AUTHORS RESPONSE TO REVIEWER #1

The authors analyze the results of four simulations of different injection scenarios on stratospheric aerosol intervention (SAI), or stratospheric aerosol geoengineering as they say in the title. The area of injection changes from equator to mid latitudes. The simulations are partly new, especially the assumption for seasonally varying injections at 60N and 60S. The other injection strategies are similar to the GeoMip5 scenarios or previous publications. The authors analyses different impacts of SAI on stratospheric and tropospheric dynamics. The analysis of the simulations includes an interesting discussion on climate and tropospheric circulation, e.g. NAO. This important aspect has not been taken into account enough in existing literature. To me, this is the main aspect of the paper. The paper is well written and reads well. I recommend publication after a few minor corrections.
Ulrike Niemeier

We thank the reviewer for the positive review and helpful comments that have improved our manuscript. We address the specific comments below in blue.

General:

As stated above, the impact of SAI on the tropospheric circulation is an important aspect in the discussion about SAI. Other parts, e.g. the impact on the Brewer Dobson Circulations, ozone or temperature have bee discussed earlier. Here the authors should cite broader and discuss their work in relation to previous publications. They have to state clearly which aspects of the analysis are new.

We note that the main GeoMIP experiments always injected SO_2 at the equator, or close by (Visioni et al., 2023, https://doi.org/10.5194/acp-23-5149-2023). While we acknowledge that other previous studies might have analysed some of the four strategies discussed here, none of them compared all of them in a consistent way, and most of them used fixed amounts of sulfur injections and an atmosphere-only model configuration.

Instead, one of the main advances of our study is that we consistently compare four injection strategies that result in similar global mean surface temperature response. Something similar to this was only done in Kravitz et al. (2019) with CESM1 but using only two injection strategies (an equatorial and a multi-latitude multi-objective strategy). We have now made sure we highlight the novelty of our study in the revised manuscript, as well as include more discussion with previous strategy exploration studies such as Franke et al (2021), Weisenstein et al (2022) and Laasko et al. (2022). We also note that we examine some SAI impacts on aspects of the climate system that previously have not been explored in detail in relation to injection strategy.

GeoMIP 5 scenarios used RCP4.5 forcing as well. They showed a rather small signal to noise ration. Therefore, a short discussion why this paper bases SAI on RCP4.5 should be added.

We have now added this – see the response to the specific comment below.

You show mainly yearly averages in the main paper. The POLAR injections depend on season and have, therefore, very different seasonal aspects. This needs to be taken more into account.

The reviewer is correct that it is important to consider not only annual means but also seasonal impacts, especially for changes to the polar vortex. We note that we do, however, already include seasonal mean zonal wind changes in the Supplement, and, we now also added the corresponding seasonal mean changes in temperature. The analysis of the impacts on the modes of extra-tropical modes of variability, i.e. NAM and SAM (Section 4.2 and 4.3), which are directly important for the surface climate changes, is made using December-to-February mean data, i.e. the season when the contribution of top-down changes in the stratosphere is particularly important.

I wonder a little about the title. A discussion on injection strategy is not new. The impact on high latitude tropical circulation and climate are far less discussed in previous literature. You use geoengineering in the title, but not again in the text. Stick to one of both.

We note that we do use the word ‘geoengineering’ in the main text. The beginning of the introduction reads: “Stratospheric Aerosol Injection (SAI) is a proposed solar geoengineering method aimed at temporarily offsetting some of the negative impacts of rising greenhouse gas levels and the resulting increases in surface temperatures”

The choice of this word in the title was made to avoid using the word ‘injection’ twice.

Specific comments:

All Figure:

Increase font size of the legend.

Done.

Introduction:

Please put your work better into relation to previous work. Injection strategies have been discussed before. Why d we need another paper.

Thank you – as noted in the response to the general comment above, we have now made sure we highlight the novelty of our study in the revised manuscript, as well as include more discussion with previous strategy exploration studies such as Franke et al (2021), Weisenstein et al (2022) and Laasko et al. (2022).

Methods:

Line 100: Generates the model a well developed QBO or more a QBO like pattern? The vertical resolution seems to me a bit low for a good QBO.

The reviewer is correct in that the simulated QBO in this model version has some deficiencies compared to observations – we have now added more details about the characteristics of the model QBO to the text.

We note that while a CESM2(WACCM6) version with increased vertical resolution (110 levels instead of 70) exists and produces a more realistic QBO (Garcia and Richter, 2019, doi: https://doi.org/10.1175/JAS-D-18-0088.1), the version is substantially more computationally expensive that the standard 70-layer version used here. As such, we have decided to use the standard version as a trade-off between improved representation of the QBO and being able to simulate more SAI strategies.

Line 104-105: SSP2-4.5 and the period 2035 to 2069 results in a low signal to noise ration. Why this scenario? The world in in 2023 on the 8.5 track.

Our experiments were designed following the considerations outlined in MacMartin et al. (2022). As discussed in Burgess et al. (2020) and UNEP (2021), the SSP2-4.5 scenario is roughly consistent with the Paris Agreement’s Nationally Determined Contributions without increased ambition. We note that in the short-term, most of the SSP scenarios look alike (and temperature is a function of the cumulative emissions, not the instantaneous ones). Nonetheless, we do not plan nor claim to be predicting the future here (and explain our reasoning in depth in MacMartin et al. 2022), but have now expanded the discussion in the text to justify our decision better.

Line 111: An injection altitude of 22 km is high. On the one hand, this kind of study aims a bit on better deployment strategies. On the other hand, the injection altitude would be difficult to do. So, why 22 km?

