Response to Reviewers' Comments

Injection Near the Stratopause Minimizes the Stratospheric Side Effects of Sulfur-Based Climate Intervention

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We thank very much the reviewers for their helpful comments. The response to each reviewer's comment is marked in blue.

RC1

The authors present an analysis of the effects of stratospheric aerosol injections in the upper stratosphere, in comparison to more typical lower stratospheric injections, as simulated in the WACCM-MAM3 climate-chemistry-aerosol model. While the results are generally presented in a clear way and could be interesting I think that the analysis lacks depth in several respects. My major concerns are listed below.

The comparison of emissions at 50 km is only done with respect to equatorial emissions at lower altitudes. However, there are earlier studies comparing different lower tropospheric emission strategies and also reporting some benefits in comparison to equatorial emissions. I think the authors need to put the potential benefits of their strategy into the perspective of these other emission strategies.

We assume the reviewer meant to ask the difference among various lower stratospheric (not "tropospheric") injection strategies.

We added discussions about high-latitude injection strategy in Lines 56-62:

"Tropical injection leverages the ascending branch of the BDC to efficiently transport aerosols into the global stratosphere, producing cooling across hemispheres and more effective surface cooling, while equatorial injection leads to substantial overcooling in the tropics and residual surface warming in the high latitudes (Kravitz et al., 2019; Tilmes et al., 2018b). High-latitude injections reduce the stratospheric warming, enhance polar cooling and sea ice preservation compared with tropical injection strategies (Lee et al., 2021; 2023b). However, it requires larger

injection amounts to achieve the same global cooling as tropical injections due to the shorter aerosol lifetime (Henry et al., 2024; Zhang et al., 2024; Duffey et al., 2025)."

I think that the model validation in 3.1 is too superficial concerning the upper mesosphere. I agree that it is useful to show an AOD comparison for the Hunga Tonga eruption where the sulfate also reached the upper troposphere. However, a distinct difference between emissions at 50 km and lower altitude emissions is the reported meridional distribution. This will depend on the representation of the overturning circulation in the upper stratosphere which for which an important feature is the semiannual oscillation near the stratopause. How well is that represented in the model. I don't think it is sufficient to say "temperature at 100 hPa, QBO strength, and polar vortex strength" show reasonable agreement with reanalysis data, because these are all features evaluated in the lower to middle stratosphere. Of course, evaluations of near-stratopause circulation are more difficult due to the lack of observations, but I think this needs to be discussed.

We agree that model evaluations near the stratopause are not easy due to a lack of observations. We compare the simulated Semiannual Oscillation (SAO) with reanalysis data in Figure S1c to evaluate model performance near the stratopause. The model can reproduce the 1 mb winds from MERRA2. We discuss SAO validation in Lines 124:

"Comparison with MERRA2 reanalysis data (2000-2020) shows reasonable agreement in key stratospheric metrics including temperature at 100 hPa, Quasi-biennial Oscillation (QBO) strength, Semiannual Oscillation (SAO) strength (1 mb tropical zonal winds) and polar vortex strength (Fig. S1), …."

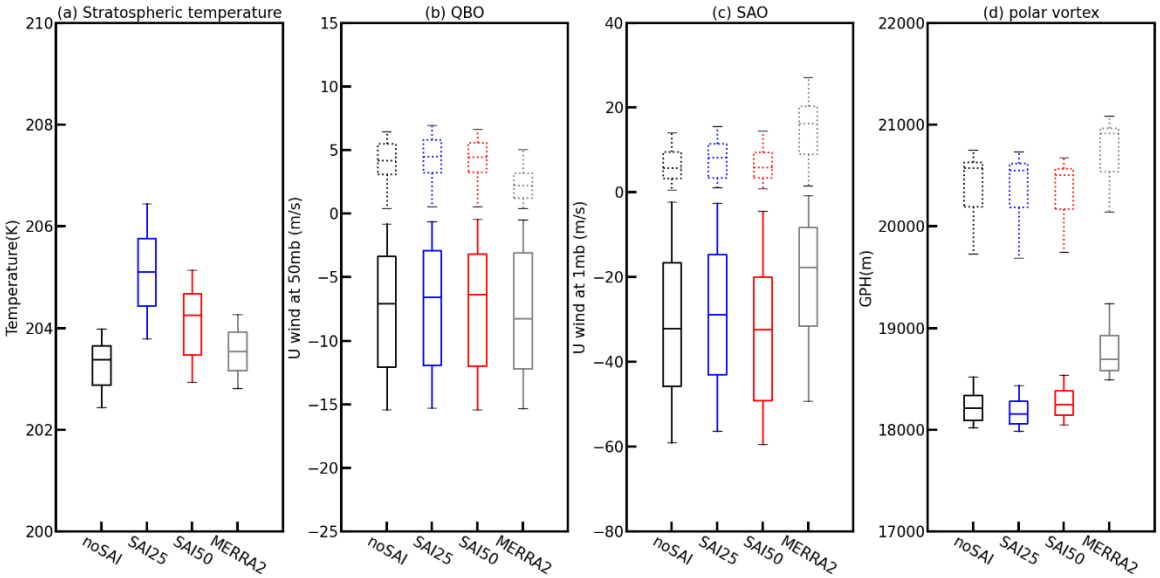

Figure S1: Box plots comparing stratospheric variability between model simulations and MERRA2 reanalysis (2000-2020). (a) near-global (60°S-60°N) temperatures at 100 mb; (b) Quasi-biennial Oscillation (QBO) strength (tropical zonal mean zonal wind at 50 mb) with east phase (dashed) and

