We thank Anonymous Referee #1 for suggestions and comments, which helped us clarify some parts of the manuscript. Our point by point answers to the comments are presented below. Referee comments are in bold and our replies in body text.

Referee #1 comments:

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.

We strongly disagree with the referee that the premise of the paper would be somehow wrong, for the reasons outlined below. On the other hand, the referee's suggestion to include information on the particle size distribution is well taken and addressed below. We have also included more 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 reason for the overcooling is two-fold, and this is now discussed more explicitly in the manuscript: Firstly, as the referee correctly points out, the tropics receive on average much more insolation than the mid and high latitudes. This would in itself lead to a maximum forcing in the equatorial region, assuming that the aerosol distribution were globally uniform. However (and secondly), localized or regional stratospheric injections do not lead to a globally uniform particle distribution, as has been shown by several earlier studies (English et al 2012, Niemeier et al 2011, Jones et al 2016). This also applies to the equatorial injections. While atmospheric circulation does transport some of the emitted sulfur away from the injection region, the particle burden still remains much higher close to the injection region than very far away from it. This is also clearly demonstrated in our Figures 4 a) and b) for injections in the tropics.

Therefore, the non-uniform radiative forcing pattern from equatorial injections is a result of both the uneven insolation pattern and the uneven aerosol distribution. While SRM cannot do anything about the former, it can have some control over the latter. Thus, investigating the impacts of different injection strategies is a highly relevant question, as also highlighted by MacMartin et al. (2016) in their recent review article listing open research questions after a decade of investigation.

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.

Atmospheric and oceanic circulation does indeed spread temperature anomalies out, as was seen also in our study: the differences in radiative forcings between scenarios did not directly translate into differences in climate variables (e.g. temperature). This was also the motivation to use an Earth System Model in this study instead of concentrating only on the aerosol radiative effects. However, our results clearly show that in some areas different injection scenarios would lead to significant differences also in temperatures (Fig 8).

Overall, the results presented in the manuscript show that the radiative forcing and resulting climate effects are dependent on the injection scenario used. Our study shows that different injection strategies lead to a very different zonal forcing which is in contradiction to this referee claim. Hence, we disagree with the referee's claim that the premise of our study would be somehow incorrect.

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.

We are a bit puzzled what the referee refers to here. The aerosol and gas phase chemistry schemes of this study are fully interactive with the ECHAM dynamical core, i.e. the aerosol particles and there precursor gases (both SO2 and its oxidation product H2SO4) are transported in the atmosphere according to ECHAM's advection scheme. The step-wise conversion of SO2 first to gas-phase H2SO4 and subsequently to particle-phase sulfate is calculated explicitly during the transport. Hence, the modelled SO2 does not stay in any location "just waiting" nor does its conversion to sulfate take 4-6 months in our simulations.

We added to the text the lines:
"*Based on the oxidation time of SO2, p4 and p6 scenarios are expected lead to a smaller radiative forcing than p2. However, these scenarios are simulated to study how the phase of the changing injection area alters the radiative forcing.* "

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

Some of the acronyms, like model names, are actually better known than the full model names and thus quite often only short names are used. However, we have now included also the full names in the manuscript.

(Max Planck Institute's Earth System Model) - added to page 2

MAECHAM6.1-HAM2.2-SALSA, The middle atmosphere configuration of the European Centre Hamburg Model coupled with Hamburg Aerosol Model including a Sectional Aerosol module for Large Scale Applications
- added to page 4.

Aerosol Comparisons between Observations and Models - added to page 5
Coupled Model Intercomparison Project - added to page 5
Aerosol Chemistry Climate Model Intercomparison Project - added to page 5

GCM changed to *General circulation model* - at page 15

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.

There are two main reasons for the chosen model strategy. First, the configuration of the MPI-ESM that we used in our simulations does not include a prognostic calculation of aerosol properties, as was stated in the manuscript on page 4.

The second reason for using precalculated aerosol properties in the ESM simulations is that modelling of aerosol microphysics is computationally heavy and for the purpose of this study the benefits are expected to be small. It is therefore feasible to simulate aerosol microphysics only for a relatively short period of time (5 years) and then use these defined aerosol fields as prescribed fields in longer climate simulations (3 x 80 years) with MPI-ESM.

It is true that the changes in the atmospheric circulation due forcings given by RCP45 scenario are not taken account in the aerosol simulations and may affect the transport of aerosol so that they would differ from those simulated by ECHAM-HAMMOZ. However, it has to be noted that many of the GeoMIP simulations (Tilmes et al 2015, Xia et al., 2016) use methods similar to those presented here.

