Author Responses to Reviewer #3's Comments

Original referee comments are in italics in black

Author responses are in blue

The manuscript presents an inter-comparison of different comprehensive set of stratospheric aerosol injection (SAI) strategies with the background emission scenario from the Shared Socioeconomic Pathway (SSP) 2-4.5 using WACCM climate model experiments. The manuscript evaluates the injection rates as well as the impact of SAI on near-surface air temperature, precipitation, Arctic sea ice, ITCZ, AMOC and tropical cyclone frequency. The information is very useful as the world is slowly acting to meet the Paris agreement on time to avoid severe climate impact and hazards. Although the manuscript contains some interesting material, which should be published, it could be significantly improved qualitatively in some parts (introduction and results). Some paragraphs and sections are poorly discussed, therefore, they need to be revised by enhancing the discussion about the scientific content, the structure of results presentations as well as combining certain figures to ease the understanding of the manuscript findings and to improve the quality of the manuscript. Particularly, the precipitation differences are overlooked. 30% changes of precipitation in keys regions such as Amazonia forest and Congo basin will significantly impact wildlife and flora in these region as well as the forest ability to absorb atmospheric CO2 as SAI has zero effect on CO2 removal. The precipitation changes overland are much important to investigate because food security, agriculture and so many others vital component for human survival.

We thank the reviewer for their helpful comments; in addition to the specific responses below we will carefully go through the manuscript to clarify presentation; we have also added emphasis and figures in supplementary material regarding the precipitation changes in key regions.

I recommend major revisions. In the following here are my major and specific points as well as general concerns:

Major points:

1. *The surface climate response to different SAI strategies is present with not much caution know the role of the impact of model inter-annual variability on the distribution of SAI into the stratosphere as well as its feedback on surface climate. According to Bittner et al (2016), one need 7 ensembles in the tropics and 40 ensembles in the extra-tropics to capture accurately model circulation response to SAI, therefore, the related feedback to surface climate. There is a need to be caution on how to discuss the findings here. More than 3 ensembles very like needed to constrained model internal variability.*

   We appreciate the reviewer's concern as to the role of model internal variability in the inferred responses. However, we believe that we acknowledge and account for the uncertainty in the diagnosed responses. We examine a response to a continuous SAI forcing, with 20-years of data per ensemble member (so 60 years in total for a single SAI strategy). The Bittner et al. study examined the vortex response to a Tambora eruption (so an instantaneous aerosols forcing) during a single year after the eruption; as such they required a much larger number of ensemble members to confidently diagnose the response.

While increasing the number of ensemble members will improve the estimate of the forced signal, when a difference between two strategies is large enough, we may not need more than one ensemble member in order to show that the difference in the responses between these two strategies is statistically significant and to explain the underlying mechanism. For example, the equator-to-pole temperature gradient (T2) in response to 60N+60S is notably different from T2 in response to other strategies; in this case, one ensemble member would be sufficient to show the difference in T2 between 60N+60S and other strategies. This notable difference is due to the offsetting of arctic amplification by providing more cooling at high latitudes.

We note the main purpose of our study is to introduce a set of novel SAI strategies and provide an overview of some of the main differences and similarities. As such, we chose to optimize the usage of computing time and simulate as many strategies as possible by running three ensemble members per each strategy, a compromise we believe is acceptable in this case.

2. The manuscript overlooks the impact of SAI strategies on precipitation and ITCZ, particularly in key region such as Amazonia and Congo Basin, which are key regions for human. Such as "the difference is no more than 30% (page 14, line 295)" are misleading regarding the interpretation of the SAI strategies on precipitation. Amazonia is responsible of 30% of oxygen production on Earth and is estimated to absorb some 2 billion tons of CO2 per year, meaning that it soaks up about 5% of the world's total carbon emissions. The peat swamp forest of the Congo Basin stores around 29 billion tons of carbon, e.g. approximately equivalent to three years' worth of global GHG emissions, while the Basin as a whole absorbs nearly 1.5 billion tons of CO2 a year. Therefore, I recommend to add two specific figures (like figure 1d) of precipitation changes under SAI strategies and SSP2-4.5 scenario for the Section 4.3.2. Precipitation

We thank the reviewer for their comments. We have modified the sentence on Line 295 as "For the corresponding changes in precipitation over the whole Earth surface (i.e. both land and ocean), the difference in rms P-E response over land is no more than 30% when comparing any SAI strategy with the SSP2-4.5 case (Fig. 8(b)). Although the difference in these global metrics between two strategies might look small, differences in the regional changes could be quite important and need to be evaluated individually".