The injection altitude is actually 21.5 km (we have clarified/changed this in the text). In general, the choice of injection altitude represents a trade-off between larger technological difficulties in case of real-world deployment for higher altitude injections and reduced cooling efficiency for lower altitude injections due to shorter aerosol lifetime and offsetting radiative impacts (as discussed in Lee et al., 2023). As discussed in MacMartin et al. (2022), the 21.5 injection altitude appears plausibly achievable with existing aircraft engines (Bingaman et al., 2020, https://doi.org/10.2514/6.2020-0618).

We note that the injection altitude of 21.5 km constitutes an improvement compared to the previous widely-used CESM1 GLENS SAI simulations, which injected SO_2 roughly at 7 km above the tropical tropopause (Tilmes et al., 2018).

Do you have an ensemble or single simulations?

We apologize for forgetting to mention the number of ensemble members (i.e. 3) per strategy used – we have now added this information.

Line 120: Please, include Fig S1 into the main paper.

As suggested, we have now included it; although for consistency with the rest of the manuscript we include only the panels for the EQ and POLAR strategies in the main manuscript, with the remainder of the strategies in the Supplement.

Line 124: POLAR... highest aerosols concentration... Where? Not in the annual mean.

Yes, as shown in Fig. 1b,f. To be clear, we meant “maximum aerosol concentration” relative to other latitudes; we have changed the text.

3. Annual mean changes......

An annual mean cannot cover the impacts discussed here. The strategy of polar is not annual. I wondered a bit, if some impacts were hided behind the annual mean. Add seasonal means here, the paper will clearly gain.

The reviewer is correct that it is important to consider not only annual means but also seasonal impacts, especially for changes to the polar vortex. We note that we do, however, already include seasonal mean zonal wind changes in the Supplement, and, we now also added the corresponding seasonal mean changes in temperature and point this out in Section 3. As discussed in the response to the corresponding comment below, we prefer to keep the seasonal mean changes in the supplement to avoid detailed large multi-panel figure and lengthy discussions of the contributing factors that may distract from the main conclusions. Further discussion of the seasonality of the zonal wind and temperature responses is found in Section 4.

Line 157: .. discussed in or detail in Zhang....... Please quantify and add a few sentences.

Done – the part now reads: "The particularly strong increase in lower stratospheric water vapour in EQ, up to 75% at 70 hPa, thus contributes to the low efficacy of this strategy (with 21 Tg-SO_2/yr needed in EQ to reach the temperature target, compared to 14 and 16 Tg-SO_2/yr in 30N+30S and 15N+15S, respectively; Section 2.1) that is also caused by the strong tropical confinement of aerosols and their large size (as discussed in more detail Section 2.2 here and in Zhang et al., 2023)."

Line 170: Do I understand this right, your model cannot calculate the RF of sulfate? No radiation double call?

We apologize for the confusion – the CESM model can indeed indirectly calculate the RF of sulfate using a double call to the radiative scheme (as it was done in the study of Visioni et al., 2022, the result of which we use here), but the diagnostics needed were unfortunately not outputted in our simulations.

Line 178-179: Fig 2 does not show a weakening of the gradient. The isolines show 200 K in the topics and 220 in NH. This will not result in a stronger temperature gradient when warming the tropics. Change the plots and/or the discussion accordingly.

We realize we were not clear here that we were referring to the SAI responses (anomalies), and not the total changes. We have rephrased this to "The SAI-induced warming in the tropical lower stratosphere drives an anomalous strengthening of the equator-to-pole meridional temperature gradients near the tropopause and lower stratosphere. This drives an anomalous increase of the subtropical to extratropical stratospheric westerly winds in both hemispheres via thermal wind balance in all seasons and most injection strategies, though more intermittently for the seasonal injection in POLAR (Fig. S4-S5). In the winter and spring hemisphere, especially in the NH, the strengthening of the polar stratospheric jet at ~60° latitude is likely the result of the associated modulation of atmospheric wave propagation and convergence due to the more westerly subtropical winds (Fig. S5; see also e.g. Walz et al., 2023)"

Line 180: Please be more precise. Which jets, where is the westerly response?

We have now clarified this; see the response above.

Line 180pp: This discussion is useless with plots of annual means. Add seasonal plots for the discussion of polar vortex, in case you mean polar vortex as you don't say so. Add seasonal plots in general, esp for POLAR.

We agree with the reviewer that the seasonal mean plots are useful when discussing the behaviour of the polar vortex. We note that we do, however, show the seasonal mean zonal wind changes in the Supplementary material, and we refer to them in Section 3.3.1. See also our response above. Note that the ***responses*** to SAI are similar across seasons, whether the polar vortex is present or not (we have now made sure this is clear in the text, as there are westerly wind responses even in the summer hemisphere with no polar vortex).

Additionally, we have decided to focus on the yearly mean responses in the main manuscript as the analysis is performed for the SAI in the future period minus quasi present day baseline period, and as such detailed assessment of the seasonal wind changes in these SAI strategies needs to take into account not only the SAI-induced impacts, but also long-term changes in other circulation drivers, especially long-term ozone recovery from the reduction in ozone depleting substances; the latter plays a particularly important role during SH spring and summer (Fig. R1 below). As such we prefer to keep the analysis of the seasonal mean responses in the supplement; however, we have now included the corresponding seasonal mean temperature responses to the supplement, and to assist interpretation of the derived changes, we have also added yearly mean zonal wind responses simulated in SSP2-4.5 to Fig. 3c,f for reference.

Line 198: No enhanced gradient in your figure of temperature anomaly.

This text has now been rewritten for clarity.

Line 236: Where do I see 15N+15S? Reference is missing.

