Response to Reviewer 1

We thank the reviewer for careful reading of our manuscript and constructive comments. Our responses to specific minor comments are given below in italics following each comment. Considering the issues raised by the reviewer has allowed us to improve our manuscript by clarifying these issues in the text. We are grateful for the reviewer's time and thoughtfulness.

Review for Weisenstein et al.,

"A model intercomparison of stratospheric solar geoengineering by accumulation mode sulphate aerosol"

Submitted ACPD

Here authors analyse CCM output from a dedicated GOIP solar engineering experiment "AM-H2SO4". This experiments is designed to inject geoengineering Sulphur (S) in the stratosphere in terms of particles (SO3 or H2SO4), so that stratospheric aerosol particles would grow mainly in accumulation mode, thereby negating effects of faster particle growth (and associated particle sedimentation). Analysis in this manuscript suggest that only three CCMs (WACCM, ECHAM5-HAM and SOCOL-AER) managed to complete these simulations. Basic idea behind these simulations is to differentiate model response to the SO2 vs particle injection under different (5 vs 25) Tg S injection magnitude scenarios. Authors find that all three models show increased radiative efficacy (in terms of radiative forcing) when Sulphur is injected in "AM-H2SO4" mode compared to gas phase injection. Also sensitivity simulations with different injection patterns (two points at 30° N and 30° S vs injection in a belt along the equator between 30° S and 30° N) find opposite response.

Overall this is well written manuscripts and fits well within ACP scope. Hence, I will like to recommend this manuscript for the publication with minor corrections.

Minor Comments:

1. Page 3: Line 28: Does that mean ECHAM has identical ozone loss in all the simulations?

   Echam uses the same prescribed ozone and OH fields for the reference and geoengineering simulations. Thus there is no ozone loss due to geoengineering in the simulations. We have made this explicit in the text.

2. Line 6 Line 18: I am really surprised that you use only 2 year spin up period. If you plot global burden, you would see steady increase in burden before curve

flattens, depending on dry and wet deposition schemes. Unless you have meteoric smoke particles transporting or mopping S-containing species downwards and there is lack of particle evaporation (temperature increase due to ozone increase), gas phase tracers (e.g. SO2, H2SO4) would show steady transport upwards. Overall tracers should reach to equilibrium state near model top after 3 to 4 years as they transport downward in the polar vortex. I think that is why WACCM (page 10 line 8) shows increasing residence with increase in injection amount. For e.g. Dhomse et al., 2013 (Figure 3) equilibrium for meteoric smoke particles is about 10 years. I suspect it should be at least 5 years for these simulations.

While we expect that aerosol concentrations near the top of the sulfate layer and at high latitudes might still be evolving after 2 years, this paper analyzes only globally averaged quantities which we find to be fairly stable with time after 2 years for most scenarios. See enclosed plots of time evolution of global burden for the CESM2 and MAECHAM5-HAM models. Aerosol global burdens are still rising after 4 years with AM-H2SO4 injections of 25 Tg/yr but are stable after 2 years for the other scenarios. The SOCOL model simulations actually used a 5 year snmi-up period and then the following 8 years are averaged, so those results should not have an issue with spin-up length. For the CESM2 and MAECHAM5-HAM models, global aerosol burdens using averages of the last 5 or 6 years of the 10 year simulations are greater than for the 8 year averages by only 2-3% with AM-H2SO4 injections of 25 Tg/yr. We will acknowledge in the paper that the spin-up period was too short for these scenarios but has minimal effect on any of the quantities presented or conclusions drawn.

None of these simulations contains meteoritic smoke particles. The residence times of aerosol shown in Figure 3 of the paper are derived from burdens and injection rates (not diagnosed removal rates), with injection rates constant in time. Thus correcting for a too-short spin-up time in the AM-H2SO4 25 Tg/yr scenarios would increase the residence time for the 25 Tg(S)/yr injections in our plot and does not explain why CESM2/WACCM shows increasing residence with an increase in injection amount.

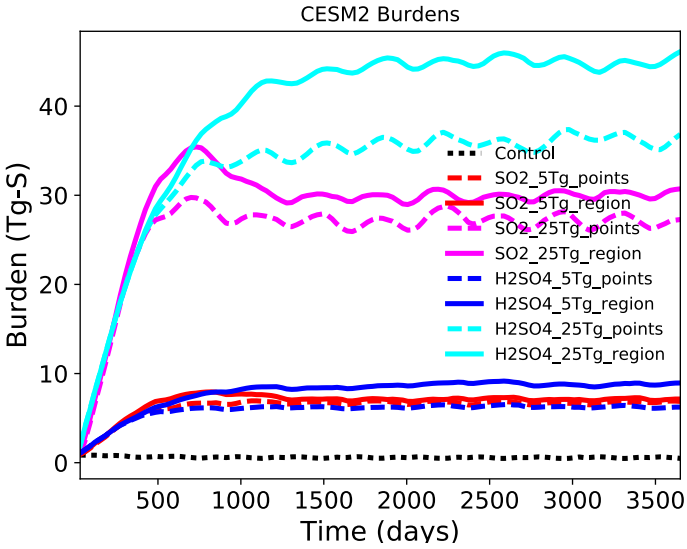

Figure R1: Time evolution of global aerosol burden in the CESM2 simulations.