We have added precipitation plots for the Amazon Basin and Congo Basin and a discussion of the results into Section 4.4. We also add regional precipitation maps for Amazon Basin and Congo Basin in the Supplementary Material.

The following paragraphs are added to the section describing regional precipitation responses.

“In this section, we focus on the Amazon Basin and Congo Basin in particular, to show that different strategies have different impacts on regional precipitation. For the Amazon Basin, we average precipitation over the region between 5N - 15S and 50W - 78W (a total land area of 7.2x10^6 km^2). For the Congo Basin, we average temperature over the region between 8N - 10S and 12E - 31E (a total land area of 4.6 x10^6 km^2). Precipitation changes in these tropical river basins have direct effects on local ecosystems. Rainforests in both regions act as carbon sinks and are thus of great importance to global climate. It is well studied that El Niño Southern Oscillation (ENSO) is one of the main drivers of interannual variability in convective precipitation over the Amazon Basin (Marengo and Espinoza, 2016; Jiménez-Muñoz et al., 2016). Precipitation over the Amazon Basin is suppressed during El-Niño events and enhanced during La Niña events (Marengo and Espinoza, 2016; Jiménez-Muñoz et al., 2016).

In the Amazon Basin, the 20-year average (2050-2069) under SSP2-4.5 is similar to the reference level (Fig. 12(a)), though with regional variations within the basin; the central region becomes drier while the southeast area gets wetter (see precipitation map in the supplementary material). All SAI strategies result in a reduction in the mean precipitation, except for the 60N+60S case (which is not statistically significantly different from either the reference or the SSP2-4.5 case). The multi-objective strategy yields the strongest precipitation reduction. The hemispherically-symmetric strategies show a dependence of the precipitation reduction on the latitude of injection, with the largest decrease in the Amazon Basin precipitation in EQ and no statistically significant decrease in 60N+60S. This pattern of precipitation changes is likely related to the corresponding changes in the intensity of the tropospheric Walker Circulation and, thus, ENSO response, as also discussed in Bednarz et al. (2023). We approximate the ENSO changes by calculating the ENSO index as a difference in near-surface air temperature between the Nino 3.4 region (5N-5S, 120W-170W) and all tropical oceans (20N-20S), based on the method described in Oldenborgh et al. (2021). The strength of the Walker Circulation is approximated by the difference in sea-level pressure between the East Pacific Ocean (5N-5S, 80-160W) and the Indian Ocean (5N-5S, 80-160E), based on the method described in Kang et al. (2020). Figure S7 in the supplementary material shows that both changes in the Nino 3.4 index and the strength of Walker Circulation partly explain the change of precipitation in the Amazon Basin, with the coefficient of determination (R^2) of the best-fit linear regression functions equal to 0.62 and 0.66, respectively.

In the Congo Basin, the average precipitation in the SSP2-4.5 scenario increases over time (Fig.12(b)), likely as the result of the intensification of the global hydrological cycle under increasing surface temperatures (Section 4.1). In contrast, all SAI strategies result in a reduction in the mean precipitation in the Congo Basin compared to the SSP2-4.5 case as the global mean surface temperatures are reduced to around the reference level (Fig. 1(a)). While the multi-objective strategy brings the 20-year average (2050-2069) mean precipitation back to the reference level, other strategies either undercompensate or overcompensate the precipitation. The equatorial and 15N+15S injection strategies result in statistically significant undercompensation of the Congo Basin precipitation compared to the reference period, while 30N+30S and 60N+60S result in a small overcompensation. The dependence of the precipitation reduction in the Congo Basin on the latitude of aerosol injection is partly indicative of the corresponding impacts from the intensity change of the tropospheric Hadley Circulation. As shown in Bednarz et al., 2023, Hadley Circulation weakens significantly under EQ and 15N+15S strategies but stays unchanged for 30N+30S and 60N+60S; these tropospheric circulation changes could thus contribute to and partially explain the precipitation changes simulated in the Congo Basin across the strategies."