west phase (solid); (c) Semiannual Oscillation (SAO) strength (tropical zonal mean zonal wind at 1 mb) with east phase (dashed) and west phase (solid); (d) Antarctic (solid) and Arctic (dashed) polar vortex strength (denoted by the geopotential height averaged over 65°S-90°S at 50 mb) in the model and MERRA2 reanalysis dataset between year 2000 and 2020. Box plots show the median (horizontal line), 25th-75th percentiles (box), and 5th-95th percentiles (whiskers).

I disagree with the title. The study doesn't provide evidence that SAI emissions near the stratopause "minimize" side effects. There could be other strategies reducing the side effects further that haven't been tested. Who knows what effects would be caused by an emission in the upper mesosphere?

Revised to be: "Injection Near the Stratopause Mitigates the Stratospheric Side Effects of Sulfur-Based Climate Intervention"

At the end of Section 3.2 the authors write "The distinct latitudinal and vertical distributions of aerosols in SAI50 enhance climate cooling benefits while minimizing negative impacts of climate intervention." Besides the issue with the word "minimizing" mentioned in connection with the title, I also think this statement is not sufficiently backed up, at least not at this point of the manuscript. Possibly the sentence is meant as an announcement for the following two subsections, but it sounds like a summary.

We revised the sentence in Line 156: "The distinct latitudinal and vertical distributions of aerosols in SAI_{50} are expected to influence the climate cooling benefits and mitigate the associated stratospheric impacts, as detailed in the following subsections."

More in general the manuscript suffers from the lack of the definition of a goal for the SAI. Without such a goal, without defining a metric it is impossible to compare which strategy performs best. The goal could (but doesn't have to) be to produce a climate as similar as possible to an unengineered climate of the same global temperature at lower greenhouse gas levels. In this sense, it is not clear if the stronger Arctic amplification simulated for SAI_{50} than for SAI_{25} is actually a desired effect. How strong is the Arctic amplification in greenhouse gas caused warming in WACCM? Which injection strategy is counteracting the amplification more exactly?

Thanks for the comment.

We acknowledge the importance of defining clear metrics for evaluating SAI strategies. Our study uses a fixed injection rate of 10 Tg per year as an idealized experimental design to compare the fundamental differences in climate response between SAI_{25} and SAI_{50}. This simplified approach

allows us to isolate and understand the physical mechanisms controlling aerosol transport and distribution at different injection heights.

In more practical scenarios (e.g. GLENS, ARISE, GeoMIP), injection amounts would need to vary over time to counteract increasing greenhouse gas forcing (Henry et al., 2024; Macmartin et al., 2022; Tilmes et al., 2018b). Regarding Arctic amplification, previous studies have shown that it cannot be fully offset under tropical SAI scenarios at ~25 km (Figure R1, Henry et al., 2024). While SAI_{25} effectively offsets greenhouse gas–induced warming in the tropics and mid-latitudes, the Arctic still experiences significant residual warming. The enhanced high-latitude cooling in SAI_{50} could potentially provide better compensation for Arctic amplification.

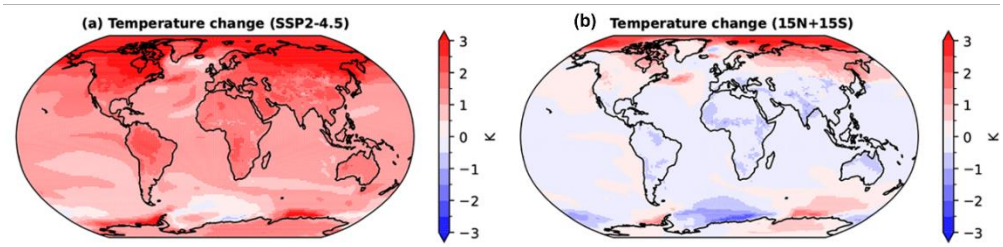

Figure R1. The ensemble-mean temperature change in 2050-2069 relative to the target period (2014–2033) in SSP2-4.5. (Figure A6 in Henry et al., 2024). (a) The surface temperature anomalies for SSP2-4.5; (b) The surface temperature anomalies for tropical SAI strategy (15S/15N).

