Response to Anonymous Reviewer #2:

We thank the reviewer for a positive review!

The manuscript presents a description of a new experiment to explore the effects of Stratospheric Aerosol Injection (SAI) on climate, part of a larger set of experiments (ARISE) to explore different types of solar radiation management. In particular, the manuscript presents an overview of the experimental setup and a very modest assessment of the results using the CESM2(WACCM6) model. In contrast to a previous SAI experiments (GLENS) run with the CESM1(WACCM) model, the new set of experiments uses a control experiment with a much lower radiative forcing scenario (SSP2-4.5) than the previous set (RCP8.5) thus necessitating a lower level of SO2 injection to stabilize near-surface temperature. Many have argued that the high radiative forcing scenarios like RCP8.5 are unrealistic, and the authors also make the argument that assessing SAI for a moderate radiative forcing scenario may prove to be a more applicable to our future.

The manuscript is very well written and a small selection of results are clearly presented. The few results that are presented, the effects of SAI on a number of measures related to near-surface temperature and precipitation, will be quite familiar to researchers in the field. The most intriguing result, the need for an injection of SO2 much more heavily weighted towards the Southern Hemisphere in this experiment with CESM2(WACCM6)-SSP2-4.5 in comparison with the earlier GLENS experiment with CESM1(WACCM)-RCP8.5 is discussed in a number of places but not illustrated with any results. I understand the reasons for the difference is part of on-going research, but a clearer picture of how the GLENS simulations compares with the ARISE simulations would add considerably to the paper and not compromise any on-going work to understand the reasons for the differences. Figures presenting the same information for the GLENS experiment as is given in Figure 2 (zonal mean stratospheric sulphate distribution) and Figure 3 (time series of T0, T1, and T2) would provide the reader with a much more complete idea of the differences. I would urge the authors to provide a bit more of a description of the differences between the GLENS and ARISE simulations.

You are correct - the difference between SO2 injections between CESM1(WACCM) and CESM2(WACCM6) is very intriguing and this also required us to dig deep into the models & emission scenarios to understand the differences. The reason for differences is not simple to explain, hence we have written a whole separate manuscript strictly focusing on this topic. We couldn't cite it at the time of submission of this paper but we have a citation now to reference: https://egusphere.copernicus.org/preprints/2022/egusphere-2022-779/

This manuscript includes specifically the differences in injections, surface temperature (and other quantities) between GLENS and ARISE and offers some explanation for these differences. We identify three main contributors including: 1) the rapid adjustment of clouds and rainfall to elevated levels of carbon dioxide, 2) the associated low-frequency dynamical responses in the Atlantic Meridional Overturning Circulation, and 3) the contrasts in future climate forcing scenarios. We cite the above manuscript now in the revised manuscript.

The model description in Section 2.1 seems quite detailed for a paper that is ostensibly about a new geoengineering experiment, particularly in describing changes between CESM1 and CESM2. I would suggest this section could be shortened somewhat.

Agreed. We have shortened each section and removed references to CESM1. In response to Reviewer #1 we did add a more detailed description of the aerosol model.

Other than these two suggestions I only have a small number of minor corrections that are listed below.

Minor comments:

Line 68 – I think the reference for Tilmes et al. (2020) is missing from the reference list.

Thank you for catching that - we have added the reference.

Line 127 – Flagging the ‘to’ in ‘trends to not shift’

Thank you - we fixed that.

Lines 163 – 164 – From the text a bit further down it is evident that T0 is used broadly to denote the global mean near-surface air temperature (at line 175) so it should be defined here and not later.

We have corrected this.