List of references:

1. Marengo, J. A. and Espinoza, J. C.: Extreme seasonal droughts and floods in Amazonia: causes, trends and impacts, International Journal of Climatology, 36, 1033–1050, https://doi.org/https://doi.org/10.1002/joc.4420, 2016.
2. Jiménez-Muñoz, J. C., Mattar, C., Barichivich, J., Santamaría-Artigas, A., Takahashi, K., Malhi, Y., Sobrino, J. A., and Schrier, G. v. d.:Record-breaking warming and extreme drought in the Amazon rainforest during the course of El Niño 2015–2016, Scientific Reports, 6,33130, https://doi.org/10.1038/srep33130, 2016.

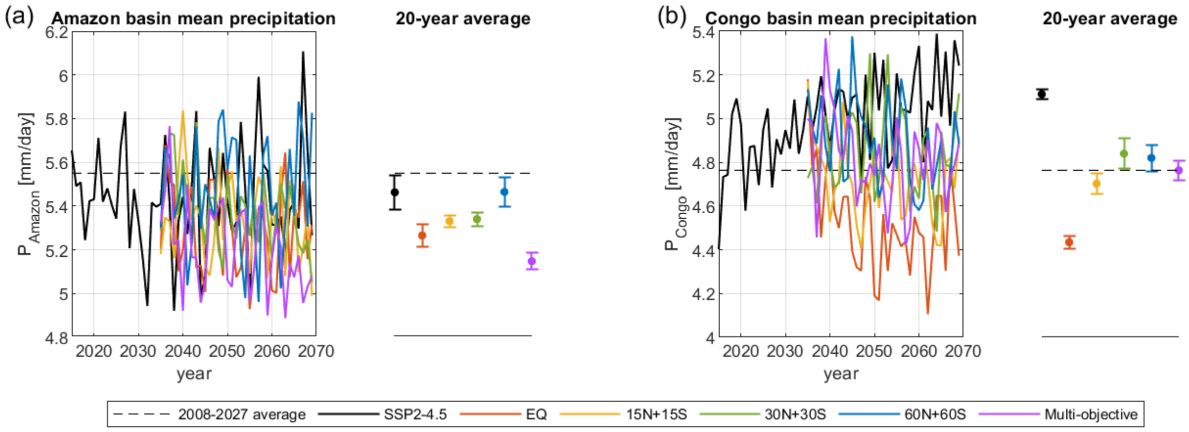


Fig 12. Time evolution of mean precipitation in (a) Amazon Basin and (b) Congo Basin. Each solid line represents the ensemble mean of each injection strategy. The dashed line represents the 20-year average during the reference period (2008--2027). The dots on the right of each panel represent the 20-year average over 2050--2069; the uncertainties in the calculated 20-year averages are estimated by ±1 standard error, and represented by the error bars.