Thank you for spotting this – we have now added the missing figure references.

Fig 4: TREFHT?

Corrected – we meant 'T_{as}'.

Fig 6: Precipitation changes between EQ and POLAR seem to be small and mainly over water. Changes over land might be more critical in POLAR.

We are not sure what the reviewer means – precipitation responses in general tend to be small and/or not statistically significant, but here all SAI strategies give rise to some precipitation responses over both land and ocean.

Fig 5.2: Do we really get a good impression from yearly mean data? Changes under POLAR are strong as well and one may oversee important aspects this way.

Figure 5 shows the responses simulated for December-to-February mean (i.e. when the contribution of top-down changes in the stratosphere is particularly important), and this is compared with the yearly mean responses in panels c and f. The comparison yields very similar responses in both cases. Analysis of the responses in other seasons (Fig. R2 below) shows overall similar responses.

Discussion:

This is mainly a summary. A discussion of the results is missing, e.g. which strategy may have stronger impact on land precipitation, monsoon (GeoMIP5 papers) etc. This is a single model study. There are many studies out to discuss shortly how much the results depend on the model.

The reviewer is right in highlighting that results are model dependent. However, while that has been discussed elsewhere, it was always in the context of one strategy for many models. Here, we are discussing strategy differences in one model, which is an important first step before discussing multiple strategies in many different models (which would also be unfeasible). We note that we highlight the need to test these one-model results, and the associated uncertainties, in a multi-model framework in the last paragraph of the summary/conclusion Section 7.

We have also now expanded it to include additional discussion about what might be considered when choosing an injection strategy in terms of impacts, i.e. the need to consider the variety of impacts when evaluating which strategy is most optimal: "We have demonstrated that some of the undesirable side-effects of SAI that have been well established for tropical injections - e.g. strengthening of the NH polar vortex and the resulting positive NAO-like surface response in winter, or weakening of the intensity of the Hadley and Walker circulations - appear to be mitigated for extra-tropical and polar injections. However, additional impacts for these strategies, like enhanced halogen activation on sulfate, changes to SAM or strengthening of the large-scale equator-to-pole gradient in case of the latter (see Fig. 1c in Zhang et al. 2023), need to also be considered, highlighting the complexity and trade-offs in evaluating which strategy is most optimal."

Line 422: Temperature do not increase only in the tropical lower stratosphere.

Corrected.

Line 454: Bendarz(2022b): say a word about the content when cited here. The reader has to open the paper to follow you.

We note that we already explain the content of Bednarz et al. (2022b), and it's relevance for the results in this manuscript, in Section 4.2, though we clarify the wording as shown below::

"Bednarz et al. (2022b) analysed the SAM changes under fixed single point SO_2 injections imposed between 30°S and 30°N in the same CESM2 version, and showed that the SAM response becomes negative under SO_2 injections in the SH as the injections are moved further into the subtropics. That work suggested that this occurs because of the poleward extent of lower stratospheric heating impacting planetary wave propagation in the stratosphere as well as eddy heat and momentum fluxes in the troposphere below. It is thus plausible that the SAM and jet responses in the EQ, 15N+15S and 30N+30S strategies here are largely dynamically driven by the lower stratospheric heating, in a manner consistent with Bednarz et al. (2022b)."

Please sort the reference list. Also, titles are missing.

Done.

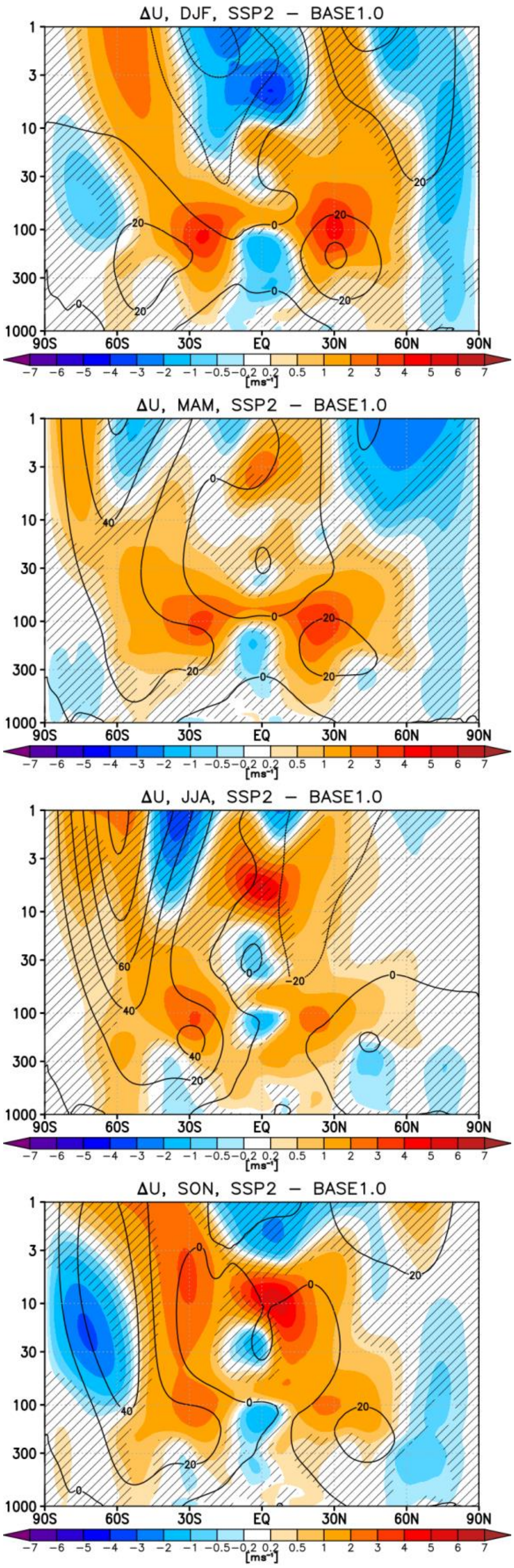

Figure R1. Hatching: seasonal mean zonal wind changes in the control SSP2-4.5 simulation (2050-2069) compared to the quasi present day baseline period (2007-2028). Contours show the corresponding baseline climatology for reference.

