Answers to reviewer 1 on ACPD paper (acp-2015-256)

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 models 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.

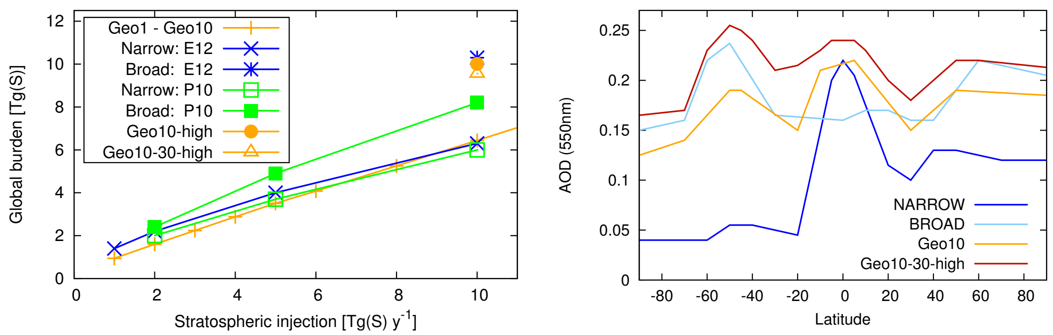

Figure 1: Left: The global sulfate aerosol burden for ECHAM5-HAM simulations Geo1 to Geo10 compared with results from Pierce et al. (2010), P10, and English et al. (2012), E12, for two different emission areas: NARROW, 5 N to 5 S, and BROAD, 30 N to 30 S. In the BROAD simulation the injection area is 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.

Specific Comments

1. Page 10943, lines 5-14: A few words describing how atmospheric oxidants are handled would be useful, i.e. whether they're prescribed or modeled interactively.

We added A simple stratospheric sulfur scheme is employed in model levels at the tropopause and above (Timmreck et al. 2001; Hommel et al. 2011b). The gaseous precursor species (OH, NO2, and O3) are prescribed on a monthly bases as well as photolysis rates of OCS, SO2, H2SO4 SO3, and O3. OCS concentrations are prescribed at the surface and transported within the model.

2. Page 10946, lines 18-20: Aerosol number in the coarse mode also appears to increase more rapidly than that in the accumulation mode (the lines in Fig. 1 are more widely separated in the coarse mode when compared with the accumulation mode), which might be worth mentioning.

We included into the text: As injection rates increase, particle number and radii increase stronger in coarse mode than in accumulation mode.

3. Page 10952, line 21: It would help if it was made clear which of the seven lines in E12's Fig. 9 were used and how they were "estimated and simplified". Was there any scaling of the 525nm AOD in E12's Fig. 9 to the 550nm AOD used here?

The data are not scaled from 523nm to 550nm. We tested the difference between these bands in a radiation simulation and got a difference of 3%. Regarding the overall larger uncertainties we decided not to take this into account in the paper.

The data for the Figure were taken from the Fig. 9 in E12: 'SO2 Broad' and and 'SO2 Narrow'. We do not have the original data. The resulted curve was smoothed to show the main features and not each single maximum and minimum. We change the text to: we draw a schematic diagram of the zonally averaged AOD obtained for a narrow and a broad injection area (Fig., right) after ECHAM5-HAM results and after estimated and smoothed values from Fig. 9 in E12. The curves were smoothed for a better overview.

4. Page 10945, lines 7-11; Page 10953, lines 13-15; Page 10955, lines 1-4: Throughout the paper (I've just selected certain points where the issue is addressed) it needs to be emphasized that the values obtained in this study are for the specific injection altitude chosen. The authors mention (p.10945) that increasing the injection height also increases the efficiency, but there is no quantitative analysis of this effect. This point needs to be made again later in the paper in Sections 4 and 5 where a specific forcing value or values are discussed - these only apply for the altitude chosen. Some quantitative estimate of the range of how the forcing and efficiency might vary with injection height is desirable.

We thank the reviewer to insist 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). See the text above fore more details.

5. Page 10953, lines 17-24: This Section is the biggest problem with the manuscript.

After careful explanation and analysis up to this point the paper loses its way here.