We added following text to end of section 2.2
"In addition, modelling aerosol microphysics is computationally heavy. Thus it was feasible to simulate aerosol microphysics only for a relatively short period (few years) and use the ECHAM-HAMMOZ simulated aerosol fields as prescribed fields in the longer simulations in MPI-ESM. Simulations with ECHAM-HAMMOZ were carried out using a free running setup to include the dynamical feedback resulting from the additional heating due to absorption radiation by the injected aerosols. However, stratospheric circulation could also be altered by changes in the atmospheric GHG concentration (in our case following the RCP4.5 scenario) and its impacts on the tropospheric climate; however, these impacts were not taken into account when the aerosol fields were calculated in ECHAM-HAMMOZ."

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.
Ozone chemistry is not included in our model configuration. The following text was added to section 2.2.1: "The *hydroxyl radical (OH) and ozone concentrations are accounted for through prescribed monthly mean fields. Thus, the effect of sulfur injections on the ozone layer is not simulated in our model*.".

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

The question of size distributions was raised by several referees, and hence we added two more figures (Fig. 3 and 6) to the manuscript as well as a new section 3.1.2 to discuss this topic. Figure 3 now shows the zonal mean effective radius in most of the studied scenarios for different seasons. Note that we use the effective radius to describe the aerosol size instead of showing the full size distribution. For example in English et al 2012 the aerosol size distribution was presented at the Equator. However, here the injection areas vary between different scenarios, and thus the objective evaluation of differences would be extremely complicated. With effective radii, the size distribution can be described by a single value and differences between regions and time can be evaluated more easily. The differences in the altitude where the particles are located are small and thus only zonal mean of effective radii is included.

In addition, Figure 6 shows the time dependent zonal mean of AOD at 533nm and together with the new figure 3, these figures answer most of the questions raised by the referee. The average time for the oxidation of SO2 can be seen in the table 1. Separating a contribution of microphysics from all the other factors which are affecting the radiative forcing is challenging. However, some qualitative evaluation can be done based on the AOD and figure 6.

In addition to the completely new section 3.1.2, the following pieces of text were added to section 3.1.1 :
"*Thus the particle effective radius is clearly smaller in scenario NHSH than in scenario NH, especially in the northern hemisphere (Fig 3). "*

“As a result, the number concentration of smaller particles increases. Figure 3 shows that the particle effective radius is on average smaller in scenario p2 than in EQ.”

“*Figure 3 shows that in scenario p2w particles are consistently smaller than in p2. However, due to the atmospheric circulation, which transports particles mainly towards poles, in scenario p2w particles are removed more quickly from the atmosphere because sulfur is injected at a larger distance from the equator. Thus there is no difference in stratospheric sulfur burden between p2 and p2w scenarios (Table1).* “

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.

The model version used in this study has been evaluated against observations of the Mt Pinatubo eruption as well as against other modelling studies, and has been shown to reproduce realistic aerosol loads and properties (Laakso et al 2016). Furthermore, previous studies with a simpler modal aerosol scheme (M7) have also shown that ECHAM-HAM is capable of capturing the main mechanisms and features of stratospheric aerosol evolution (Niemeier et al. 2009, Toohey et al 2011). Kokkola et al. (2009) showed that the sectional aerosol module (SALSA), which is used in this study, outperforms M7 in conditions where both high and low SO2 concentration conditions are present simultaneously.

We added text “*The model has been shown to simulate the stratospheric aerosol loads and radiative properties consistently compared to observations of the Mt Pinatubo 1991 eruption as well as other models (Laakso et al., 2016).*” to section 2.2.1.

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.
Map projections of the figures have been changed from Miller to Robinson. The hatching now shows areas which are not statistically significant (instead of significant differences)

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

We added variances for the temperature and precipitation values and warming rates. All the results in Table 1 are statistically significant.

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.
We thank the reviewer for these corrections. We have done our best to correct this issue. The final text will be polished off by the ACP copy-editing team.

In addition to comments presented here, supplement comments are taken into account in the revised manuscript.

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?

We hope that Copernicus Publications takes note of this comment. The manuscript was prepared using the Copernicus Publications Word template (docx) (http://www.atmospheric-chemistry-and-physics.net/Copernicus_Word_template.docx) as advised by the journal. It defines the font size, style of references and also numbering of lines.

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