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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Exploring accumulation-mode-H_2SO_4 versus SO_2 stratospheric sulfate geoengineering in a sectional aerosol-chemistry-climate model" *by* Sandro Vattioni et al.

Anonymous Referee #2

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Manuscript by Vattioni et al studies several injection strategies for stratospheric sulfur geoengineering with gas phase SO2 and sulfate droplet injections. Research is done by using global 3D-aerosol-chemistry-climate model. This is one of the few studies where the impacts of stratospheric sulfur injections are studied with a sectional aerosol model coupled with/included in a global climate model. Authors have simulated several different scenarios to cover a wide range of options to inject sulfur to stratosphere.

Even though the general idea of the study and the studied scenarios are not totally new, the study shows several eye opening results and it is a good addition to existing

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Discussion paper

research. Currently there are relatively few studies where stratospheric sulfur geoengineering is simulated by including aerosol microphysics and especially with sectional aerosol model. Radiative forcing of stratospheric sulfur geoengineering is dependent on several factors, related to how sulfur is injected, but also how the microphysics is modelled. Thus it is valuable to get information from different scenarios simulated with different models. Authors also quantify microphysical processes (such as nucleation, condensation, coagulation) in various scenarios which helps to understand the impacts of microphysical processes on geoengineering. In addition, for example, the responses in OH concentration were surprising, but well justified. Overall this is an interesting and excellent study. It is well written and does not leave open questions. Thus I recommend publishing this manuscript and I have only minor comments on some specific points in the text. I also have to say that it is quite impressive that the work is based on a master's thesis.

I want to mention that I do not agree with reviewer 1 concern about differences between longitudinal distribution of emitted sulfur. As it is generally known, and pointed out in this study, results from point like simulations do not differ much from injections over all longitudes. In addition, it would be challenging to do an apple to apple comparison between the results of this and earlier studies, and I think it is not necessary in this case. My opinion is that the author's choice to use "all longitudes" -case as "default" option and pulsed scenario as a sensitivity case would have been natural choice for me too.

P1, l 17 As was already commented by Alan Robock, using parentheses like this is a bit confusing.

P1 , l22-23 " Increasing the local SO2 flux in the injection region by pulse or point emissions reduces the. . ." Would it be better to say something like: "concentrating injections to smaller regions by pulse or. . ." You don't just increase emissions somewhere but simultaneously decrease (remove) them elsewhere.

P2, l20 I would include following citation: Niemeier, U. and Timmreck, C.: What is the limit of climate engineering by stratospheric injection of SO2?, Atmos. Chem. Phys., 15, 9129-9141, https://doi.org/10.5194/acp-15-9129-2015, 2015. It shows nicely the reduced efficiency in the case of really high loading.

P2, l22 Is it really a limitation? This study is not concentrating on this topic so this sentence can be removed.

P4, L2 Just a comment, sigma is usually fixed and same mode width does not represent well both coarse mode particles in troposphere and stratosphere (long living particles).

P4, L8. Sectional aerosol model is also used in: Laakso, A., Kokkola, H., Partanen, A.-I., Niemeier, U., Timmreck, C., Lehtinen, K. E. J., Hakkarainen, H., and Korhonen, H.: Radiative and climate impacts of a large volcanic eruption during stratospheric sulfur geoengineering, Atmos. Chem. Phys., 16, 305-323, https://doi.org/10.5194/acp-16-305-2016, 2016.

Laakso, A., Korhonen, H., Romakkaniemi, S., and Kokkola, H.: Radiative and climate effects of stratospheric sulfur geoengineering using seasonally varying injection areas, Atmos. Chem. Phys., 17, 6957-6974, https://doi.org/10.5194/acp-17-6957-2017, 2017.

P4, L18. , “the radiation scheme did not interact with the aerosol module” This is not true.

P6, L11 Is there some explanation behind the decision to use 1.83 MT S yr-1 injections? For me it sounds like an accidental choice where you originally planned to do injections with certain mass but after all simulations were done, you noticed that unit in emission(/injection) was not what it should have been. However, I do not say that this is a problem, because there is not anything “wrong” to use this value, but if there is a sensible reason for use this specific value, it should be mentioned.

This is also just a comment, but it would have been nice to see differences between

SO2 and sulfate injections in a case of larger amount of injection.

P6, L13 There are several estimations for mass of the emitted sulfur from Mt Pinatubo eruption. It would be good to cite some study.

P6, L21 QBO nudging (without nudging winds generally) is new to me. If you can open this method by few clear sentences, it would be great. If not, then it is ok as it is.

P8, L28 and L33, Based on table 2, I got 26.8% shorter resid. time in GEO_SO2_15 than in corresponding AERO-case (not 23.3%). What is 32% difference in L30? It would not be the first time that I cannot calculate something right but please check these.

P9, L2 and maybe due to the coagulation?

P9-> It would be useful if radiative forcing for LW was mentioned at some point. Kleinschmitt et al. 2018 got quite large LW forcing values compared to other studies and it would be interesting to see how this is in the model used in this study. I expect that there is not much (absolute) difference between cases where sulfur is injected as SO2 or sulfate (?).

P10, L4. Reduction is seen only in clear-sky forcing but not in all-sky.

P10, L6 As was pointed out by reviewer 1 too, I had to google “surf zone” so maybe it is not that familiar word.

P10, L21. It is better to use 25km instead of 24hPa to be consistent with experiment names.

P11, L2 0.95 -> 0.095 um

P11, L11 compared to . . .

P12, L5 Based on my experience, aerosols are not affecting much on LW fluxes at the surface. This line (“The longwave surface. . .”) can be removed.

P12, L14. “constant climatological SO2”? What does it mean?

P12, L21 Parenthesis thing - same as in abstract

P12, L29-30 Just a comment: I don’t know has this been pointed out in some earlier studies, but if it has, at least I have missed it. This was an interesting remark and it sounds credible. In addition, the size distribution of particles is different in tropical peak compared to higher latitudes.

P13. L18 “...the smaller the negative side effects” Can you really say this? There are several negative side effects which are not studied here.

P14, L29 “are only increased by about 4%” I would remove word “only”. I was surprised that OH concentration was generally increased.

Figures: In addition to reviewer 1 comments please correct following typos: Fig3: Areosol -> aerosol (in upper right) Fig6: Janauary -> January

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