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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "What is the limit of stratospheric sulfur climate engineering?" *by* U. Niemeier and C. Timmreck

Anonymous Referee #3

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The authors use a microphysical aerosol modal model whose mode widths are tailored to match the sectional model MAIA under different SO2 loadings. The aerosol model is radiatively coupled to the GCM. The details on the chemistry are not provided. The authors conduct a series of simulations with increasing SO2 injection rates in a single grid box at about 19km above the equator, and a few other simulations with varying spatial scales (e.g. Geo10-lon and Geo10-30) and mode widths (e.g. "Volc100"). This appears to be a decent model setup to study an interesting, important, and novel problem. My primary concern is that the bulk of their simulations investigate increasing so2 injections in one stratospheric grid box at about 19km, which has been done before. Their main results simply confirm that other studies could be extrapolated with reasonable accuracy. This paper could be significantly improved if the authors explore the

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uncertainties involved with 100 Tg injections regarding aerosol mode widths, spatial domain of SO2 injections, and model dynamics and chemistry, and bracket their results with this uncertainty range. The following revisions, (some of them major) would address this.

1) Stratospheric dynamics have a critical impact on aerosol microphysics and lifetime. As the authors noted, there is significant disagreement between their broad (30S-30N) injection results and those of Pierce et al. (2010) and English et al (2012), and they speculate that differences in the tropical transport barrier may be a primary reason why. Are there observations or other studies that estimate what the actual transport efficiency across this tropical transport barrier might be? Which model(s) are more accurate - Pierce, English, or this work? What does the absence of QBO in their model do to the stratospheric circulation? what are the possible errors that arise from it? What is the stratospheric age-of-air in their model compared to a best-guess from observations? the ECHAM model has a rather coarse vertical resolution (39 vertical levels); how might that affect stratospheric dynamics and strat-trop exchange? Based on these dynamical uncertainties, what are estimates regarding how this may translate to errors in geoengineering efficacy? For example, if the age-of-air in your model is 10% too short, does that translate into a geoengineering AOD that is 10% too low? or vice versa. New model simulations of 100 Tg injections with modified dynamics (QBO, gravity waves, etc) that alter transport efficiency across tropical barrier and/or stratospheric age-of-air would help quantify these uncertainties.

2) While modal aerosol models are vastly superior to bulk aerosol models, they have several limitations to sectional models. The authors took some care with specifying mode widths to match results from the MAIA sectional model as documented in Kokkola et al. 2009, but there are some limitations to this approach, and other things to consider. The authors state that they use two different specifications, one for VOLC with no coarse mode, and one for GEO with a coarse mode width of 1.2. However, in Kokkola et al. 2009, there is no setup with a coarse mode width of 1.2. I think the

authors modified "Setup 1" and reduced the coarse mode from 2.0 to 1.2 for their GEO mode width. If this is the case, where is it documented that changing "Setup 1" to a smaller coarse mode width improved results? Please clarify this in the paper, as I was confused. Regardless, in Kokkola et al. 2009, the ideal mode widths seemed to be a function of SO2 concentration. If this is the case, then the authors would likely need to change the specified mode widths when changing from a narrow SO2 injection to a broad SO2 injection, and/or from a smaller injection rate to a high injection rate, as the SO2 concentrations would change by 1 to 2 orders of magnitude. The authors partially did this with their VOLC versus GEO specifications, but there are plenty of additional uncertainties. For example, other modeling studies of stratospheric aerosol size distributions have found different results. Weisenstein et al. (2007) investigated coarse mode widths between 1.45 and 1.58 and found that modal models were accurate sometimes, but not always, and there was no single mode width specification that was consistently most accurate. English et al (2013) calculated equivalent lognormal mode widths from their sectional model after large volcanic eruptions and found the coarse mode widths to vary between 1.2 and 2.0. (Please cite both of these papers). Also, as aerosol size evolves, mode widths can change. 2-moment modal models such as what the authors use here are unable to represent this. Some of these things may be able to be calculated, but others may require new simulations, such as changing the GEO mode width from 1.2 to 2.0 and comparing 100-Tg injections. (my understanding is that the VOLC simulations completed actually remove the coarse mode rather than changing the mode width).

3) The authors note the impact of injection height and pulsed injections. At 100 Tg injection rates, how much more effective is an injection at 25 km compared to 19km? At 100 Tg injection rates, how much more effective is a pulsed injection compared to a continuous injection? New model simulations may be required to confidently include these parameters when calculating an uncertainty range.

