Answers to reviewer 3 on the ACPD paper (acp-2015-25)

What is the limit of stratospheric sulfur climate engineering?

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We thank all reviewers for the careful reading and the thoroughly suggestions. They help us to improve many parts of the paper, especially the comparison to previous studies. Two reviewers were asking on putting more emphasis onto the injection height. We performed two additional simulations with an increased injection height (24 km) and two different meridional extensions (grid box and 30N to 30S). Efficiency is increased by 50% and 36% in these simulations. This allowed a much better comparison to the results of English et al (2012) and Pierce et al (2010) and helped to explain the differences (Fig. 1). We added an extra sub-section for this topic, added the two simulations to table 2 and changed the text in the comparison to other model accordingly. Our main conclusion there is: From Geo10-high and Geo10-30-high can we see that the main impact on efficiency is the increase in injection height, while the increase of the area in meridional directions decreases the efficiency. We assume this also be valid for the difference between "NARROW" and "BROAD" in E12.

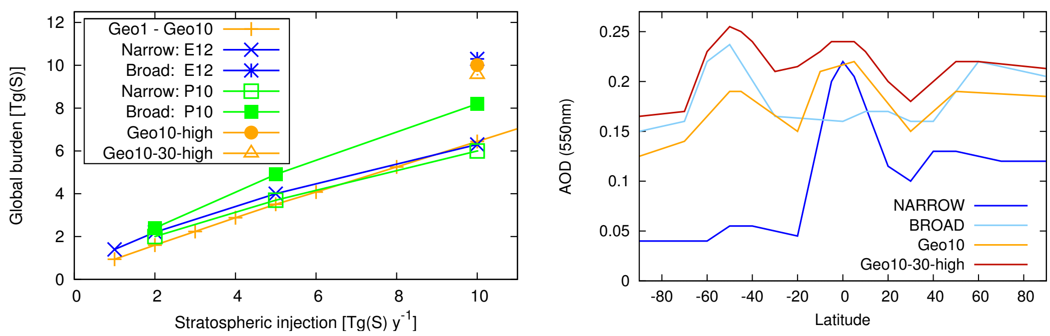

Figure 1: Left: The global sulfate aerosol burden for ECHAM5-HAM simulations Geo1 to Geo10 compared with results from Pierce et al. (2010), P10, and English et al. (2012), E12, for two different emission areas: NARROW, 5 N to 5 S, and BROAD, 30 N to 30 S. In the BROAD simulation the injection area is additionally increased vertically to 20 – 24 km. Right: Plots comparing the zonal mean of the AOD for a narrow and a broad injection area. Plots were created using smoothed values of Geo10 and Geo10-30-high and estimated from "SO_2 NARROW" and "SO_2 BROAD" data after English et al. (2012).

We completely rewrote section 4 and changed the headline to: Limit, uncertainties, and consequences of strong sulfur injections? We included a short discussion of the uncertainties estimated from the experiment design and the model concept. We also include a discussion on some possible impacts: 'What would be the consequences of a 5.5 W m^{-2}

reduction of the forcing´´ and discuss briefly impacts on precipitation, ozone, cloud condensation particles, which were estimated from previous studies.

General comments

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?

The results of the two additional simulations made it possible to better distinguish between the impact of an increased injection height and an increased meridional extension. The global burden results of the new simulations are now quite close to the ones given in English et al (2012). However we still see differences in the simulated meridional transport between both models. We know from ECHAM5-HAM that the model slightly overestimates the meridional transport. An answer which of the other models is more accurate is quite difficult, most probably better with a detailed model data intercomparison. Here we have to refer to the upcoming SSIRC model intercomparison http://www.sparc-ssirc.org/.

What does the absence of QBO in their model do to the stratospheric circulation? what are the possible errors that arise from it?

