Reply to the comments from Anonymous Reviewer-2

The authors simulate the solar radiation method (SRM) of stratospheric sulfate aerosols by prescribing a uniform layer of sulfate aerosol concentration. Assuming different altitudes of this layer, they determine the impact on different variables like radiative forcing, surface temperature, and humidity and temperature in the stratosphere. The paper is very well written and includes an impressive literature review. I recommend publication after the authors addressed the following comments.

We thank the reviewer for the constructive comments and the time spent on reviewing the manuscript.

General comments

My main concern is related to the prescribed aerosol layer. Prescribing aerosols in a climate model is a widely used technique and can be used to be computationally more efficient. However, the prescribed layer should be comparable to reality and preferable been calculated previously with a aerosol microphysical model. This study simplified the method by prescribing the aerosol concentration meridionally uniformly at a certain height (km) above sea level. This distribution is quite unrealistic. However, this may not have extremely strong impact on the main conclusion of the paper, the dependency of radiative forcing on altitude.

Stronger impact has the fact that the prescribed profiles do not include the changes caused by sedimentation. Sedimentation causes a vertical spread of the aerosols, the stronger the higher the injection altitude above the tropopause (e.g. (Tilmes et al. (2017)). This difference to a more realistic profile is not mentioned in the paper. As a consequence, heating of the aerosols will not occur in a small layer as shown in Figure 6a and 7. The heated layer extends to the tropopause. This has consequences for clouds and humidity transport. This aspect is not at all discussed in the paper.

I agree with the authors that a simplification is useful and can help to gain new insight. The dependency of the radiative forcing on the altitude of the SRM agent has been shown for three different SRM techniques by Niemeier et al (2013). Sulfur injection studies have shown that the resulting radiative forcing depends on the altitude of the injection. This has mostly been related to the vertical extension of the aerosol layer. This paper could give new insights by adding a simulation with more realistic sulfate profile. This would show whether the altitude of the injection or the vertical extension of the aerosol layer is the cause of an increased radiative forcing.

The limitation of the model, no aerosol microphysics, are well described. The more important limitation of the prescribed sulfate concentration are not prescribed. Please, add a figure of the prescribed sulfate layers in the main article. For a reader who is not familiar with stratospheric sulfate distribution it would be very helpful.

Thanks for these comments. We agree that sedimentation and a realistic distribution of aerosols in the vertical and related radiative heating distribution is important and they are missing in our study. However, we believe, as the reviewer has also pointed out, that the qualitative and fundamental effects related to the height of the aerosols will not be altered. In this paper, our main aim is to investigate sensitivity to the height of aerosol layer and not the height of aerosol injection which brings additional complexity by spreading the aerosols in the vertical. We plan to use realistic vertical distributions corresponding to various heights of injections in a future study. These limitations are discussed in a paragraph in the last section in the revised manuscript. The height sensitivity studied by Niemeier et al (2013) is discussed in the introduction. The figure as suggested by the reviewer is also included in the revised manuscript (Figure 1 in the revised manuscript).

Specific comments

Page 3 Line 27: I don't understand this sentence.

We rewrote the sentence in the revised version to remove the ambiguity. It is revised as "The altitude of the prescribed aerosol layer can thus affect climate depending on the proximity of the heated layer to the tropopause as heat exchange between stratosphere and troposphere can lead to changes in clouds and stratospheric water vapor"

Section 2.2: How is the sulfate layer created? The global distribution in Nalam et al (2017) is unrealistic. Aerosols follow tropopause in a meridional cross section (e.g. Visioni et al. (2017) or Dhomse et al. (2014)). Add figures of sulfate distribution. It remains unclear how the sulfate is distributed vertically. I assume you only change the height of the prescribed layer.

Yes, the aerosols are added in single layers as discussed at the end of the first paragraph in section 2.2. See our response to the general comments. A figure showing the aerosol distribution is also included in the revised manuscript.

Page 4 Line 22: 20 Mt SO4? Change unit to Tg (SI unit)

We have modified the units throughout the manuscript to Tg.

Page 6 Line16: Thus, CO2 reduction would be most effective. This would be a good statement in the conclusion.

Thank you for the suggestion. Yes, the efficacy of sulfates is less than one and for equivalent change in radiative forcing CO_2 reduction will be more effective. We discuss this in the revised manuscript.

Page 7 Line 16 - 26: Results of Kuebbeler et al (2012) should be taken into account.