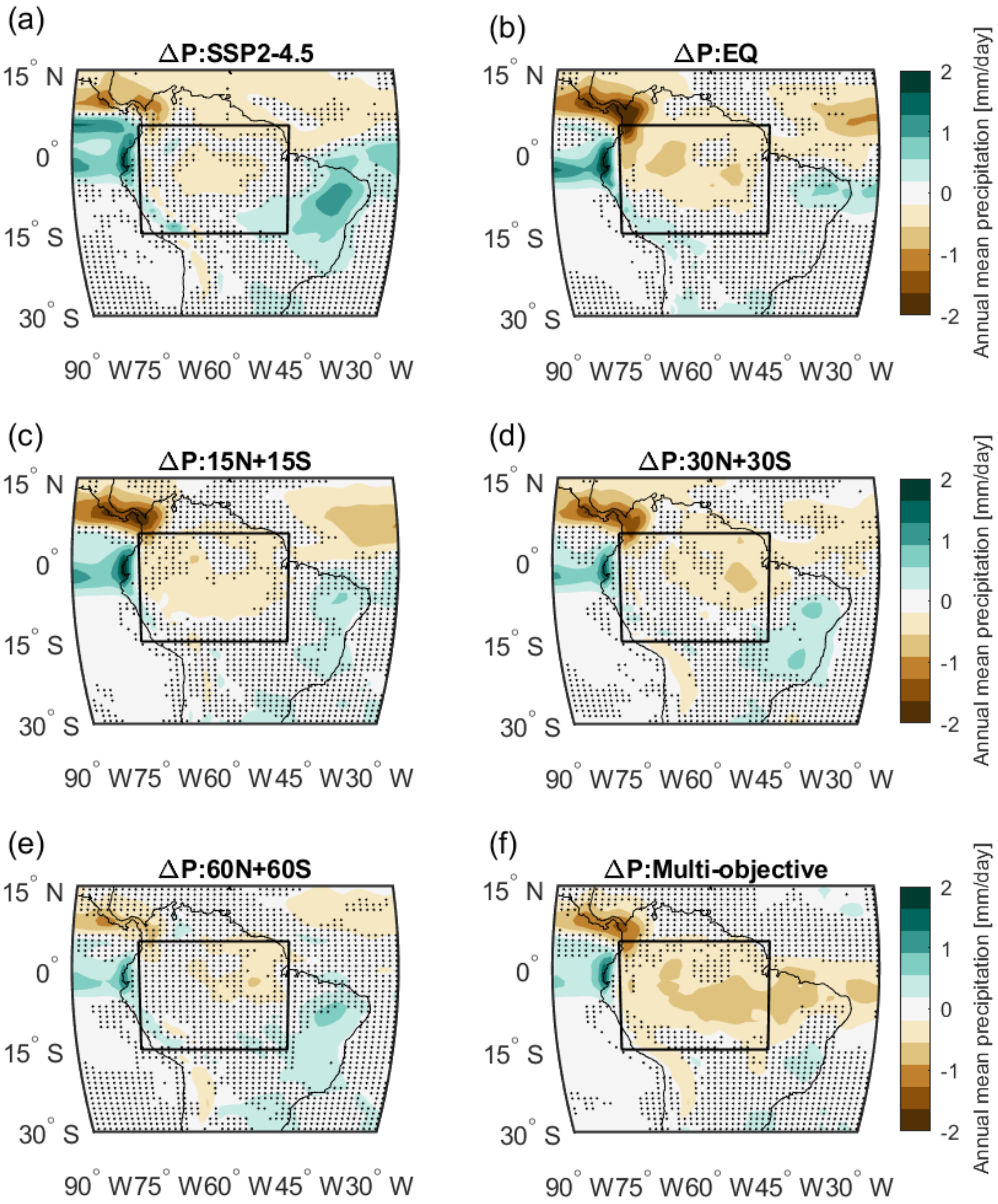


Figure S5. Changes in precipitation (averaged over 2050–2069) in Amazon Basin compared to the reference period (2008–2027) for (a) SSP2-4.5 and (b)-(f) different SAI injection strategies. Shaded areas indicate where the response is not statistically significant based on a two-tailed Welch's t-test with a confidence level of 95%.