We add the discussions in the manuscript in Line227-232:
"Additionally, while this study uses idealized fixed-rate injections to compare fundamental differences between injection heights, more practical implementation would require varying injection rates to meet specific climate objectives (Henry et al., 2024; Macmartin et al., 2022; Tilmes et al., 2018b). The enhanced high-latitude cooling observed in SAI_{50} suggests potential advantages for offsetting Arctic amplification, though determining optimal injection strategies would depend on defined climate goals and metrics."

Concerning Arctic amplification, the authors write: "In SAI50, the simulated 22% greater global mean surface cooling compared to the 10% increase in global mean AOD (Fig. 1a), is primarily attributed to Arctic amplification effects (Barnes and Polvani, 2015), with a minor contribution from the reduced stratospheric water vapor enrichment (Fig. 2c-d)." I don't understand this statement. Arctic amplification, depending on the mechanism which causes it in WACCM, should be part of the temperature response in both strategies. Shouldn't part of the difference between SAI50 and SAI25 be due to the different aerosol distributions. Is the idea that polar aerosols create a larger forcing than low-latitude aerosols? Would that be related to the surface-temperature

dependence of stratospheric aerosol forcing as discussed by Hegde et al. (2025). Or to aerosol forcing being more efficient at high than low latitudes? Anyhow, I think it is necessary to physically explain the relatively strong additional global cooling for a relatively weak AOD increase.

Sorry for the confusion. The reviewer is correct that Arctic amplification occurs in both SAI_{50} and SAI_{25} scenarios. The stronger cooling response in SAI_{50} (22% greater global mean surface cooling) relative to its AOD increase (10%) can be attributed to two factors:

- The spatial distribution of aerosols: SAI_{50} has a higher proportion of aerosols at high latitudes compared to SAI_{25}.
- Enhanced efficiency of aerosol forcing at high latitudes: Due to Arctic amplification mechanisms (such as ice-albedo feedback and stable atmospheric conditions), aerosol forcing in the Arctic region produces a stronger cooling effect per unit AOD than at lower latitudes.

This explains why a relatively modest 10% increase in global mean AOD results in a disproportionate 22% enhancement in global mean surface cooling in SAI_{50}.

We clarified in the manuscript in Lines 191-195:

"In SAI_{50}, the simulated 22% greater global mean surface cooling compared to the 10% increase in global mean AOD (Fig. 1a) primarily reflects the higher proportion of aerosols distributed at high latitudes, where Arctic amplification mechanisms enhance the cooling efficiency of aerosol forcing. Arctic amplification processes, including ice-albedo feedback and stable atmospheric conditions (Barnes and Polvani, 2015), contribute to this enhanced regional cooling response. A minor contribution also comes from the reduced stratospheric water vapor enrichment (Fig. 2c-d)."

Figures 3c and 3d show simulated annual cycles of the high-latitude cooling signals. In the Arctic there is a pronounced seasonal cycle, while it is negligible in the Antarctic. This behaviour is just stated but not explained. To develop trust in such signals it is important to explain the physical mechanism causing this difference. Moreover, with respect of the "cooling benefits" discussion it would be important to discuss if these different annual cycles just offset different annual cycles of high-latitude greenhouse gas warming or if seasonal cycles are strongly modified.

The pronounced seasonal cycle of Arctic cooling compared to Antarctic cooling under SAI scenarios reflects fundamental differences in surface characteristics between these regions. The Arctic Ocean's seasonal sea ice plays a crucial role, while Antarctica's permanent ice sheet leads to more stable conditions year-round.

Arctic amplification - the enhanced temperature response in the Arctic region - occurs primarily from October to April and is strongly tied to sea ice loss (Dai et al., 2019). During summer, incoming solar energy is consumed by sea ice melt. In fall-winter, areas where sea ice has

retreated expose open water, leading to increased outgoing longwave radiation and heat fluxes that drive stronger temperature changes. This mechanism explains why both Arctic warming under greenhouse gas forcing and Arctic cooling under SAI scenarios show maximum intensity during October-April.

We have explained this mechanism in the manuscript (Lines 204-207):

"The Arctic cooling exhibits pronounced seasonality, with maximum effects during fall-winter seasons (Fig. 3c). This seasonal pattern aligns with the mechanism of Arctic amplification, which is driven by increased outgoing longwave radiation and heat fluxes from areas of seasonal sea ice loss during October-April (Dai et al., 2019). In contrast, Antarctica's year-round ice cover results in more uniform cooling throughout the year (Fig. 3d)."

SAI effectively offsets the seasonal peak of polar warming, while the simulated seasonal cycles of surface temperature over both poles remain largely unchanged, as shown in Fig. R2.

