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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative and climate impacts of a large volcanic eruption during stratospheric sulfur geoengineering" *by* A. Laakso et al.

Anonymous Referee #1

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General

This paper constitutes a model study on the radiative effects of a large volcanic eruption during stratospheric sulphur geoengineering. A global aerosol-climate model and an Earth system model is used in two steps. The detailed aerosol climate model MAECHAM5-HAM-SALSA is used to define global aerosol fields and then the simulated stratospheric aerosol fields from MAECHAM5-HAM-SALSA are prescribed to the Earth system model MPI-ESM. A careful spin-up calculation was performed and a very positive aspect of the study is the use of ensemble runs rather than relying on a single

model run. Also the set of scenarios is appropriately chosen ranging from volcanic eruption only studies to different SRM runs. The quantification of the radiative effects, cooling and also the impact on precipitation constitutes important information. A further strength of the paper is also the investigation of regional effects. The uncertainties of the results are addressed by ensemble studies.

I have one major point, where I do not understand the results of the model simulations: It is not clear to me, why in the model the peak of the stratospheric sulfate burden is reached only five months after the eruption (see also below). I strongly suggest a more detailed discussion of this point, which without further explanation looks like a model artifact to the reader.

In summary, this is a very valuable contribution to ACP. On top of the point mentioned above, I have a number of suggestions for clarifications and corrections (see below). If these comments are addressed appropriately in a revised version, I am convinced that the paper can be accepted for publication.

Detail

It is stated in the paper that under unperturbed conditions, the atmosphere is "almost clean" of particles. I think this is an overstatement. First, it is unclear under which conditions the stratosphere is really unperturbed, i.e. not influenced by small volcanic eruptions. Second, OCS provides a source of sulphur to the stratosphere. Therefore there will always be a Junge layer in the stratosphere, so that "clean" is misleading. In any case, there is no reference here for this statement

Volcanic ash emissions are not taken into account in the study. The argument is that ash particles are deposited fast. However the citation (Niemeier et al., 2009) used to back up this conclusion is a model study, I recommend considering a study based on observations. For example, the eruption of the Chilean volcano Puyehue-Cordón

Caulle in June 2011 emitted a lot of ash that prevailed long enough in the atmosphere to cause interruption of passenger aircraft activity in Australia. In any case, it is not the question how much sulphur is contained in volcanic ash (close to zero) but how much sulphur is emitted in conjunction with the ash. The sulphur contribution is not the same for each eruption – again there should be observational studies here.

The dynamical feedback from the increased stratospheric sulphur load was taken into account. I think this is an interesting point of the study. Could the authors present some discussion on this point?

There is some discussion on the differences between MPI-ESM and the SALSA aerosol treatment and the consequences for the radiative effects. Is there a reference to a study, where these differences have been investigated? Can "somewhat different radiative forcing" be quantified? How well does the scaling to other wavelengths work? Would the scattering radius be a better quantity to use than the effective radius? I suggest further discussion of these issues.

HIRS does not directly observe the stratospheric sulphur burden (p. 21864). I think a bit more information on the satellite measurements should be given here. How do the HIRS values agree with other observations?

It is not clear to me, why in the model the peak of the stratospheric sulfate burden is reached 5 months after the eruption. I can understand how this could be the case for the HIRS observations, where the initial plume has to be diluted somewhat to be properly quantified by measurements. But I do not think that such a behaviour is seen in Lidar observations of the Pinatubo cloud. In the model however there is no substantial source of stratospheric sulphur after the eruption. The only process I can see is the conversion of SO_2 to sulfate, but the timescale for this to happen should be much less than five months. I suggest more discussion of this point.

How is the sulphur lifetime in the stratosphere defined? Burden over loss rate? This quantity could also be a function of time. I suggest further discussion.

There is some discussion of the oxidation of SO_2 in the model (p. 21860). If there is something problematic in the model here, it will have to do with the OH concentrations in the model – correct? Is there any information on the quality of OH in the model from previous studies? OH is a pretty important component in atmospheric chemistry.

The statement that "in July polar vortices are weaker" needs to be corrected. In July there is no polar vortex in the Arctic, solely a solid body (anticyclonic) circulation. The polar vortex in the Arctic (and Antarctic, where of course the seasons are shifted) is only present in the winter/spring period. The transport barrier at the edge of the vortex is indeed most strongly pronounced in winter. Important for the arguments here might also be the seasonal variability of the transport barrier between the subtropics and tropics (there are a number of recent studies on this issue), which should be discussed here.

Minor

- throughout the paper: replace 'volcano eruption' by 'volcanic eruption'
- p 21839, l 9: aerosol aerosol particles
- p 21839, l 25: decades or centuries?
- p 21840, l 1: papers by Robock 2000 'and' Timmreck . . .
- p 21841, l 21: a number the number
- p 21841, l 22: a number concentration one number concentration
- p 21842, l 14: a sea sea
- p. 21845, l 14: citation for 8.5 Tg?

- p. 21850, l 14: quantify ‘faster’
- p. 21851, l 4: restarted
- p. 21851, l 21: quantify ‘quite large’
- p. 21852, l 2: Isn’t ‘cooling’ a radiative effect’ – I think I know what you mean but the point could be made a bit clearer here.
- p 21853, l 3: to low at low
- p 21855, l 17: reduction compared to what?
- p 21859 l 24: poleward transport of what?

Remaining from initial review

Throughout the paper there are little issues in the text where a "the" is missing or should be replaced by "a". Please correct.

In acknowledgements: Julich Jülich; also Center ‘of’ Climate . . . (l. 7, p. 21864)

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 21837, 2015.