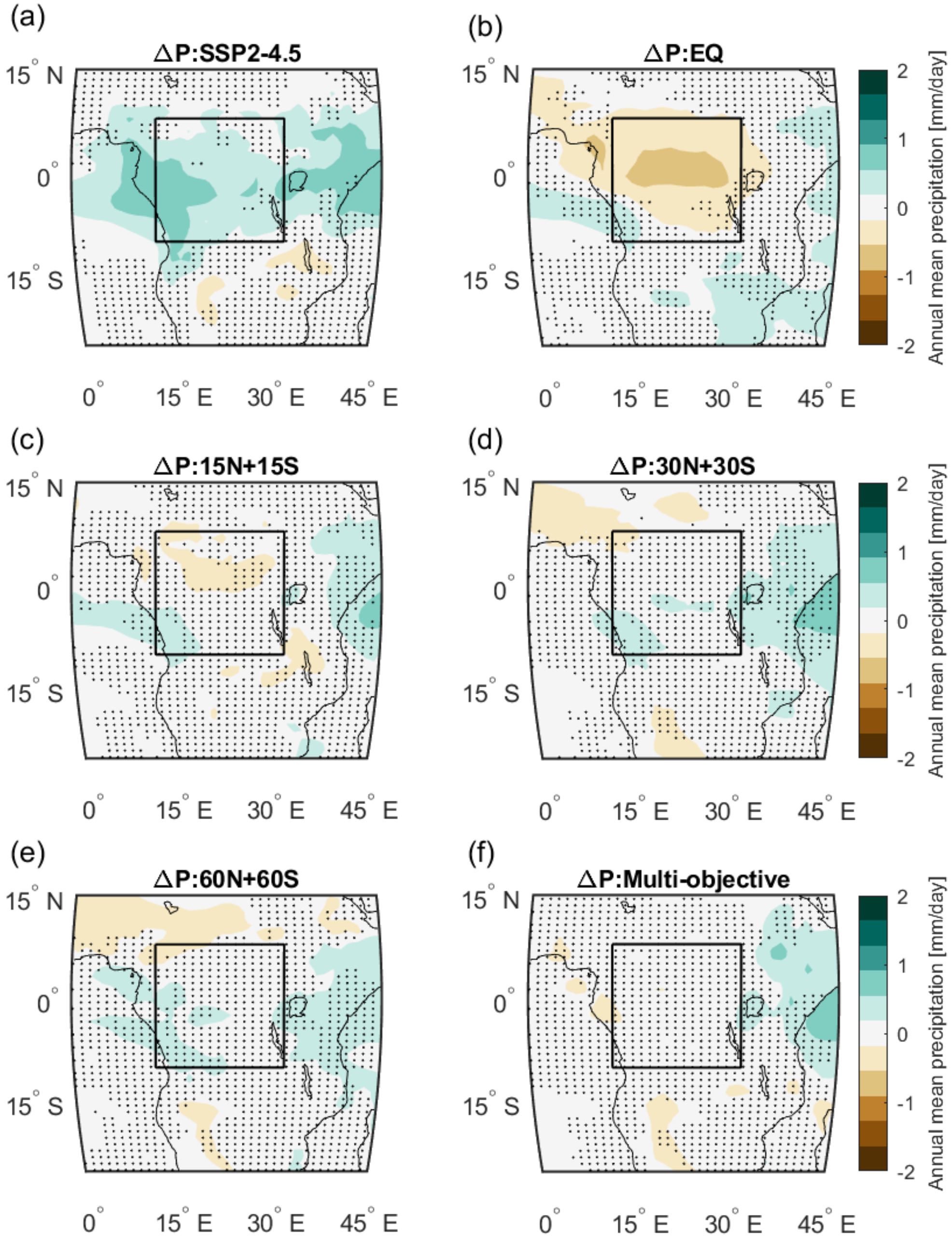


Figure S6. Changes in precipitation (averaged over 2050–2069) in Congo Basin compared to the reference period (2008–2027) for (a) SSP2-4.5 and (b)-(f) different SAI injection strategies. Shaded areas indicate where the response is not statistically significant based on a two-tailed Welch's t-test with a confidence level of 95%.