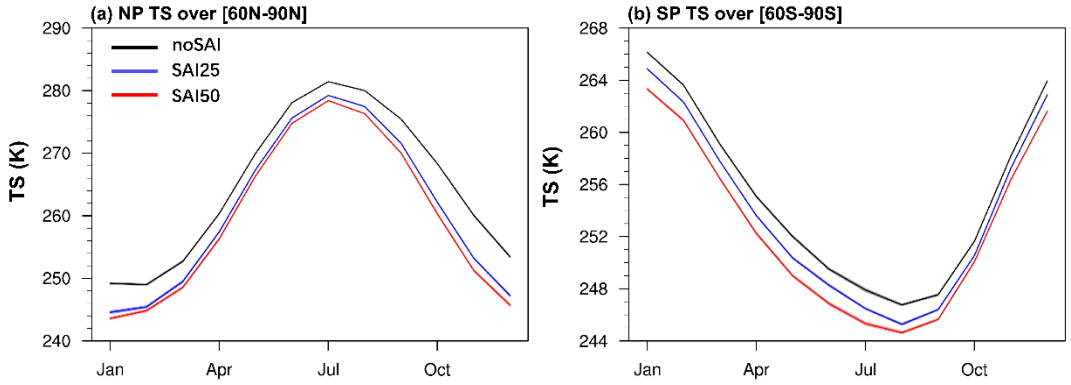

Figure R2: (a) The averaged surface temperature over Arctic (60N-90N). The black line denotes the global warming scenario in 2040. The blue and red lines denote the SAI25 and SAI50 scenarios, respectively; (b) Same as (a) but for Antarctic over 60S-90S.

Finally I see a major issue with the lack of discussion of the additional costs of emitting near the stratopause compared to the lower troposphere. The authors are briefly mentioning the option of using rockets and conclude that the “results clearly indicate that a detailed engineering design study […] is warranted.” I think at least a brief estimation of costs based on existing rockets would be necessary. One could argue that scientifically it is interesting to see the dependence of SAI effects on the injection height. But if feasibility plays no role, why not emit at 70 or 100 km? As the authors claim to “propose a novel SAI approach” I think a minimum effort on estimating feasibility is necessary.

We appreciate the reviewer's comment regarding implementation costs. While cost considerations are indeed important for real-world deployment of any SAI approach, our study represents a theoretical exploration using idealized numerical experiments and focuses specifically on

understanding the physical mechanisms and climate response to stratospheric aerosol injection at different altitudes. This theoretical exploration is essential for advancing our fundamental understanding of stratospheric dynamics and aerosol-climate interactions.

While we acknowledge that implementation feasibility is an important consideration for any proposed climate intervention strategy, a detailed engineering and economic analysis would require expertise beyond atmospheric sciences and would constitute a separate study entirely. Our results provide the physical basis necessary for future interdisciplinary assessments that could then evaluate technical feasibility and economic viability in detail.

RC2

Review by Thomas Peter

General comments

This is an interesting manuscript on a new idea for how climate intervention through stratospheric aerosol injection (SAI) could be implemented by injecting at much higher altitudes (~50 km) than previously proposed. This could lead to significantly less harmful side effects than previous proposals to inject SO2 into the lower stratosphere. This could reduce both the strong warming of the lower stratosphere due to IR absorption by H2SO4-H2O aerosol, which affects climate and precipitation zones in the troposphere, and the depletion of stratospheric ozone. As the authors describe in their manuscript, this new idea could even boost the efficiency of surface cooling.

To my knowledge, this idea is original, and the proposed method could potentially be very important and would fit well into ACP. The authors are to be commended for developing this concept. Unfortunately, however, I do not believe that the technical details necessary to justify the feasibility of this novel method are sufficiently developed for publication in ACP.

Thanks Prof. Thomas Peter for the constructive suggestions, which help to improve the manuscript. Point-to-point responses are marked in blue.

The differences in aerosol formation at an altitude of 50 km compared to 25 km are considerable and would need to be discussed:

(1) The temperatures are so high that the formation of H2SO4-H2O droplets close to injection altitude seems unlikely.

(2) The air density is so low that the fall velocity of particles, if they form, will be very high, which is a major limitation.

(3) The H2SO4 photolysis, which is neglected in this modeling, could massively alter the model results.

These three aspects are not addressed in the submitted manuscript (with the exception of a reference to the fact that H2SO4 photolysis is neglected). However, the issues associated with these aspects are central to such a new proposal and must not be ignored. Therefore, I do not think that the manuscript can be accepted for publication in its present form.

Sorry for the confusion. We think all three aspects stem from one potential misunderstanding that needs clarification:

We inject SO_2 at 50 km, but our simulations show that sulfate aerosol formation occurs at much lower altitudes. Figure R3 shows the simulated zonal and vertical distributions of SO_2, sulfate aerosol, and H_2SO_4 anomalies in both SAI_{25} and SAI_{50} scenarios. In SAI_{25}, the simulated aerosol

peak is located around 25 km in the tropics (the injection region). However, in SAI_{50}, the sulfate aerosol forms at lower altitudes, with peak concentrations below 20 km at high latitudes. The simulated H_2SO_4 anomalies are several orders of magnitude smaller than SO_2 at 50 km. We chose to inject SO_2 at 50 km because the overturning circulation rapidly transports these precursor gases poleward, leading to a more uniform global distribution. In the revised manuscript, we add Figure R3 into the supplement.