Punge et al. (2009) compare zonal mean values of stratospheric CH4 concentrations between the east and west phase of a QBO. Concentrations in the tropics are increased by 10% and decreased by 10 to 15% in extra tropics in an easterly phase at 10 hPa. They state differences in the strength of the transport barrier with QBO phase as reason. Differences in meridional transport were already found by Plumb and Bell (1982). Hommel et al. (2015) found modulations of the size distribution of the aerosol by the QBO but in the bulk of the stratospheric aerosol layer for most of the analyzed parameters (incl. the effective radius) only moderate statistically significant QBO signatures (<10 %). See also later comment on Sec. 2.1

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.

Age of air (AoA) measures the mean transit time of air parcels along the Brewer-Dobson circulation (BDC) starting from their entry into the stratosphere. AoA is determined both by transport along the residual circulation and by two-way mass exchange (mixing) (Garny et al, 2014). We cannot estimate the age of air from our simulations but previous studies showed an age of air of 3 to 3.5 years in ECHAM6, depending on the model vertical resolution, 4 years in MaEcham5 (Manzini and Feichter (1999)) compared to 4.5 to 5.5 years in measurements (Bunzel and Schmidt (2013)). The comparison of L47 and L95 simulations in Bunzel and Schmidt (2013) shows a smaller age of air for L47 and a slightly higher upward mass flux through the 70-hPa pressure surface. In a simulation with high vertical resolution the QBO is resolved with additional implication on meridional transport (see below).

Garcia and Randel (2008) show an age of air of about 3 years for the WACCM model and values in-between 2.8 and 3.8 years depending on the method (Garcia et al, 2011). Compared to the measurements both models show a too low AoA and we will not gain an answer on the sulfate transport from this value.

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.

We agree on that but a detailed investigation of stratospheric transport dynamics could be a topic on its own. Our intention is to explore the limits of climate engineering by stratospheric injection of SO2 and we choose a continuous 10TgS/y as our reference standard case from which we explore the parameter space.

2)Weisenstein et al. (2007) investigated coarse mode widths between 1.45 and 1.58 and found that modal models were accu- rate 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)

We added to section 3.2.3: Weisenstein et al (2007) compared a modal aerosol model with a fine bin model, showing that with optimized mode width a modal model can describe the distribution of a bin model reasonable. English et al (2013) highlighted the changing mode width over time after a volcanic eruption. This changing time factor is not important under SRM. However, the result show that under different injection rates the mode distribution differs which might alter the TOA radiative forcing as well. We agree with the reviewer that a modal model is a simplification, as all ready stated in the paper. Regarding the uncertainties related to injection area, transport and the fact that the difference between experiment Geo100 and Volc100 is about 6% in TOA forcing, we decided not to put our focus on a sophisticated system of different mode width choice depending on the SO2 concentration. Of course, the choice of the mode width has an impact on the TOA forcing, because the it depends on the particle size(e,g Timmreck et al. (2010), but a detailed discussion of this aspect is beyond the focus of our paper).

3) The authors note the impact of injection height and pulsed injections. At 100 Tg in-

jection 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 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).

Thank you for insisting on further details on the impact of the injection height (details see above). WE also included a value on the impact of pulsed injections and included a summary on the estimated uncertainties into Sect. 4 (details see above). We changed the last sentence in the abstract to: This result implies that the solar radiation management strategy required to keep temperatures constant at that anticipated for 2020, whilst maintaining "business as usual" conditions, would require atmospheric injections into a height of 60 hPa of the order of 45 Tg(S) yr^{-1} ($\pm$ 12% or 6 Tg(S) yr^{-1}) which amounts to 5 to 7 times that emitted from of the Mt. Pinatubo eruption each year. We rewrote Section 4 and included many of the calculated uncertainties there and additionally into the previous sections.

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.

The title changed slightly to: "What is the limit of climate engineering by stratospheric injection of SO2?" We added simulations with an increased injection height.

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.

Thank you very much for the suggestion. We corrected this in the text.

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.

See above.

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.