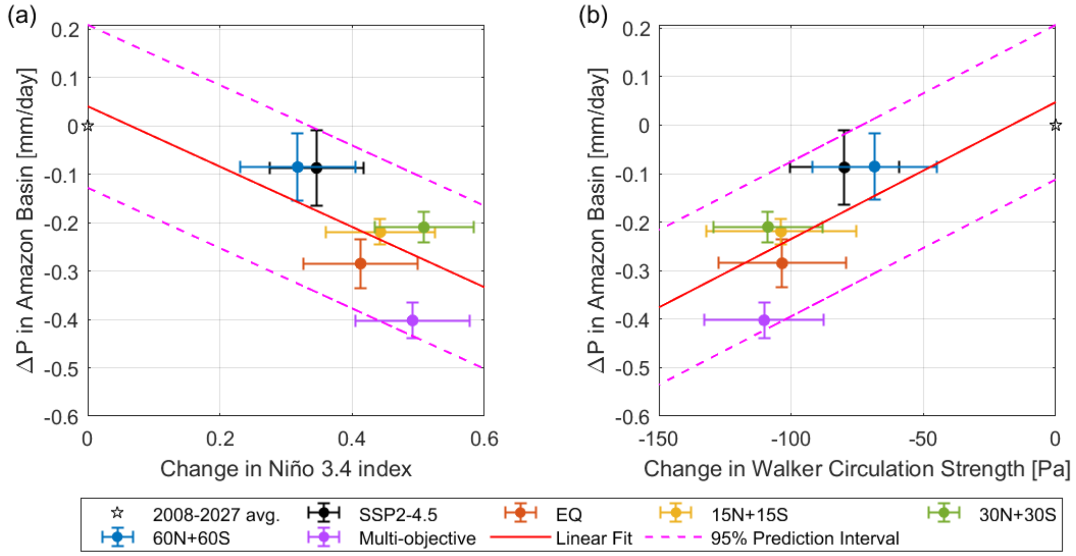


Fig S7. Correlation of change in precipitation in the Amazon Basin with (a) change in the Nino 3.4 index and (b) change in the strength of the Walker Circulation. Precipitation in the Amazon Basin is calculated as the average over the land region between 5N-15S and 50-78W. Nino 3.4 index is calculated as the difference in near-surface air temperature anomaly over the nino 3.4 region (5N-5S, 120-170W) and near-surface air temperature anomaly over all tropical oceans (20N-20S). The strength of Walker Circulation is calculated as the difference in sea-level pressure between the East Pacific Ocean (5N-5S, 80-160W), and the Indian Ocean (5N-5S, 80-160E). The change in these metrics is calculated as the difference between a 20-year average (2050-2069) and the reference period level (2008-2027). The error bars represent the standard error of the mean.

3. *The discussion on the regional and global impact is mingled, therefore, I would like to suggest to restructure the results section 4 as following:*

   a. *4. Results*

      4.1 Large-scale g…..

      4.2 Injection rates and…

      4.3. Global surface climate response (fig 7 and fig 9)

      4.4. Regional surface climate response

   b. *Please reorder the subsection as the following. After the “precipitation minus evaporation” section as well as “ITCZ”, please discuss “tropical Cyclone frequency” and then followed by “SSI” and “AMOC”.*

We thank the reviewer's suggestions on the results section, and have made the changes accordingly.

4. Regrouping several figures is necessary here to ease the clarity and understanding the result better. Figure 8 and Figure 10 need to be put together.

We thank the reviewer for the comment. However, we don't think those two figures should be combined, as Figure 8 shows the rms changes of temperature and precipitation while Figure 10 shows ITCZ.

5. Moving most of the figures in the appendix into the main manuscript is necessary for clarity.

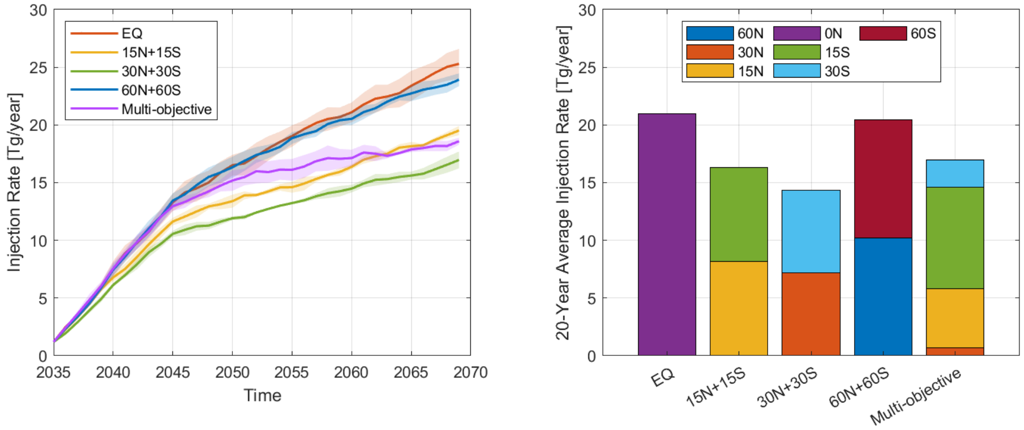
We thank the reviewer for this comment. We have moved most figures in the appendix to the main paper, including combining what was Fig. A6 with Fig. 10 (on ITCZ). We have moved Figure A3-A5 to Supplementary materials.

Minor points:

1. Page 2, line 51-58, this "the differences in surface climate responses between some SAI strategies are much easier to detect than between others" needs to be rephrase each strategy may depend on how many ensemble used for taking into account model internal variability, which can induce different injection rates based on model and SAI strategy. Please rephrase it.

We thank the reviewer for the suggestion: We have now rephrased the sentence to say: "The detectability of the differences in surface climate responses between SAI strategies depend on, among other factors, the level of global cooling and natural variability. While different SAI strategies do not result in the same surface climate, the differences in surface climate responses between some SAI strategies are much easier to detect than between others."

The reviewer is correct that natural variability will affect injection rates, yielding (slightly) different injection rates for each ensemble member. However, the standard error of injection rates among three ensemble members for each SAI strategy is relatively small, so this is a small effect relative to the direct role of natural variability in assessing differences between strategies. Therefore, model internal variability does not affect the conclusion quoted here (line 51-52 in the manuscript). We have updated Figure 2(a) to reflect the standard error of the injection rates.