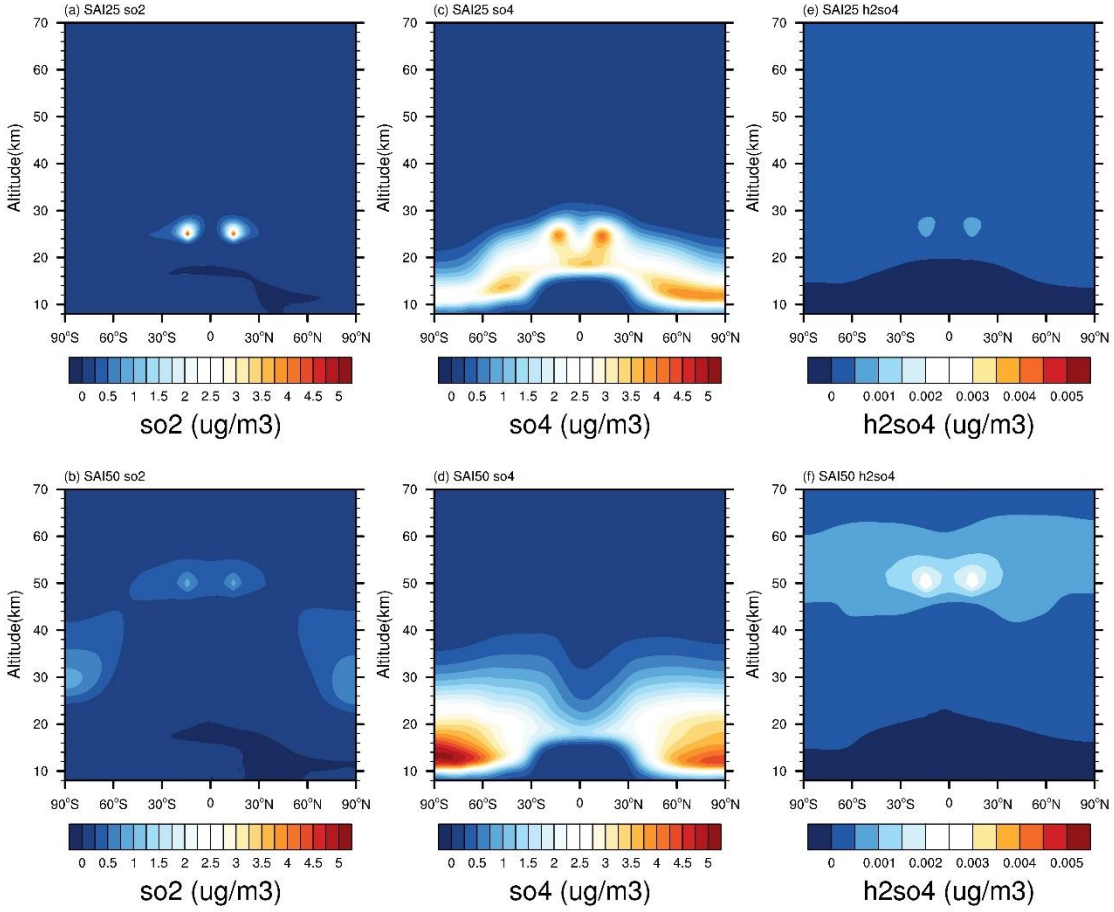

Figure R3: (a) The vertical distribution of the zonal and annual mean SO_2 anomalies in SAI_{25}. (b) same as (a) but for SAI_{50}; (c-d) same as (a-b) but for sulfate aerosol. (e-f) same as (a-b) but for H_2SO_4. Note that the contour range in panel (e-f) H_2SO_4 is 3 magnitudes smaller than sulfate and SO_2.

Detailed responses to each specific aspect are shown below in the Specific Comments section.

Specific comments

In the following, I will first explain my concerns regarding these three points in more detail and then provide a list of minor comments and suggestions for improvement of the manuscript.

Regarding (1): At the stratopause, temperatures range between 260 and 270 K. At such high temperatures, the normal Junge aerosol can no longer exist. The H2SO4 vapor pressure of aqueous sulfuric acid is approximately 10-8 hPa (see Fig. 4 of Carslaw et al., Revs. Geophys., 35, 2, p. 125,1997). At the stratopause this would correspond to 10 ppbv H2SO4 in the gas phase, i.e. about 100 times the total mixing ratio normally present in the lower stratosphere during volcanically quiescent periods. I think the mixing ratio reached by SAI50 would remain lower. Therefore, the nucleation of sulfuric acid particles probably only begins well below the injection height. Figure 1C seems to confirm that. How does the model treat this nucleation? No information is provided about the microphysical code, which for this proposal should be of major concern. I suppose the microphysics is treated by a modal approach, and it would be important to see size distributions at various altitudes.

