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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Harnessing Stratospheric Diffusion Barriers for Enhanced Climate Geoengineering" *by* Nikolas O. Aksamit et al.

Anonymous Referee #2

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The paper by Nikolas O. Aksamit and co-authors is investigating a method to identify dynamical injection locations for stratospheric aerosol geoengineering approaches. The paper demonstrates that within the first seven days, this approach produces a wider particle distribution than fixed injection locations, using a simplified model (without aerosol microphysics). In a second experiment, a fully interactive Earth System model is used to test this approach. The authors claim that this results in improved effects of this type of geoengineering application.

I have several problems with this paper, and I don't think that it can be published in the current from and a significant revision is needed.

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Discussion paper

1. From what is stated in the paper, the authors may wrongly assume that H2SO4 in the model refers to liquid sulfate (SO4=), which is an H2SO4 solution. However, in WACCM, H2SO4 is a gas that will nucleate and form sulfate aerosols. To derive the sulfate burden in WACCM, one needs consider use so4_a1, so4_a2 and so4_a3 variables. The lifetime of sulfate can be identified in using the excess sulfate burden to decrease by 1/e from its peak, where excess burden is the amount above the pre-injection burden. However, this can be tricky due to seasonal and natural variability.

2. I don't see any support for the conclusions that "studies appear to have underestimated the potential coverage and therefore reflectance of geoengineered sulfate-aerosols". The coverage of sulfate aerosols has not been investigated in this paper based on the global interactive climate model. Also, the authors have not investigated changes in coagulation. They have been looking at the effective radius, which does not give a conclusive result. There are several other statements in the conclusions that have not been addressed in the paper, for example, this study has also not demonstrated that fixed aerosols result in "heating hot spots".

3. The dynamical injection method proposed here has demonstrated that within the first 10 days an idealized aerosol disperses faster if injected in regions in dispersion regions (which is not a new or surprising result). Any discussion about the efficiency of SO2 injections vs sulfate with regard to geoengineering is irrelevant, because the benefit has only been shown in a simple model and for a short time period. What happens if the new method is applied for 10 years and not just for 1 season? Will there be any difference in the aerosol burden?

4. Finally, it is not really clear how the approach of injecting into diffusive regions can improve controllability and improve a controller that relies on the fact that specific injection locations will result in specific AOD and temperature changes. Injecting into regions of increased mixing, how can this be used for the controller development?

Here are some more detailed comments (not all issues are listed)

Line 90: This part needs more explanation, since people should not be required to have to read the referenced papers.

Line 105: How were geoengineering objectives improved in this study? No temperature targets have been met with this method.

Line 111: What years has been chosen? The SSP8.5 experiment covers 2015-2100? Did you use a standard future fully coupled model configurations?

Line 118: The importance of transport barriers varies with altitude so differences with altitudes can be expected, what do you mean with “fundamental differences”?

Line 119: Please clarify how aerosols are modeled in this simulation, how do they become non-reactive fluid-parcels?

Line 121: Please explain what is meant by “short temporal neighborhood of the wind-fields output”? Also, the following sentence is unclear. Please describe, what you mean by fluid following particles (are those aerosols in the model, if so which aerosols where looked at?)

Line 126: Sentence is unclear: what “numerous natural climate cycles” is referred to here? Do you mean you used the entire 18.75 years for informing the injection locations?

Line 128: Are these 1-year simulations performed with prescribed wind fields? If so, are those wind fields derived from the 18.75 years of simulation? Having a table that describes the experiments would be helpful. What do you mean by seasonal injections of sulfate precursors? Did you inject SO2 only once or every season, how much?

Line 210-212: What do you mean by aerosol concentrations, what aerosols are used? What do you choose for the size of a bin, since this is a modal model?

Line 218ff: The text is very confusing. The authors discuss H2SO4 (the aerosol precursor gas) which then nucleates to form sulfate aerosols. After injections of SO2 and

oxidation to H2SO4, H2SO4 will decay and aerosol will be formed. It is not clear how the authors learn about the aerosol lifetime and microphysical processes, while investigating H2SO4.

Line 224: Please explain above how the model simulation has been performed without microphysics (do you mean aerosol microphysics)? Again, another very confusing statement here, the authors now state they injected aerosols. What aerosols have been used?

226: If this is a 2D model lon/lat, what are zonal concentrations? Zonal averages? Were injections performed at one point or over one longitude band? How fast are aerosols been transported longitudinal? How do those locations correspond to the transport barriers subtropics and polar jet stream? Why is there a difference between winter and summer?

228: What do you mean by: there is north movement of "the volume of particles"?

Figure 5: From the figure caption one cannot understand what the lines represent? Fraction of sulfates with regard to what?

Figure 6: Assuming that the same amount of "aerosol" has been injected for the fixed and the variable injection sides, how can the Relative Entropy be different at the start?

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