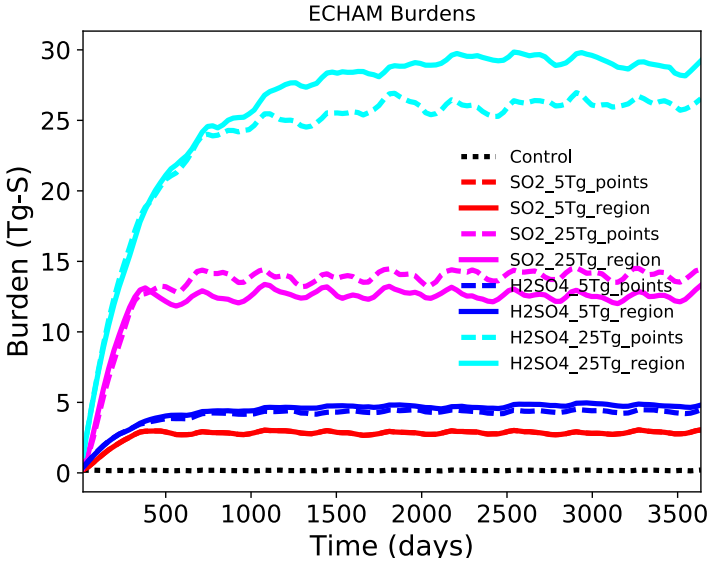

Figure R2: Time evolution of global aerosol burden in the MAECHAM5-HAM simulations.

3. Page 6: Line 19: What is baseline or reference simulation? Do you mean from respective SSP8.5 simulation? Is it from a single ensemble member or from ensemble mean?

The baseline scenario is described in the preceding paragraphs with SSP8.5 2040 GHGs and ODSs and SSTs for the 1988-2007 period. It is from a single ensemble member and we use an 8-year average from each model in our analysis.

4. Page 8 : line 1: Are you sure about only 10%? One needs to have very fast wet deposition. I think you should provide a line plot showing time variation in global burden.

   The tropospheric burden is found to be 10% or less of the global aerosol burden increase in the SOCOL-AER model, even for the 25 Tg(S)/yr cases. Tropospheric burdens are not available for the other models.

   A line plot of global burden time variation is provided for the reviewre (figures R1 and R2 herer, but as we don't have saved data of the 5-year spin-up period for SOCOL-AER, we do not include these plots in the paper.

5. Page 9: line 1: it should be other way round : weaker stratosphere troposphere exchange in the SH hence more aerosol accumulate in SH mid-lats.

   This referred to the lack of mixing between mid latitude and polar air in the southern hemisphere and hence the aerosol burden contrast in those two regions, which is not seen in the northern hemisphere with its weaker polar vortex and more efficient mixing to the pole. We modified the text to make this explicit.

6. Page 11: Figure 4: Does slope remain constant if you use only last 5 year data (5 year spin up).

   Yes, using the last 5 years produces minimal impact of the figure.

7. Page 12: line 9 : Any idea why ECHAM shows much weaker sensitivity.

   We are not able to diagnose the precise reason or reasons for the weaker sensitivity in the ECHAM model. Previous comparisons between ECHAM and CESM pointed to weaker tropical upwelling in ECHAM than CESM. The models also differ in the details of their aerosol formulations and their chemistry.

8. Page 18 : line 6 : Edit : 30°S-30°N

   We have corrected this.

9. Page 21: line 19: Are you sure it is minor. In Dhomse et al (2015), it is about 3%. With significant Cly decrease, future ozone losses would be largely controlled by NOy chemistry (e.g. Ravishankara et al.,2009), I would expect up to 5% ozone increase in the tropical middle stratosphere.

We do discuss ozone control by NOy chemistry and have added the reference to Ravishankara et al. (2009) and additional discussion regarding future ClOx and NOx changes. Ozone increases due to SO2 and AM-H2SO4 injections in these models are found in the 10-50 hPa region and near the tropical tropopause in the CESM model.

References:

Dhomse, S.S., Saunders, R.W., Tian, W., Chipperfield, M.P. and Plane, J.M.C., 2013. Plutoniumâ⓪❺238 observations as a test of modeled transport and surface deposition of meteoric smoke particles. Geophysical Research Letters, 40(16), pp.4454-4458.

Dhomse, S. S., M. P. Chipperfield, W. Feng, R. Hossaini, G. W. Mann, and M. L. Santee (2015), Revisiting the hemispheric asymmetry in midlatitude ozone changes following the Mount Pinatubo eruption: A 3-D model study, Geophys. Res. Lett., 42, 3038–3047, doi:10.1002/ 2015GL063052.

Ravishankara, A.R., Daniel, J.S. and Portmann, R.W., 2009. Nitrous oxide (N2O): the dominant ozone-depleting substance emitted in the 21st century. Science, 326(5949), pp.123-125.

Reply

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