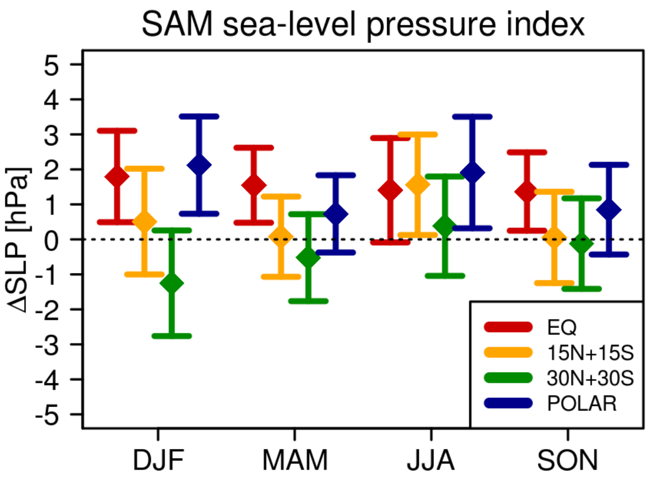

Figure R2. Seasonal mean changes in the SAM sea level pressure index for each SAI strategy compared to the present-day BASE1.0 baseline period. The errorbars denote ±2 standard error of the difference in means.

AUTHORS RESPONSE TO REVIEWER #2

The article explores the effects of different injection strategies for stratospheric aerosol geoengineering on various aspects of the stratospheric and tropospheric circulation. The results are important to highlight the different possible impacts of different strategies which had not been explored before. The paper is well presented and interesting, but I have some concerns as listed below that should be considered before publication.

We thank the reviewer for the positive review and helpful comments that have improved our manuscript. We address the specific comments below in blue.

- Methods: I understand that only one simulation is analyzed for each type of injection strategy. This could be problematic when extracting the forced signal at high latitudes, where the large internal variability can dominate. I would recommend using several members and extracting the ensemble mean response in order to obtain more robust results. If this is not possible I would ask the authors to at least acknowledge the possible uncertainties due to this limitation, in particular for the NH polar vortex response and the surface response.

We apologize for forgetting to mention the number of ensemble members per strategy used – the results are based on 3 ensemble member per SAI strategy and 3 ensemble members of the SSP2-4.5 simulation. We have now added this information to Section 2.1.

- Figure 1 and 2, L179-180: 'increases equator-to-pole meridional temperature gradients near the tropopause and lower stratosphere and thus forces strengthening of stratospheric jets in both hemispheres'

This argument should be rephrased more carefully. In the lower stratosphere temperature is lowest in the tropics except in SH summer. As SAI warms this region, the eq-to-pole gradient becomes smaller in absolute value. This implies a reduction of the (negative) wind shear, thus an acceleration of the wind above the temperature perturbation and a deceleration below. This is seen clearly in Fig. 2 (top left) for EQ. However, the high latitude strengthening of the zonal wind is also linked to the structure of the cooling over the polar cap, which is present in both hemispheres in the lowermost stratosphere in all strategies except POLAR, and also in middle and high latitude in the SH. Could you explain why you get this cooling regions? In general the cooling of the middle and upper stratosphere is quite an outstanding feature in all strategies and it should be discussed as it can influence not only the winds but also ozone.

We realize we were not clear here that we were referring to the SAI responses (anomalies), and not the total changes. We have rephrased this to "The SAI-induced warming in the tropical lower stratosphere drives an anomalous strengthening of the equator-to-pole meridional temperature gradients near the tropopause and lower stratosphere. This drives an anomalous increase of the subtropical to extratropical stratospheric westerly winds in both hemispheres via thermal wind balance in all seasons and most injection strategies, though more intermittently for the seasonal injection in POLAR (Fig. S4-S5). In the winter and spring hemisphere, especially in the NH, the strengthening of the polar stratospheric jet at ~60° latitude is likely the result of the associated modulation of atmospheric wave propagation and convergence due to the more westerly subtropical winds (Fig. S5; see also e.g. Walz et al., 2023)"

We agree that the high latitude strengthening of the zonal wind is also linked to the high latitude lower stratospheric cooling, although we think it is likely the result of the zonal wind changes (and not their driver).

Regarding temperature changes in the mid and upper stratosphere, the cooling diagnosed in this region in the old Fig. 1 is largely driven by the long-term increases in GHG (as we compare a future period with SAI against a present-day period). Since the analysis of the ozone changes in Section 6 is made, purposely, using a comparison of a future period with SAI against the same period of the control GHG simulation, this effect would not influence the ozone results in Section 6.

While we acknowledge that the SAI-induced changes in stratospheric water vapour and BDC discussed in the manuscript will contribute to the cooling simulated in the upper stratosphere (as also discussed in Bednarz et al., 2023, DOI:10.22541/essoar.168563422.29801203/v1), this effect will likely be smaller that the impact of long-term GHG changes. And while reduction in upper stratospheric temperatures acts to increase ozone concentrations, the overall simulated ozone response to SAI in the upper stratosphere is negative and reflects enhanced HOx-mediated chemical ozone loss under SAI-induced stratospheric moistening.