The reviewer is correct that the formation of sulfate aerosol is difficult at stratopause near 50 km. As shown in Fig.R2, the simulated sulfur-species at 50 km in SAI_{50} scenario is dominated by SO_2, rather than H_2SO_4 gas nor sulfate aerosols. The simulated H_2SO_4 gases is 3 orders of magnitude lower than SO_2 at 50 km in SAI_{50}. Aerosols form when SO_2 gases are transported to higher latitudes and lower altitudes, with peak aerosol concentrations simulated in lower-middle stratosphere (Fig.R3). We added Figure R3 to supplement file (Figure S3), and explained this point in Line 149:

"Note that the sulfate aerosol evaporates into sulfuric acid gas above 35-40 km but reforms when the gas is transported to lower altitudes (10-30 km) via large-scale circulation."

In Line 138-140 :

"…It's important to note that while SO2 is injected at 50 km, the actual sulfate aerosol formation occurs at much lower altitudes (primarily between 10-30 km) due to the rapid transport of precursor gases and more favorable conditions for aerosol formation at lower altitudes. Above 40 km, the simulated stratospheric sulfur species primarily exist in the form of SO2, with ~3 orders of magnitudes higher than H2SO4 (Fig. S3). Above 40 km, the simulated stratospheric sulfur species primarily exist in the form of SO_2, with ~3 orders of magnitudes higher than H_2SO_4 (Fig. S3)."

We also describe aerosol scheme MAM3 in the method section in Line78-81:

"MAM3 provides a physically-based treatment of aerosol size, mixing, and key microphysical processes, including nucleation, growth, deposition, and interactions with clouds and precipitation (Liu et al., 2012). The nucleation of sulfate aerosol is produced from aqueous-phase SO_2 oxidation and to a lesser extent from H_2SO_4 condensation on pre-existing aerosol (Liu et al., 2012)."

Regarding (2): The air is so thin at the injection height that particles with a radius of 100 nm sediment by about 10 km within a month (eyeballed from Fig. 2 of Müller & Peter, Ber. Bunsenges. Phys. Chem. 96, p. 353, 1992). Since this is a fundamental aspect of the proposed injection scheme, it would be necessary to check the model's implementation of this process

carefully and to provide arguments, why this fast sedimentation does not invalidate the whole procedure.

We agree that aerosol sedimentation is faster in thinner air at higher altitudes. However, in our SAI_{50} simulations, the injected aerosols do not remain near 50 km but accumulate around 25–30 km altitude (see Fig. 1c).

We also confirm that CESM-WACCM can represent gravitational settling, and this process has been validated against observational estimates of aerosol lifetimes from major volcanic eruptions, such as Pinatubo and Hunga Tonga (Figure 1a).

Regarding (3): The photolysis of H2SO4 molecules is a central process in this scheme and cannot be ignored without good reason. A quick test with our own chemistry-climate model shows that the amount of condensed H2SO4 in the Junge aerosol layer is 2-4 times higher without photolysis than with photolysis. I would estimate that the reduction of aerosol mass in the upper stratosphere could more than a factor of 10. I must therefore assume that in the modeling work shown, a significant portion of the aerosol is formed solely due to the lack of photolysis in the model.

Following the reviewer's suggestion, we compared the vertical distributions of sulfur-containing species in our SAI simulations. As shown in Figure R3-4, the simulated concentrations of H_2SO_4 are 1-3 orders of magnitude lower than those of sulfate aerosols.

We add H_2SO_4 photolysis into the model and compare the vertical distributions of the sulfur species with and without photolysis. Shown in Fig. R3b, the simulated vertical distributions of sulfur-containing gases and aerosols remain largely unchanged between simulations with and without photolysis. These results indicate that H_2SO_4 photochemistry has a limited impact on the overall sulfur distribution in SAI_{50} mainly because sulfate aerosols form in much lower altitude instead of stratopause, where the precursors SO_2 are transported rapidly by the overturning circulation.