2. Page 3, line 79, How can you affirm this "...the conclusion is expected to be reasonably robust and model independent..." knowing the model internal variability is not constrained by observations? Please rephrase it.

We have modified the sentence as "Although the estimate of 6-8 distinct injection choices was made using CESM1(WACCM) simulations, the conclusion is expected to hold relatively well in CESM2(WACCM) due to similarities in the stratospheric circulation and aerosol microphysics between the two model versions. This is demonstrated by the results of a set of fixed-amount single-latitude injection simulations (Fig. S1 in supplementary material)". As the estimate is primarily determined by stratospheric circulation, it is reasonable to expect broadly similar number of distinct degrees of freedom in other models, but this needs to be validated.

3. Page 8, line 184-185, Please replace the sentence by "Figure 2 shows the evolution of the total SO2 injection rate in each SAI strategy (a), and the 20-year (2050–2069) average injection rates (b)."

We have modified the sentence as "Figure 2 shows the evolution of the total SO2 injection rate in each SAI strategy (Fig.2(a)), and the 20-year (2050–2069) average injection rates (Fig. 2(b))".

4. Page 8, line 190-192 I wonder the role of the BDC on the "This hemispheric asymmetry in the distribution of SO2 injections is likely due to the rapid cloud responses to elevated CO2 levels in CESM2(WACCM6), resulting in greater radiative heating that needs to be mitigated in the SH (Fasullo and Richter, 2023)."

The reviewer is correct that asymmetry in BDC does mean that even a hemispherically symmetric injection rate will lead to some asymmetry in the aerosol optical depth, but this is a relatively small effect compared with the asymmetry that is needed in the multi-objective strategy to compensate for the effect noted. A similar strategy executed in CESM1, which has similar stratosphere, but different cloud fast response to CO2, required more injection in NH to compensate (Fasullo and Richter, 2023).

We calculated the interhemispheric imbalance for zonal mean AOD, $l1$, for the hemispherically symmetric strategies 15N+15S, 30N+30S, and 60N+60S; the values of $l1$ for these three strategies are 0.004, -0.004, and -0.007, respectively, which are negligible compared to the magnitude of zonal mean AOD and much smaller than the value of $l1$ of -0.02 needed by the multi-objective strategy to compensate for T1. Thus, the hemispheric asymmetry in the multi-objective strategy is not due to BDC. We add a brief note to the text commenting on this.

Reference:

Fasullo, J. T. and Richter, J. H.: Dependence of strategic solar climate intervention on background scenario and model physics, Atmospheric Chemistry and Physics, 23, 163–182, https://doi.org/10.5194/acp-23-163-2023, 2023.

5. Page 9, line 221-222, this is misleading "This asymmetry arises as the northern hemisphere has a stronger Brewer-Dobson circulation than the southern hemisphere". The inter-annual variability, which is much larger in NH than in SH, is what causes the asymmetry as the BDC is stronger in SH than NH.

We believe the statement in the current manuscript is correct; the stronger magnitude of climatological BDC in the NH than in the SH has been reported in a number of studies, e.g. Holton, 1990, Rosenlof and Holton, 1993, and Rosenlof, 1995.

List of references:

1. Holton, J. R.: On the global exchange of mass between the stratosphere and troposphere, Journal of the Atmospheric Sciences, 47, 392-395, 10.1175/1520-0469(1990)047<0392:otgeom>2.0.co;2, 1990.
2. Rosenlof, K. H., and Holton, J. R.: Estimates of the stratospheric residual circulation using the downward control principle, Journal of Geophysical Research-Atmospheres, 98, 10465-10479, 10.1029/93jd00392, 1993.
3. Rosenlof, K. H.: Seasonal cycle of the residual mean meridional circulation in the stratosphere, Journal of Geophysical Research-Atmospheres, 100, 5173-5191, 10.1029/94jd03122, 1995.

6. Page 10, line 229, please add "the seasonality in" before "the Brewer-Dobson circulation" you add these citations.

We made the change.

7. Page 10, line 229, please remove "also".

We reworded the sentence on line 230 as follows: "The distribution of AOD in the annual injection cases exhibits a marked seasonal cycle, with extratropical AOD maximizing in winter and spring at each hemisphere, due to seasonality in the strength of the stratospheric transport".

