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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative and climate effects of stratospheric sulfur geoengineering using seasonally varying injection areas" *by* Anton Laakso et al.

Anonymous Referee #1

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I recommend major revision or rejection of this paper for the reasons below. The fundamental premise of the paper is wrong, and the impacts of the different injection strategies on the resulting steady-state stratospheric aerosol cloud, including the size distribution of the particles, are not investigated. There are many missing details about the models used.

The entire premise of this study is incorrect. The authors claim that overcooling of the Tropics is because of equatorial injection, but this does not account for two things. The first is the Brewer-Dobson circulation. If aerosols are created in the Tropics, they will be carried poleward by the stratospheric circulation, and the resulting steady-state aerosol distribution will be very smooth. The radiative forcing depends on the interaction of

the aerosol cloud with insolation, not on where it was injected. And the latitudinal distribution of insolation varies much more than that of the aerosol cloud, and it is thin that determines the radiative forcing much more strongly. The second is atmospheric and oceanic circulation that spreads temperature anomalies out, and transports energy latitudinally. So the schemes they have modeled would not be expected to have much impact on the latitudinal distribution of aerosols.

On p. 3, line 27, you describe a scheme to allow time for the aerosols to form, but the gas does not stay in that location just waiting. It gets blown around by the stratospheric winds. You have to do a trajectory analysis to decide where to do the injection. And 4 and 6 months are much too long to wait. Most of the SO2 is converted to sulfate in less than a month.

The paper is full of acronyms that are never defined. Every one has to be defined the first time it is used.

I don't understand the procedure used for the simulations. If the aerosol clouds are produced with a version of the climate model that responds to the radiative forcing from the stratospheric aerosol cloud, then why not use this model for the actual simulations. As was done, the aerosol cloud is prescribed externally and the resulting changes in stratospheric circulation do not affect the aerosol cloud.

How was ozone chemistry addressed? Did the aerosols affect ozone, and did the radiative forcing from ozone depletion affect the climate? Of course, these impacts will vary depending on what years were simulated and what the assumed ODP concentrations were.

What is missing in this paper is an analysis of the sulfate clouds simulated by the different injection schemes. What we need to see is the resulting mean distributions of aerosol amount and size distributions as a function of latitude and altitude. This is what is forcing the model, and how the different injection schemes affect these distributions is fundamental to the rest of the paper. For example, how do microphysics and transport

interact with each other? When varying the latitude as a function of season, does the new SO2 encounter a pristine atmosphere, or are the residual aerosols still there? How does the time to create the aerosols vary for the different schemes? Part of the goal should be to produce smaller particles so as to get more radiative forcing for the same S injection, which would be accomplished by creating new particles rather than making existing particles larger. Did this really happen? It is important to show this. And then, how does this affect the radiative forcing? Without these details, we cannot evaluate whether it makes a difference or not how the injections are done.

How good is the aerosol model anyway? Does it produce the correct spatial and size distributions? It at least needs to be tested on the 1991 Pinatubo eruption.

You should use a different projection for figs. 6 and 8. The one used (Mercator?) makes the poles too large. At least use equal spacing with latitude.

The paper is missing statistical significance testing in parts. For example, are the differences shown in Table 1 significantly different or not?

There are many missing articles (the, a) and the wrong use of prepositions (at, in, for, to). I tried to correct as many as I could, but a native speaker of English should go through and edit the entire paper. I know the article problem exists for native speakers of Russian, Chinese and Japanese. I guess there are no articles in Finnish either.

Note to authors: Three things in your formatting annoy me, and it is not a good idea to annoy reviewers. First of all, use 12 pt font. Such a tiny font is hard to read. Second, separate the references by using hanging indent or extra spacing between each reference. It is very hard to find specific references the way you have it formatted. Third, number the lines sequentially from the beginning and do not start over on each page. If I want to refer to the line number in my comments, I have to first also search for the page number each time. Why make me do that?

The 108 comments in the attached annotated manuscript also have to be addressed.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2017-107/acp-2017-107-RC1-supplement.pdf

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