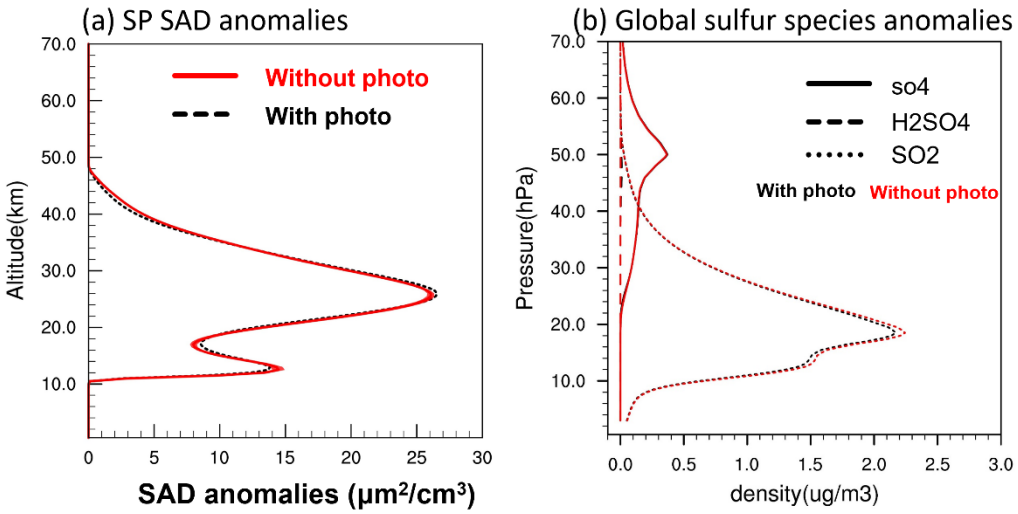

Figure R4. (a) Simulated vertical distributions of the Antarctic SAD with (black) and without (red) H_2SO_4 photolysis (same as Fig 1c red line) averaged from September-October-November (SON). (b) Simulated annual global mean vertical distribution of the sulfur species with (black) and without (red) H_2SO_4 photolysis. The simulated sulfate, H_2SO_4, and SO_2 are denoted by solid, dashed, and dotted line, respectively.

While points (1) and (2) can probably be positively resolved with the existing model runs (showing that the model correctly calculates the partial and vapor pressures of the aqueous sulfuric acid under upper stratospheric conditions and the sedimentation velocity of the aerosol particles formed), point (3) is likely to pose a bigger problem. If photolysis cannot be incorporated into the existing model, at least a very clear warning should be included for the reader that this omission may significantly influence the result and diminish the efficiency of the proposed method.

Please see the point-to-point response above.

Technical comments listed by line number

1. L. 19: “SAI using sulfur cools the planet” à “SAI using sulfur has been proposed to cool the planet”

Done

2. L, 20: Better don’t talk about “traditional” SAI, in particular not in the first sentence of the abstract. All SAI is still in the proposal phase and largely unsubstantiated ideas, nothing traditional.

Revised to be “A commonly proposed SAI, with sulfur dioxide injection rate of 10 Tg/year at 25 km ….”

3. L. 38: In addition to Ferraro et al. (2015) and Visioni et al. (2021), another excellent example making this point is Wunderlin et al., “Side effects of sulfur-based geoengineering due to absorptivity of sulfate aerosols”, GRL, 2024.

   Done

4. L. 66: I do not understand the “positive ozone chemical tendency”.

   We rewrite the sentence in Line 68: “By doing so, high-altitude sulfate aerosols reduce NOx levels, slowing NOx-driven ozone loss and allowing ozone to accumulate in the middle stratosphere, which can offset the ozone loss caused by reactive halogen species in the lower stratosphere.”

5. L. 73: Here I expected more information on the type and characteristics of the microphysical module used in this modelling work

   We also describe aerosol scheme MAM3, in Lines 78-81:

   “MAM3 provides a physically-based treatment of aerosol size, mixing, and key microphysical processes, including nucleation, growth, deposition, and interactions with clouds and precipitation (Liu et al., 2012). The nucleation of sulfate aerosol is produced from aqueous-phase SO_2 oxidation and to a lesser extent from H_2SO_4 condensation on pre-existing aerosol (Liu et al., 2012).”

6. L. 79: I do not understand and do not accept that the fact that the column-integrated stratospheric burden of H2SO4 is much smaller that the burden of sulfate aerosols could be used for not having to take the photochemistry of H2SO4 into account. In the warm upper stratosphere, all H2SO4 is gaseous and exposed to H2SO4 + hv.

   As discussed in previous response, the simulated aerosol peaks in 20-25 km in altitude instead of 50 km. SO_2 is released at 50 km but transported polewards and downwards until sulfate aerosols are formed.

   We delete the sentence in the method section and address the H_2SO_4 photochemistry in the result section in Line 139-140:

   “…, and the simulated stratospheric sulfur species primarily exist in the form of sulfate and SO_2, with ~3 orders of magnitudes higher than H_2SO_4 (Fig. S3)

7. L. 84: “SO2 was continuously injected … with a total rate of 10 Tg per year”. It might be more meaningful to say “with a total rate of 27 Mg per year” to stress the continuous character, or even “with a total rate of 27 tons per year”.

In the SAI experiments, SO_2 was continuously and uniformly injected throughout the year, with a total annual amount of 10 Tg injected per year.

8. L. 87: 5 years of model spin-up plus 15 years of actual model run. This is okay. But then, does Figure 1 show the 5 years of spin-up plus 10 years of model run?

   In the revised manuscript, Figure 1a is extended to 20 years, consistent with 15 years model run with 5 years spin-up.

9. L. 90: I do not understand these scaling factors. Where are they from?

   We rewrite in Line 99-101: "For simulations of year 2000, model is initialized with atmospheric ODS and Greenhouse Gases (GHGs) conditions of year 2000. For simulations of year 2040 (2065), the ODS and GHGs are fixed in the year of 2040 (2065)."