8. Page 12, line 265, this “as solar reduction doesn’t significantly change the Walker Circulation” is not clear. Please clarify or remove it.

We modified the sentences to read:

“The pattern is similar to the pattern associated with the positive phase of the El-Nino Southern Oscillation (ENSO; e.g. McGregor et al., 2022), albeit differing in the strength and horizontal extent of the anomalous warming in the eastern equatorial Pacific. This is associated with changes in the strength and the position of the Walker Circulation (Bednarz et al., 2023-strategy2), caused likely in part by the the aerosol heating in the lower stratosphere (Simpson et al., 2019), and the resulting changes on tropical precipitation patterns in the region.”

9. Page 12, line 275-279, this paragraph is not clear. Please rephrase it.

We made a few edits on this paragraph and combined this paragraph with the previous paragraph. “However, we find that in all SAI strategies, the rms temperature change is larger than the rms temperature change that one would expect due to natural variability alone (i.e. 0.08◦ C, represented by the dashed line in Fig. 8a), indicating imperfect compensation of the pattern of warming under climate change. Among the SAI strategies considered here, the multi-objective strategy best minimizes the spatial rms of temperature changes, as indicated by the lowest rms temperature change (rms T=0.38◦ C). The 60N+60S strategy results in an uneven cooling with the highest rms temperature change (rms T=0.57◦ C), but still much smaller than the rms temperature change in SSP2-4.5 without SAI (rms T=1.53◦ C).”

10. The result about precipitation responses in section 4.3.2 are overlooked. Please better discuss these results.

We have added further discussions on the regional precipitation responses in section 4.4 as described earlier.

11. Page 13, line 292-293, this “The difference in rms ... temperature responses.” is not correct for Amazonia & congo basin (Fig 9).

We have updated this sentence to “The difference in the spatial rms of P-E and precipitation responses between SAI simulations and the SSP 2-4.5 simulation is notably smaller than the difference in rms temperature responses, indicating poorer compensation of these metrics than temperature”. The metrics rms temperature, rms P-E, and rms precipitation are global metrics (integrating regional changes across the globe), thus your statement is not appropriate here, but we do add further discussion of the regional changes over these regions as described earlier.

12. Page 14, line 295, this “the difference is no more than 30 %” is really misleading as the precipitation changes as well as their importance on mainland and certain key regions are not homogeneously distributed.

We have modified the sentence on Line 295 as “For the corresponding changes in precipitation over the whole Earth surface (i.e. both land and ocean), the difference in rms precipitation response is no more than 30% when comparing any SAI strategy with the SSP2-4.5 case (Fig. 8(b)). While the difference in these global metrics between two strategies is modest, the corresponding regional P-E and precipitation changes can be locally statistically significant and thus need to be evaluated individually (Fig. 12)”.

13. Page 14, line 308-311, A discussion about the link between TICZ changes with different SAI strategies is missing.

We have added the following sentences in the paragraph on line 308-311:

Line 310: “The difference in the shift of ITCZ between the hemispherically-symmetric injection cases is modest, generally within one standard error.”

Line 311: “The multi-objective strategy is the only one that explicitly targets hemispheric asymmetry; while T1 is an imperfect proxy for managing ITCZ, it does result in improved compensation relative to the hemispherically-symmetric strategies, indicating the value of including an objective associated with asymmetric compensation.”

14. Figures 8 and 10 should be combined for clear discussion and reduction the numbers. For instance global and regional plots separately.

We thank the reviewer for the comment. However, as also noted in our response to the major point #4, we don’t think those two figures should be combined, as Figure 8 shows the rms changes of temperature and precipitation while Figure 10 shows ITCZ.

15. There is needs for separating global and region response better from page11 to the end.

We thank the reviewer for the comment. As noted in our response to major point #3, we separate the discussions on global surface climate responses and regional surface climate responses into different sections.

16. Page 17, line 326, it is not figure Fig 11a but Fig. 12a.

We thank the reviewer for this comment. We have corrected this in Line 326.

17. Page 17, Paragraph 338-341 is speculative. Please rephrase it.

We have deleted this paragraph.

18. Please move most of the Appendix figures into the main text discussed.

We thank the reviewer for this comment. As noted in our response to major point #5, we have moved most figures in the appendix to the main paper. We have moved Figure A3-A5 to Supplementary materials.