- Another question is why the warming in the lower stratosphere is weaker in the case of polar injection. Is it because the injected material is transported into the troposphere and removed from the atmosphere?

We discuss the reason behind the lower magnitude of the lower stratospheric warming in Section 3.3.1, which we now expanded to read: "We find a strong dependency of the magnitude of the tropical lower stratospheric heating on the SAI strategy, with EQ showing the strongest warming of ~8.8 K at 50 hPa (20°S-20°N) and POLAR showing the smallest warming of ~0.4 K in that region. This can be explained by the spatial distribution of the simulated aerosol cloud, i.e. the amount of sulfate in the tropical lower stratosphere (Section 2, Fig. 1), as well as the average aerosol size (with largest, hence more absorptive, aerosols simulated in EQ and smallest, hence less absorptive, aerosols in POLAR; Fig. 1 and S1)." We have now also added this information into the summary/discussion Section 7.

- It would be useful to add letters to the figure panels.

We agree and have added this to the plots.

- L120: the confinement to the tropical pipe will only work above ~20 km, while below that level there is strong horizontal mixing. It could be interesting (beyond this work) to investigate that case.

We thank the reviewer for the interesting idea! We agree this is something that should be explored in the future studies.

- L110: 'throughout a year' → throughout each year

We rephrased this to: throughout any given year.

- L214-215: 'Warming in the lower stratosphere also reduces the stability of the stratosphere itself, thereby accelerating the deep branch of the BDC.'The BDC is forced by wave driving, so I would expect that thermal changes induce wind changes which modify wave propagation conditions and this drive the BDC. In order to explore this mechanism, it would be good to include the Eliassen-Palm flux divergence in order to examine the associated changes. Also the heat flux plots could be extended in altitude to show the stratosphere.

Thank you for the suggestion. The reviewer is correct, and the simulated BDC changes are indeed associated with the consistent changes in wave driving. We have now added the associated yearly mean EP flux divergence changes to the supplement (Fig. S8), and we include the discussion of these changes into the revised manuscript.

We note that stratospheric heat flux changes show qualitatively similar responses to those inferred from the EP flux divergence changes. As such, we have decided to keep truncating the heat flux plots at 100 hPa so as to focus in this case on the changes in the troposphere.

- L381-382: These conclusions cannot be extracted from the analysis because you are not comparing to the past period as you do for the previous figures, so the reader does not know if the BDC is stronger for SAI than for SSP2-4.5. It would be necessary to include those maps too or at least mention how is the BDC in the reference simulation.

We agree and have now added the BDC response simulated in SSP2-4.5 to Figure 4c,f (grey points).

- L292: typo 'an qualitatively'

Corrected

- L329: typo 'near-air surface' should be near-surface air

Corrected

- L375: the different sign above and below the climatological ozone maximum should be noted (due to opposite-sign vertical gradients)

Thank you - we have now added this.

AUTHORS RESPONSE TO REVIEWER #3

The paper by Bednarz et al. examines the dependence of the climate effects of different injection strategies (specifically geographic position) for SAI, by using the CESM2-WACCM6 model. In particular, they highlight the widely different outcomes in terms of atmospheric circulation and ozone changes, despite using the same surface temperature target. An interesting conclusions of the paper is that polar injections can lead to much smaller circulation changes, although for other metrics (e.g. ozone) the side effects can be more serious than for tropical injections. I think that the paper deserves to be published after some corrections as detailed below.

We thank the reviewer for the positive review and helpful comments that have improved our manuscript. We address the specific comments below in blue.

GENERAL COMMENTS

1. One of the major conclusions of the paper, in my view, is that some of the undesirable side-effects of SAI in the stratosphere that are well established for tropical injections, such as stratospheric moistening, perturbations to the Brewer-Dobson circulation and stratosphere-troposphere coupling as well as some effects in the troposphere (e.g., weakening of the Hadley Cell) are largely reduced with polar injections. A lot of these side effects come from stratospheric heating, which is effectively reduced in the case of polar injections, but the detailed reason why this is the case is only marginally mentioned in Section 3.1 (spatial distribution of the aerosol cloud). I think the discussion of this feature and the quantification of the direct (radiative) heating should be expanded.

We agree and have now added the plots of spatial distributions of anomalous sulfate mass mixing ratios, surface aerosol densities and sAOD for EQ and POLAR that were previously in the Supplement only (Fig. S1) as our new Fig. 1. We have also included panels for the corresponding aerosol effective radius. We now specify the reasons for the different magnitudes of lower stratospheric warming across the strategies more clearly in Section 3.1.1: "This can be explained by the spatial distribution of the simulated aerosol cloud, i.e. the amount of sulfate in the tropical lower stratosphere (Section 2, Fig. 1), as well as the average aerosol size (with largest, hence more absorptive, aerosols simulated in EQ and smallest, hence less absorptive, aerosols in POLAR; Fig. 1 and S1)". We now also repeat these reasons in the summary/discussion Section 7.

We have also now expanded the last paragraph in Section 7 to include what does this mean in terms of impacts and what might be considered when choosing an injection strategy, i.e. the need to consider the variety of impacts when evaluating which strategy is most optimal: "We have demonstrated that some of the undesirable side-effects of SAI that have been well established for tropical injections - e.g. strengthening of the NH polar vortex and the resulting positive NAO-like surface response in winter, or weakening of the intensity of the Hadley and Walker circulations - appear to be mitigated for extra-tropical and polar injections. However, additional impacts for these strategies, like enhanced halogen activation on sulfate, changes to SAM or strengthening of the large-scale equator-to-pole gradient in case of the latter (see Fig. 1c in Zhang et al. 2023), need to also be considered, highlighting the complexity and trade-offs in evaluating which strategy is most optimal."