Based on these additional simulations, and other estimates of uncertainties based on

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your own calculations and other papers, calculate an uncertainty range of injection rates required to counteract "business as usual" and include that range in the abstract (e.g. maybe it's 20-50 Tg/yr instead of 45 Tg/yr).

Specific suggestions

Title: Either change title to "What is the limit of climate engineering via continuous SO2 injections at 19km altitude", or preferably, conduct more detailed assessment of uncertainty ranges via sensitivity studies, some of which are outlined above.

Abstract: you use the term "injection strengths" but a more accurate term would be "injection rates". Please go through the manuscript and be consistent with whatever term you decide on. You also use "injection flux" and "emission strength" in other places. I think rate is better than strength or flux.

Abstract: Mention that the 45 TgS/yr calculation comes from continuous so2 injections in a single grid box at 19km altitude, and add uncertainty ranges around it based on the sensitivity studies completed as per my primary suggestions. for example, is your best guess 30 to 60 TgS/yr so2 injection based on uncertainty analysis of stratospheric dynamics, aerosol microphysics, injection domain, etc.

Section 2.1: What is the chemistry scheme in your model? Please provide this information in the paper revision and a brief citation to or explanation of the pros/cons/possible errors involved with the chemistry scheme on geoengineering efficacy.

Section 2.1 para 5: Please clarify how you changed sigma to 1.2 instead of 2; are those results published somewhere?

Section 2.1 para 6: It seems like QBO could significantly impact your conclusions. What is your rationale for saying that it wouldn't? What is your best guess as to what the AOD & burdens would be if your model did resolve QBO? Would efficacy be better, worse, and/or what is the uncertainty?

Section 2.2: you mention other studies that found improved results with increased

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injection height and pulsed injections. These are important "pieces of the puzzle" for determining what the actual geoengineering limitations might be.

Section 3.1 para 2: In the paragraph starting with "A more detailed illustration.." there are several sentence fragments that could be improved.

Section 3.2.1: The bulleted list 1-4 has several grammatical errors: (improvements suggested): 1. "Nucleation continuously forms new small particles within the injection area." 3. "Due to advection, larger particles in the the accumulation and coarse modes are globally dispersed." 4. "The larger the ratio, the larger the coagulation coefficient."

Section 3.2.2 para 1: Yes, you mention the possible impacts of QBO. It would be interesting to do a sensitivity study on the effects of QBO on geoengineering efficacy. At a minimum, estimate the uncertainty in your results based on this.

Section 3.3 para 3: What do the observations say about meridional transport/tropical transport barrier? Which of the three models is most accurate? How do these varying results contribute to an uncertainty analysis of the actual limits with stratospheric so2 geoengineering?

Section 3.3 para 3: It is "AOD", not ADO

Section 4 para 2: It would be interesting to calculate the CO2 emissions from 6 million aircraft flights per year. The net geoengineering efficacy would be reduced further due to the LW absorption from additional CO2.

Section 4 para 3: grammar error here: " may get via sedimenation...". And after "changes in precipitation" add ", etc." or equivalent.

Conclusions para 2: grammar: "This study contributes". grammar: "less evenly distributed".

Conclusions: Here and elsewhere, change "injection flux" to "injection rate" everywhere in the paper.

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Table 1: Instead of "geoeng" or "volc", it would be more useful to state the mode peaks and widths. Perhaps you could put "geoeng" or "volc" in parantheses.

Fig.1: There are specific definitions of "TOA radiative forcing"; please make sure you are consistent with them.

Fig.2: The legend overlaps with some of the curves, and the y-axis units needs a superscript.

Fig.3: First sentence is not clear. Do you mean to say "injected in a one grid box wide area"?

References English, J. M., O. B. Toon, and M. J. Mills (2013), Microphysical simulations of large volcanic eruptions: Pinatubo and Toba, J. Geophys. Res. Atmos., 118, 1880–1895, doi:10.1002/jgrd.50196.

Weisenstein, D. K., J. E. Penner, M. Herzog, and X. Liu (2007), Global 2-D intercomparison of sectional and modal aerosol modules, Atmos. Chem. Phys., 7, 2339–2355, doi:10.5194/acp-7-2339-2007.

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