The results from Kuebbeler et al (2012) are now discussed in the revised manuscript in section 3.2.

Page 8 Line 30: See also Liepert and Prevedi (2009) and Kravitz et al (2013).

Thank you for the references. We have cited these references in the revised version.

Page 8 Line 27pp: This is a very short summary of this topic.

We have added more discussion on fast adjustments and related precipitation in this section in the revised manuscript.

Page 9 Stratospheric dynamics: The vertical resolution of you model is very limited. Good results of stratospheric dynamics need a higher resolution, even in a model that is still not capable to generate a QBO. You should mention the QBO in the first paragraph and not only cite the two papers.

We have added more discussion in the first paragraph of section 3.4 in the revised version to discuss this limitation of our model.

Page 9 Line 27: You show significance for the temperature. How about the zonal wind? Your differences are very small. Please add a figure with significance of the zonal wind.

As suggested by the reviewer, a sperate figure of zonal wind is added in the revised manuscript (Fig-7 in revised manuscript). The statistically significant wind changes shown as hatched areas in the figure.

Page 9 Line 29: Higher temperature in the tropics because of more solar radiation. The aerosols cannot absorb as much radiation during polar winter then during summer.

We have modified the line to include this information.

Page 10 Line 3: Significant?

Yes. Significant at 5% significance level, as shown in the revised figure 7.

Page 10 Line 4 to 10: Your vertical profile of aerosols is unrealistic. As a consequence the vertical profile of temperature anomaly and humidity anomaly are unrealistic. You have to say this and the consequences for your results.

We have discussed the consequence of the unrealistic aerosol profile on temperature and humidity distribution in a paragraph in the last section in the revised manuscript.

Page 12 Line 5 to 6: I don't understand this sentence? What would be the relation between microphysics and global mean surface temperature?

The aerosol microphysical changes can affect the optical and radiative properties of the aerosols through nucleation, condensation, coagulation, hygroscopic growth, etc (e.g. Heckendron et al., 2009). The changes in the optical and radiative properties affects the radiative forcing and thereby influences the surface cooling efficiency in the aerosol geoengineering schemes. This is discussed in the revised manuscript.

Page 12 Line 15-16: This is not a new result and can be found in many previous studies. I don't think there is a tendency to include radiation to aerosol coupling for SW only in the models, at least not in general.

We agree with the reviewer that this result is not new. We delete the phrase "and the need for carefully accounting LW forcing along with the SW forcing"

Page 12 Line 25 Many studies show that higher altitude of injection causes stronger forcing. Most studies relate this to the thicker aerosol layer. Niemeier et al. (2013) have show that for different SRM methods the forcing depends on the altitude of the forcing agent. Thus, your result is not new. It remains open, whether the top level of the aerosols or the vertical extension of the sulfate layer is more important. Your study would gain a lot if you could answer this by adding a simulation with a more realistic vertical profile.

We appreciate the comment. We have discussed the results from Niemeier et al. (2013) in the introduction of the revised manuscript and the discussed the limitation due to vertical aerosol extension in the last section. Previous studies have estimated the climate sensitivity to altitude mainly based on the sedimentation effects and transportation. In our approach, the primary focus was to estimate the changes in effective radiative forcing due to changes in fast adjustment processes (e.g., Boucher et al., 2017) when the aerosol induced warm layer moves away or closer to the tropopause. As mentioned in response to the general comments, we plan to study the effects of realistic profile in a future study.

Page 13 Line 15 to 25: As stated above, the main limitation is the profile itself. The profile changes when injecting at higher altitude and the particle sediment. This has to be reflected in your profile and would change e.g. vertical humidity transport.

We agree. We have discussed this limitation and its consequence related to the idealized profile in a paragraph in the revised "Discussion and conclusion" section.

References:

Boucher, O., Kleinschmitt, C. and Myhre, G.: Quasi-Additivity of the Radiative Effects of Marine Cloud Brightening and Stratospheric Sulfate Aerosol Injection, Geophys. Res. Lett., 44(21), 11,158-11,165, doi:10.1002/2017GL074647, 2017.

Heckendorn, P., Weisenstein, D., Fueglistaler, S., Luo, B. P., Rozanov, E., Schraner, M., Thomason, L. W. and Peter, T.: The impact of geoengineering aerosols on stratospheric temperature and ozone, Environ. Res. Lett., 4(4), 045108, doi:10.1088/1748-9326/4/4/045108, 2009.