10. L. 103: The "2022 Hunga volcanic eruption". Okay, everybody knows which volcano this is, yet it is a pretty crude abbreviate

    Revised to be "Hunga Tonga-Hunga Ha'apai"

11. L. 110: The "spread is designed to capture" sounds weird. Are you designing a spread or is the spread the result of your simulations?

    Thanks. Revised to be: "The spread across our simulations of 45 ensemble members represents the natural variations in stratospheric circulation."

12. L. 127: Sentence confusing. For clarity, I would rewrite "… are similar for all lower altitude injections (at 20 km, 25 km and 35 km),...".

    Done

13. L. 129: The word anomaly is used abundantly, also when it is just a "change" or even a total number. For instance, in Figure 1a is it really AOD anomaly or just AOD. And why is it in Figure 1C simply "SAD", and not "SAD anomaly"?

    Thanks for the comment. Figure 1a, b, c are all AOD or SAD anomalies. We modified Fig.1c label to be "SAD anomaly". We checked throughout the manuscript.

14. L. 260: "would be" instead "is".

    Corrected

15. L. 295: Where is the dip in SAD (Fig. 1c) at 18 km come from?

    We show the monthly vertical distribution of aerosols and found that the dip in SAD at ~18 km results from the interaction between transport and microphysical processes. Analysis of monthly vertical distributions reveals two distinct aerosol peaks:

- An upper peak (25-30 km) formed by continuous poleward transport of newly formed sulfate aerosols via the upper branch of the Brewer-Dobson Circulation (BDC)=
- A lower peak (~15 km) representing older aerosols that have descended through gravitational settling

During Antarctic spring (shown in Fig.1c), these two separate peaks create the observed dip at ~18 km. This pattern emerges as the BDC continuously brings new aerosols to high altitudes while previously transported aerosols settle to lower levels

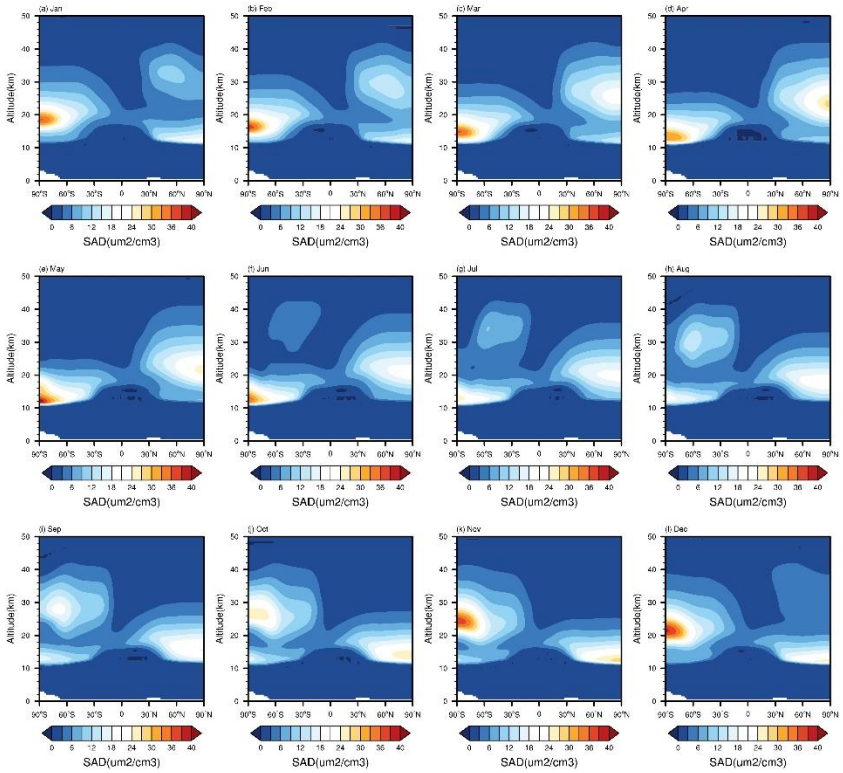

Figure R6. Simulated monthly vertical distribution of SAD anomalies in SAI_{50} from January (a) to December (l) after injection.

16. L. 295: The red curve in Fig. 1c would probably look quite different if H2SO4 + hv was taken into account.

We have conducted additional simulations that include H2SO4 photolysis (H2SO4+hv). The simulated Surface Area Density (SAD) vertical distributions are shown in Figure R3. While H2SO4 photolysis does affect the sulfate aerosol lifecycle, the overall vertical distribution pattern remains similar to our original simulations without photochemistry - the simulated SAD still peaks between 20-30 km. This is because the primary formation of sulfate aerosol occurs at lower altitudes, and photolysis becomes more significant only at higher altitudes where the aerosol concentration is already low.

17. L. 296: Why distinct?

    Deleted

18. L. 301: “Multiple” is not a verb.

    Corrected

19. L. 305: “from ensembles” is slang. “of the ensemble members” would be appropriate.

    Corrected