2. When it comes to the discussion of the different ozone responses to the different strategies (section 6.), a lot of emphasis is given to the dynamical changes (leading to the changes in ozone documented in the paper), while chemical processes (halogen activation on S-aerosols) is only qualitatively discussed, in particular for the case of the polar injections. I think this aspect should be discussed more extensively and if possible, the authors could consider quantifying the chemical contribution to the ozone changes. Also, what about the interaction between SO2, the liquid H2SO4-

H2O aerosols and PSC formation (in particular STS PSCs)? What about N2O5 hydrolysis? Why is the ozone response in the global stratosphere (in particular the upper stratosphere) so much smaller in the case of polar injections, according to Fig. 7? I don't think that dynamics can explain it all. Another key result that is not even mentioned in the text are the sizable tropospheric ozone changes, in particular for the case of polar injections - these can affect the cooling efficiency of the aerosols, too. I think these are all aspects that deserve somewhat more discussion, and they would aid the mechanistic understanding and can better motivate future similar studies with other models.

We agree with the reviewer that too much emphasis was placed in our manuscript on the dynamical drivers of the simulated ozone responses, and not enough mention was given to the chemical changes. We have improved on this in the revised version of the manuscript.

Amongst other, we have added information on the SAI impacts on N_2O_5 hydrolysis and, thus, chemical ozone loss in the mid stratosphere, with the associated changes in active nitrogen species (NO_x) simulated in each SAI strategy added to the supplement (Fig. S16). With that it becomes clear that it is both the much smaller circulation response as well as the absence of significant changes in NO_x in the middle stratosphere that explains the absence of significant middle stratospheric ozone changes in POLAR (as pointed out by the reviewer). We have also added the associated changes in active halogen species (ClO_x and BrO_x) to the supplement (Fig. S16), and clarified that significant changes in halogen activation are simulated under all strategies due to a combination of halogen activation on sulfate itself and increased formation of STSs and PSCs inside the stronger and colder polar vortex.

While we fully agree that the detailed quantification of contribution of individual dynamical and chemical processes to the simulated ozone changes, and their dependence on injection strategy, is an important aspect to consider, we prefer to keep it as beyond the scope of our, already lengthy, manuscript. Instead, we have now highlighted in the manuscript the need to quantify these contributions and assess the associated uncertainties (especially in a multi-model framework), and plan to undertake this analysis in the future.

We also now mention the tropospheric ozone changes in the manuscript.

4. Most of the relevant literature is included in the paper, but some additional recommendations are given below to put the present paper (even) more into context, in particular with respect to the stratospheric water vapor feedback (e.g. Banerjee et al., 2019). Also, more comparisons with other papers showing the impacts of the injection location could be done (e.g., Weistenstein et al., 2022).

Thank you – we have now included these references in the revised manuscript.

5. The paper never discussed microphysical changes arising from tropical vs polar injections. In particular, the size distribution changes depending on the injection latitude could be another very interesting aspect to document... as that could also help the reader understand the lifetime / cooling efficiency depending on the injection strategy. This is a model with interactive microphysics so I guess this aspect could be studied?

The reviewer is correct that this is an interesting and important consideration here. However, the reason we have not thoroughly discussed microphysical changes, in particular the differences in the simulated aerosol size distributions, in these simulations, and their relationship to aerosol lifetime and cooling efficiency is that this aspect is considered explicitly in our accompanying study by Zhang et al. (2023, currently in review). But, as also noted in the response above, since we agree with the reviewer that this aspect is directly relevant for the results and conclusions of this paper, we have now added the plots of spatial distributions of anomalous sulfate mass mixing ratios, surface aerosol densities and sAOD for EQ and POLAR that were previously in the Supplement only (Fig. S1) as our new Fig. 1. We have also included panels for the corresponding aerosol effective radius.

SPECIFIC COMMENTS

L26-36 General comment about the abstract: it reads a bit qualitative, especially towards the end. It would be nice to give a "sign" of the changes when it comes to the latitudinal dependency of the outcome. Could we provide some insight into the general pattern that is arising (i.e. polar injections leading to more of X, tropical injections leading to more of Y).

We agree and have added the following: “These impacts tend to maximize under the equatorial injection strategy and become smaller as the aerosols are injected away from the equator into the sub-tropics and higher latitudes.”

L64 Another paper that is worth citing here is Banerjee et al., 2019, as well as (most recently) Nowack et al., 2023; they provide a more up-to-date assessment of the sWV feedback across CMIP5 and CMIP6 models.

Thank you! We have now added these references.

L87-88 It would be good to highlight the novely over the "feedback" mechanism documented for GLENS, as those papers are what most of the community (even the non-SAI crowd) is most familiar with.

Good idea – we have now added this information.

L122 The 30N-30S injection points were also tested in Weistenstein et al., 2022 - who compared region vs point injections... and also came to similar conclusions, i.e. that injecting just outside of the tropical pipe leads to more uniform aerosol distributions in the global stratosphere... and this was tested across 3 fully independent aerosol CCMs. It might be worth highlighting the consistency with that study.

We agree and have now noted the agreement with the Weisenstein et al. study in the text.

L126 I would have not expected that injecting at the equator or 30S/N would make such a difference in terms of "average sizes", given that the coagulation time-scales (and condensational growth) are quite a bit smaller than the typical transit times in the BDC (6-12 months). Unless the tropical injections lead to aerosols that are more "tropically" confined. Have the authors verified this?

