and true profiles (purple solid lines) both at 520 nm (ECHAM5-HAM model simulation results) for 65° N (upper panels), 15° N (middle panels) and 65° S (lower panels).'.
To prevent further misunderstandings, we added this information to other passages in the preprint when the term "true profile" is mentioned.

Comment: 14. Line 175: Please replace ± 55°N with 55S-55N or something similar.

Reply: Replaced with - 55 to 55° N .

Comment: 15. Line 178: Do you have corresponding model simulation values for Figures 4 and 5? If so, please include them.

Reply: This refers to minor point 13 (see above). The 'true profiles' are the results of the ECHAM simulations, as explained above. This means that the ECHAM simulations are shown in the figures mentioned. We added this information in the corresponding text and captions.

Comment: 16. Lines 180-181: Adding model-simulated SAOD using the 55S-55N latitude band would strengthen comparisons. Can you provide insight into the causes of maxima and minima in these figures?

Reply: Thank you for the suggestion. We added the model-simulated SAOD and added the following explanations to the text: "The maximum in SAOD near the equator is due to the fact that 1 or 2 Tg S/y are continuously injected in this region (compare Sect. 2.1). The minima of the SAOD in the subtropics and maxima at mid and high latitudes are due to the latitude dependence of the tropopause height, which is higher in the tropics (about 18 km) than at the mid and high latitudes (about 8 km).".

Comment: 17. Lines 187-188: Section 3.2 is based on quasi-steady-state data, but here total errors seem to be derived from the initial phase. This is confusing—please clarify. Also, if you are analyzing a background case (0 Tg S/y) from the first month of the initial phase, a table in Section 2.1 listing all model simulations (0 Tg S/y, 1 Tg S/y, and 2 Tg S/y) would improve clarity.

Reply: We think there is a misunderstanding. Lines 187 – 188 are in subsection 3.3 Initial phase data (and not subsection 3.2 quasi steady-state), which is why the first sentence in this subsection refers to the initial phase.

Comment: 18. Lines 190-192: Could you please elaborate a bit more on as to why the total errors would be larger for Figure 7 compared to Figure 3?

Reply: Thank you for pointing this out. In the course of answering point 12 (see above), we added

additional explanations.

Comment: 19. Lines 206-207: What is the rationale for using only the first month of the initial phase? How is that related to detectability of satellite occultation instruments? Would the model have reached equilibrium by then? Please see my comments above for line 75.

Reply: Thank you for the comment. Please see our previous detailed answers above (replies to the major concerns).

Comment: 20. Line 214: How was the selection made to use one extinction profile per month for Figures 2, 4, 5 and 9? It would be beneficial for the authors to clarify their rationale for this choice and to compare averaged profiles with those currently presented in the manuscript. Also, please consider showing model SAOD values in Figures.

Reply: As explained above (major point 2), there is a misunderstanding here due to our wording, which we have corrected (see above). The data from the ECHAM simulations are monthly averages.

Comment: 21. Lines 219-220: SAGE III/ISS measurements were not available before 2017. Please correct this.

Reply: Thank you for pointing this out, we corrected that. It now says: "2017 – 2024".

Comment: 22. Lines 236-237: To highlight differences, please consider overlaying SAOD values from Figure 6 onto Figure 10 (e.g., plotting SAOD for 1 and 2 Tg cases at 50S and 30N from January to December).

Reply: Thank you for the suggestion. We tested this, but decided against it, as the graphical representation as a whole is less clear (rather messy) and the informative value at 'a glance' is lost. Nevertheless, we understand that such a visualisation can theoretically be practical for displaying differences.

Comment: 23. Lines 258-260: The discrepancy discussed here may result from the model's non-equilibrium state during the initial phase. To ensure consistency, it would be preferable for the analyses to focus on data from the model's equilibrium (quasi steady) state.

Reply: Thank your for the constructive comment. Please see the detailed answers to the questions about the initial phase above.