We added A simple stratospheric sulfur scheme is employed in model levels at the tropopause and above (Timmreck (2001); Hommel et al. (2011)). The gaseous precursor species (OH, NO2, and O3) are prescribed on a monthly bases, as well as photolysis rates of OCS, H2SO4, SO2, SO3, and O3. OCS concentrations are prescribed at the surface and transported within the model. to the text in the model description and added to the introduction: The model is not coupled to an ocean model, nor is a full atmospheric chemistry module integrated. Thus, impacts on climate or ozone concentrations cannot be simulated. In Section 4 some estimates after previous studies on a possible impact on the ozone concentration are given.

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

The sigma value is given as a parameter in the model code. We slightly changed the text in this paragraph. The results are not published previously but given in Figure 1 in this paper. Reason for the change from the set up of Niemeier et al. (2009) is that we used Heckendorn et al. (2009) as reference for our mode setup and decided to stick closer to the setup which was determined in the box model comparison in Kokkola et al. (2009) for the related SO_2 concentration.

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 and burdens would be if your model did resolve QBO? Would efficacy be better, worse, and/or what is the uncertainty?

The following text was added to Section 3.2.4: This study was performed with a relative coarse vertical resolution of 39 levels up to 0.01 hPa. Increasing the amount of vertical levels and consequently reducing the vertical grid space would slightly increase efficiency due to less numerical diffusion (3% higher burden estimated from a volcanic eruption study). Including the QBO via nudging may also increase efficiency. Punge et al (2009) show that methane concentrations in the tropics change by $\pm$ 10% and by 10 to 15% in extra tropics depending on the QBO phase. These differences are caused by the different meridional transport as a consequence of different stratospheric transport barrier strength between QBO east and west phases Plumb and Bell (1982). A detailed analysis of the QBO impact on the tropical stratospheric aerosol layer was recently published by Hommel et al. (2015). They found in the bulk of the stratospheric aerosol layer for most of the analyzed parameters (incl. the effective radius) only moderate statistically significant QBO signatures (<10 %). Simulating an internally generated QBO like oscillation by increasing the vertical resolution to 90 levels would cause a slowing of the QBO oscillation and for injection rates roughly about 8 Tg(S) yr^{-1} a constant QBO west phase in the lower stratosphere with overlaying easterlies. Aquila et al. (2014). Increasing injection rates strengthen the constant QBO west phase and, following Plumb and Bell (1982), decrease efficiency further by reducing the meridional transport.

Section 2.2: you mention other studies that found improved results with increased injection height and pulsed injections. These are important "pieces of the puzzle" for determining what the actual geoengineering limitations might be. See above.

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

Thank you, we changed the text slightly.

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."

Thank you, we followed this suggestion.

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 aminimum, estimate the uncertainty in your results based on this.

We refer again to the very detailed study by Hommel et al (2015) who investigate the impact of the QBO on the tropical aerosol layer during volcanically undisturbed times. They found below 10 hPa, in those regions where the aerosol mixing ratio is largest (50–20 hPa, or 20–26 km), that in most of the analyzed parameters only moderate statistically significant QBO signatures (<10 %). This is also valid for the effective radius, where QBO-induced modulations are smaller than 5 %. A detailed sensitivity study would be beyond the scope of the paper. For the changed text see our comment above.

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?

Observations on sulfate transport are only available from short periods after volcanic eruptions, mostly Mt. Pinatubo eruptions. AS 6 weeks later Cerro Hudson erupted, measurements are influenced by a small degree by this eruption. This allows many interpretations of the sulfur transport after the Mt.Pinatubo eruption. Therefore, these questions cannot be answered within this study. The planed intercomparison study within SSIRC may give an answer.

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

Done

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.

The number of necessary flights are no longer in the text. See also comments to the other reviewers.

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

Section 4 has changed (see above).

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

Done

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

We followed this suggestion.

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.

We added the sigma values to Table 1.

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

We changed the text to: These data are derived from calling the radiation calculation in the model twice, once without and once with aerosols. With this method we calculate the instantaneous aerosol forcing only and get the radiative forcing of the aerosol.

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

We corrected the legend and slightly changed the figure regarding a comment of reviewer 1.Superscript still missing

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

We corrected this to: Burden of (left) SO2 and (right) sulfate coarse mode particles as calculated the first grid box along the Equator for two different simulations.

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