The fact that tropical injections confine the aerosols (and H_2SO_4) more, allowing for further condensational growth, is something that has been demonstrated before in other works. Visioni et al. (2018, https://doi.org/10.5194/acp-18-2787-2018) highlighted the effect of the QBO phase in the condensational growth of the aerosols in the tropics, due to increased confinement (see Figure 7 therein). With the same climate model with sectional aerosol microphysics, this has also been postulated to be the cause behind somewhat larger effective radii in the case of tropical volcanic eruptions in Pitari et al. (2016, https://doi.org/10.3390/atmos7060075, see Fig. 11 therein). This has also been highlighted before in CESM1 (in Visioni et al., 2020, https://doi.org/10.1029/2020GL089470, see Supplement). If the stratospheric air is more confined, continuous injections tend to inject SO_2 in a gridbox that already has higher sulfate aerosol concentrations, favouring condensation (and thus, larger particles) over coagulation (which is why this effect is not observed if injections are not all year round, as e.g. in Visioni et al. (2019, https://doi.org/10.1029/2019GL083680)). We have now expanded this discussion in the manuscript to make sure this point is better captured.

L145 This is a crucial point which deserves more attention, as many of the downstream effects depend on this. I think this deserves a more detailed discussion, especially with respect to the radiative contribution to this signal.

We agree – this part now reads: “We find a strong dependency of the magnitude of the tropical lower stratospheric heating on the SAI strategy, with EQ showing the strongest warming of ~8.8 K at 50 hPa (20°S-20°N) and POLAR showing the smallest warming of ~0.4 K in that region. This can be explained by the spatial distribution of the simulated aerosol cloud, i.e. the amount of sulfate in the tropical lower stratosphere (Section 2, Fig. 1), as well as the average aerosol size (with largest, hence more absorptive, aerosols simulated in EQ and smallest, hence less absorptive, aerosols in POLAR; Fig. 1 and S1).” We now also reiterate this point in the summary/discussion Section 7.

L162 I strongly recommend using the more up-to-date value from Banerjee, of 0.22 W/m2 (Fig.5), which is based on many more models than the 2 papers cited here.

Thank you – we have added this.

L264-265 could this teleconnection be related to an ENSO-like response to SAI in these runs?

That is right – we now clarify this in the text.

L290 I would expect aerosols to have actually the smallest impact on albedo over Antarctica, given the very reflective underlying surface (high surface albedo all year around). Hence, I do not think polar injections really lead to any "reduced (net) summer isolation" changes over Antarctica... and if anything, these changes in insolation would be reflected by the snow/ice of the surface. Can the authors comment on this?

We have rephrased this to:

“The SH high latitude responses in POLAR on the other hand, where the SAI direct impact is largely focused in the mid- and high latitudes in austral summer (Fig. S1, bottom), is likely primarily driven by the cooling of the Antarctic region caused by the reduced summer insolation under SAI and the subsequent changes in meridional heat transport (in a manner analogous to that inferred for the Arctic in Lee et al., 2023), thereby forcing changes in the SH tropospheric winds and sea level pressures.”

As illustrated in Fig. R3 below, POLAR does indeed show significant summer cooling of the Antarctic region, although we agree that the contribution of different drivers (i.e. direct cooling of Antarctic surface vs cooling of the Antarctic region and the subsequent changes in meridional heat transport) is unclear here, but should be explored in the future.

Equations 1-2 I think the mathematical symbols do not match those mentioned in the main text.

Thank you for spotting this – now corrected.

L325 Can the authors briefly explain why the Hadley Cell weakens under SAI? What is the underlying mechanism?

We note that we do already explain the potential mechanism behind the changes in the intensity of both the Hadley and Walker Circulations in the next paragraph (second paragraph of Section 5.2): “This behaviour likely arises because of the combination of how much cooling occurs in each strategy in the tropical troposphere (compared to higher latitudes) as well as to the strength and meridional extent of lower stratospheric heating. The latter increases tropospheric static stability and thus reduces tropical convection, thus adding on to the decrease in the intensity brought about by the purely thermodynamic considerations."

L403-405 as mentioned in one of the main comments, I think this could be expanded and perhaps be analyzed more quantitatively.

We have now added the associated changes in active halogen species (ClOx and BrOx) to the supplement and include more discussion of various processes driving the enhancement of halogen activation in the different strategies.

Similarly, the sizable tropospheric ozone changes should be discussed, as they might have important implications for tropospheric chemistry and/or air-quality.

We agree and have included the following text into the manuscript:

"Apart from impacting UV transmittance, these lower stratospheric ozone reductions also markedly reduce tropospheric ozone concentrations simulated in the SH mid and high latitudes, as less stratospheric ozone is brought down to the troposphere, with potential consequences for the aerosol cooling efficiency, tropospheric chemistry and air quality."

L474-479 I think it might be good to be a bit more quantitative here, or at least give some information concerning the "direction" of the changes and if some coherent pattern emerges, concerning the advantages/disadvantages of each injection strategy (polar vs tropical).

We have rephrased this part to read:
"In the SH, while ozone columns increase in the subtropics for EQ and 15N+15S, in the mid and high latitudes all strategies show reductions in total column ozone (ranging from 13-22 DU over the Antarctic in the annual mean). These are likely driven by the combination of processes, including an enhancement of chemical ozone loss from halogen activation - either on sulfate itself or on increased STSs and PSCs concentrations inside stronger and colder polar vortex - and a reduction in ozone transport inside the strengthened polar vortex, the contribution of which varies depending on the SAI strategy used. Our results thus underscore the need for more research in quantifying the contributions of individual drivers of SAI ozone response as well as in narrowing the associated uncertainties, in particular in a multi-model framework."

