Comments on “Introducing a Comprehensive Set of Stratospheric Aerosol Injection Strategies” by Yan Zhang al.

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.

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.

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

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.

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.
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).”
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).”
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.
6. Page 10, line 229, please add “the seasonality in” before “the Brewer-Dobson circulation” you add these citations.
7. Page 10, line 229, please remove “also”.
8. Page 12, line 265, this “as solar reduction doesn’t significantly change the Walker Circulation” is not clear. Please clarify or remove it.
9. Page 12, line 275-279, this paragraph is not clear. Please rephrase it.
10. The result about precipitation responses in section 4.3.2 are overlooked. Please better discuss these results.
11. Page 13, line 292-293, this “The difference in rms ... temperature responses.” is not correct for Amazonia & congo basin (Fig 9).
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.
13. Page 14, line 308-311, A discussion about the link between TICZ changes with different SAI strategies is missing.
14. Figures 8 and 10 should be combined for clear discussion and reduction the numbers. For instance global and regional plots separately.
15. There is needs for separating global and region response better from page 11 to the end.

16. Page 17, line 326, it is not figure Fig 11a but Fig. 12a.
17. Page 17, Paragraph 338-341 is speculative. Please rephrase it.
18. Please move most of the Appendix figures into the main text discussed.

Reference:

@article{Bittner-2016,
author = {Bittner, Matthias and Timmreck, Claudia and Schmidt, Hauke and Toohey, Matthew and Krüger, Kirstin},
title = {The impact of wave-mean flow interaction on the Northern Hemisphere polar vortex after tropical volcanic eruptions},
journal = {Journal of Geophysical Research: Atmospheres},
volume = {121},
number = {10},
pages = {5281-5297},
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url = {https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2015JD024603},
year = {2016}
}

WMO Ozone assessment chap about SAI for an overview about all different techniques already performed: https://csl.noaa.gov/assessments/ozone/2022/downloads/Chapter6_2022OzoneAssessment.pdf