We completely rewrote this section and changed the headline to: “Limit, uncertainties, and consequences of strong sulfur injections“. See the text above fore more details.

Specifically:

(a) What is the source of the "flight emissions" data? Are the emissions comparable to the geoengineering levels under discussion? (line 17).

The values were calculated after the payload given in Robock et al (2008). We skipped this part and discuss only very briefly possible injection heights (after McClellan et al):...Increasing the injection height would reduce the required amount of SO2. However, this would be technically much more challenging. Following McClellan et al. (2012) many existing planes would require technical changes to reach a height of 18 to 20 km. Only Boing F14E may reach higher levels, which otherwise could only be achieved by newly developed technology like hybrid air ships....

(b) I can't make sense of the phrase "injection efficiency given per achieved reduction of TOA forcing in Wm-2" (line 18).

(c) The efficiency of SO2 injection was defined previously (page 10945, lines 20-21) as "the ratio of the top of atmosphere (TOA) forcing to injection strength", which is quite clear. Here, however, it's defined differently as "the amount of sulfur per Wm-2 which is needed to get a certain TOA forcing" (lines 19-20), which I don't follow.

(d) Then (line 20) there's a reference to "These data" - which data?

(e) Then follows (lines 20-22 and more in the next paragraph) some numerical values which appear out of nowhere with no explanation of their source. Neither do I understand what they mean. In lines 20-22 it says "to obtain a reduction of -1 Wm-2 an injection of 4.5 Tg(S)/yr per Wm-2 is necessary, while -7 Wm-2 TOA forcing requires an injection of almost 10Tg(S) per Wm-2". Where do these numbers come from? What does it mean to describe an injection in units of "Tg(S)/yr per Wm-2"? To me, an injection rate is an amount of substance per unit time, so I don't understand what it means to describe an injection in terms of mass per unit time per Wm-2. This section, where the central question of the paper's title is finally addressed, needs to be thoroughly revised. As it stands it makes no sense to this reviewer.

Thank you very much for pointing this out. We revised the text accordingly. The Points b) to e) were taken into account when rewriting the section. The text mentioned in c) and e) is removed.

Minor Comments/Technical Corrections

Comments 1 to 10 were done as suggested. Thank you for the careful corrections.

11. Page 10961, caption to Table 1: Include a few words to make it clear that 'Geo10', which occurs a lot in the text but is not in the Table, is just the 10 Tg(S)/yr version of simulation 'Geo'.

We added a note to the table caption.

12. Page 10963, Figure 1 (Left): Remove the crosses from the part of the curve for injection rate values greater than 100 Tg(S)/yr: having them on the plot implies that simulations were done for these rates (at about 120, 140, 160, 180 and 200 Tg(S)/yr) but the text

suggests that this is just an extrapolation of the fit.

Done.

13. Page 10964, Figure 2: A color blind person is likely to find this plot difficult to interpret - I suggest either changing to a 'colourblind accessible' set of colours or taking a different approach to this plot.

We tried to improve the figure by changing the colors and reducing the number of lines.

14. Page 10967, caption to Figure 5: The phrase 'with different injection rates of 10 Tg(S)/yr' doesn't really mean anything. If they're all injecting at 10 Tg(S)/yr then the rates are not different. I think you mean that they all inject at the same rate but have different injection strategies or implementations.

Thank you. We changed the caption text.

References

English, J. M., Toon, O. B., and Mills, M. J.: Microphysical simulations of sulfur burdens from stratospheric sulfur geoengineering, Atmos. Chem. Phys., 12, 4775–4793, doi: 10.5194/acp-12-4775-2012, 2012.

McClellan, J., Keith, D. W., and Apt, J.: Cost analysis of stratospheric albedo modification delivery systems, Environmental Research Letters, 7(3), doi:10.1088/748-9326/7/3/034019, 2012.

Pierce, J. R., Weisenstein, D. K., Heckendorn, P., Peter, T., and Keith, D. W.: Efficient formation of stratospheric aerosol for climate engineering by emission of condensible vapor from aircraft, GRL, 37, L18 805, doi:10.1029/2010GL043975, 2010.