As discussed in the response to the general comment above, while we fully agree that the detailed quantification of contribution of individual dynamical and chemical processes to the simulated ozone changes, and their dependence on injection strategy, is an important aspect to consider, we prefer to keep it as beyond the scope of our, already lengthy, manuscript. Instead, we have now highlighted in the manuscript the need to quantify these contributions and assesses the associated uncertainties (in particular in a multi-model framework), and plan to undertake this analysis in the future.

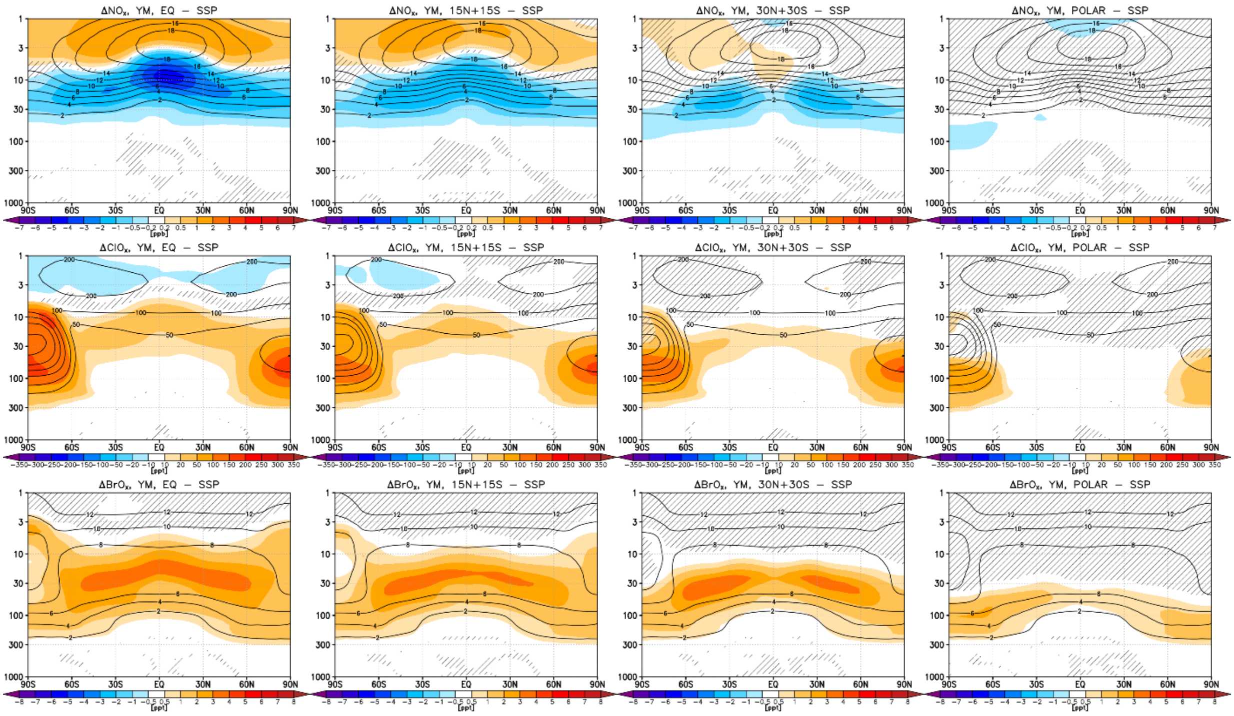

Figure S14. Yearly mean changes in (top) NO_x [ppb], (middle) ClO_x [ppt] and (bottom) BrO_x [ppt] simulated in each of the SAI strategy (columns) compared to the same period of the SSP2-4.5 simulation. Contours show the values in the corresponding values in SSP2-4.5 for reference.

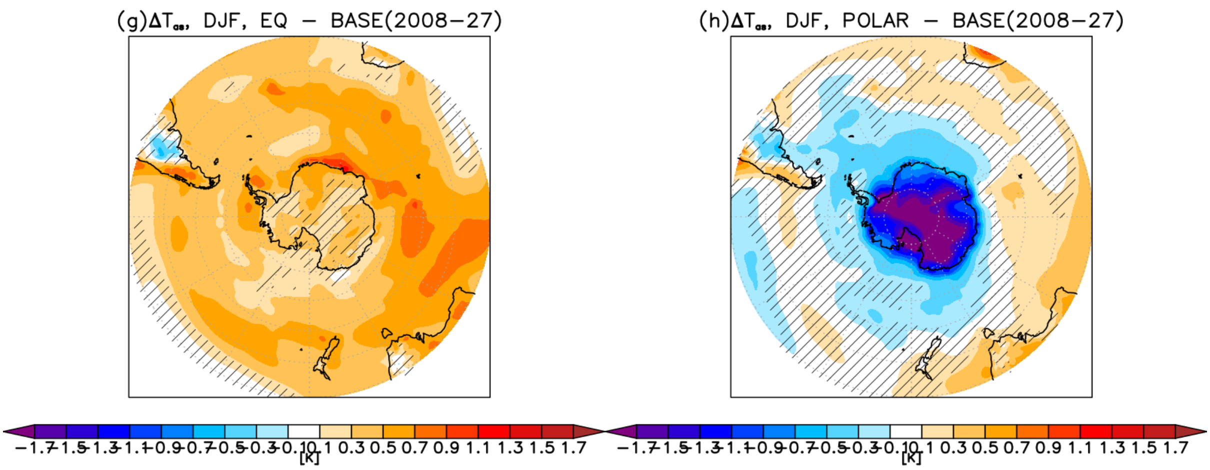

Figure R3. DJF mean changes in near-surface air temperature southward from 60°S in (g) EQ and (h) POLAR compared to the baseline period.