“Shortwave radiative forcing and feedback to the surface by sulphate geoengineering: Analysis of the Geoengineering Model Intercomparison Project G4 scenario” by Hiroki Kashimura et al.

Response to the Referee #1

Dear Referee#1

We thank the referee for a carful review and constructive comments. Please find below the authors’ response. In this reply we denote referee's comments and questions using blue; our responses are in black and relevant text in the manuscript in Times font with changes shown in red.

We revised the title of the manuscript as
“Shortwave radiative forcing, rapid adjustment, and feedback to the surface by sulphate geoengineering: Analysis of the Geoengineering Model Intercomparison Project G4 scenario”
following another reviewer’s comment.

Kashimura et al. determine the shortwave radiative forcing at the surface of stratospheric sulfur injection (SAI) and it’s changes due to clouds. They use results of experiment G4 of the geoengineering model Intercomparison project, constant injection of 2.5 Tg(S)/y, of six different models for the study. They apply a single-layer model of short-wave (SW) radiation to estimate the feedbacks caused by the reduced incoming SW radiation due to the scattering sulfate aerosol layer. This is a strong simplification but it allows to differentiate between different cloud feedbacks.

It is important to know the rate of SW reduction at the surface to estimate the impact of geoengineering. The single-layer model provides information on the impact of SAI on clouds and the study highlights the differences between the models. A comparable study has not previously been performed. I recommend the publication of this work after considering the following remarks.

General:

Kashimura et al. concentrate on SW radiation. However, stratospheric sulfate aerosols absorb long-wave (LW) radiation, which heats the stratosphere. This reduces the efficiency of SAI. The injection rate, necessary to counterbalance a certain anthropogenic forcing, is determined by the top of atmosphere forcing imbalance, not by the SW radiation at the surface. Therefore, the LW absorption is important and the role of LW radiation needs to be discussed. The relevance for the presented results should be described in more detail.

=>We agree that the LW absorption by the stratospheric aerosols is important for studying SAI. However, there are many interactions among LW, temperature, and various other components of the climate system through the emission and absorption of LW. Because of such complexity, unlike the SW effects that we have explored in this study, it is difficult to distinguish and estimate the LW effect of each process.
We carefully consider how to include an analysis and discussion about LW radiation, and decided to estimate the rapid adjustment (or response) of LW radiation in the clear-sky,

by using a method similar to the Gregory plot. (Note that another referee requested to distinguish *rapid adjustments*, which is independent on ΔT, and *feedbacks*, which is proportional to ΔT, from what we call "feedback effect" in the previous manuscript; and a method similar to the Gregory plot was added to the revised manuscript.) The LW rapid response should include, at least, the effect of LW absorption by the stratospheric sulphate aerosols and that of the rapid adjustment of the water vapour. We added such analysis and discussion on the new Section 4.2 as follows.

Page 14–15

Section 4.2 Rapid adjustment of longwave radiation

This study has concentrated on SW for the reasons described in Section 1; however, it may be valuable for some readers to mention the role of LW. A well-known effect of LW in the sulphate aerosol geoengineering is heating of the stratosphere. The sulphate aerosols induced by the SO_2 injection absorb LW and heat the stratosphere (e.g., Heckendorn et al., 2009; Pitari et al., 2014). For the energy budget at TOA, increase of the LW absorption results in decrease of the outgoing LW, which manifests as a heating of the climate system. Needless to say, there are many interactions among LW, temperature, and various other components of the climate system, through the emission and absorption of LW. Because of such complexity, unlike the SW changes that we have explored in this study, it is difficult to distinguish and estimate the effect of each factor on LW changes.

One possible and useful analysis for LW is to estimate the rapid adjustment (or response), which is independent of ΔT, by the same method used in Section 3.4. Gregory-like plots are made for the difference of net LW for clear-sky at the surface (ΔLW_{SURF}^{CS}) and at TOA (ΔLW_{TOA}^{CS}) as shown by black "+" signs and red "x" signs, respectively, in Fig. 11. The rapid adjustment in the clear-sky at the TOA shown by the y-intercept of the ΔLW_{TOA}^{CS} regression line shows a heating effect of about 0.57 Wm^{-2} in the multi-model mean. This rapid adjustment should mainly consist of the effect of LW absorption due to the stratospheric sulphate aerosols, since the decrease of the water vapour suggested by the rapid adjustment of EWV yields less LW absorption and an increase in outgoing LW at TOA (i.e., sense of cooling). It is important to take this heating effect in mind when we consider the energy budget at TOA for the sulphate geoengineering. Though the sulphate aerosols' LW effect is significant at TOA, such effect might become less significant at the surface, because the rapid adjustment estimated from ΔLW_{SURF}^{CS} is small compared to the SRM forcing and total reactions at the surface.

A second aspect which is not or only shortly discussed is the meridional distribution of the aerosols. The two models coupled to an aerosol microphysics show most probably different distributions. This has a clear impact on the forcing (English et al. (2013), Niemeier and Timmreck (2015)).

=>Because HadGEM2-ES calculates sulphate aerosols both in the stratosphere and troposphere in the same way and does not output the *stratospheric* sulphate AOD separately, we cannot obtain AOD due to the SO_2 injection accurately. The difference (G4 – RCP4.5) of the sulphate AOD, which is the sum of the AOD in the troposphere and that in the stratosphere, may give an *approximate* distribution of the stratospheric sulphate AOD in G4, but a fair comparison with the prescribed AOD and MIROC-ESM-CHEM-AMP is impossible. For readers who want to refer to the *approximate* AOD distribution in HadGEM2-ES, the stratospheric sulphate AOD distribution in MIROC-ESM-CHEM-AMP, and the prescribed AOD, we provide a figure (Fig. S1) as a supplemental file. This figure shows that the difference in the globally averaged amount of AOD should be significant rather than the meridional distribution of the AOD. This is newly mentioned in the manuscript as follows.

Page 10, line 11–16
It is the difference in the mean AOD rather than its meridional distribution as shown in Fig. S1 that leads to the underestimation of the AOD in G4. The globally and temporally averaged stratospheric sulphate AOD in MIROC-ESM-CHEM-AMP is 0.083 and that in HadGEM2- ES is approximately 0.054, though that of the prescribed AOD is 0.037. Note that the above value for HadGEM2-ES is the difference (G4 – RCP4.5) in the sulphate AOD for both troposphere and stratosphere because HadGEM2-ES does not calculate the sulphate aerosols in the tropospheric and stratosphere separately.

The importance of the particle size is not mentioned at all. Scattering of SW radiation decreases with increasing particle size (Pierce et al. (2010)). Is the particle radius similar in the models prescribing the AOD? Do the two aerosol models simulate similar AOD?

=>We added a sentence mentioning the importance of the particle size in Introduction. In addition, we added a paragraph describing the particle size of participating models in Section 2, and we added the particle sizes to Table 1.

Page 3, line 17–19, Section 1
Even though the prescribed AOD is given, a difference in an assumed particle size for the stratospheric sulphate aerosols causes difference in the SRM forcing (Pierce et al. 2010).

Page 4, line 31–page 5 line 5, Section 2
The mean stratospheric sulphate aerosol particle sizes and standard deviation of their log-normal distribution (σ) in each model are also shown in Table 1. In HadGEM2-ES, the tropospheric aerosol scheme and the associated microphysical properties (Bellouin et al. 2011) is simply extended into the stratosphere. Modifications to the stratospheric aerosol size distribution have been applied in subsequent HadGEM2-ES studies (Jones et al. 2016a,b), but have not been applied here. In MIROC-ESM-CHEM-AMP, the microphysics module for stratospheric sulphate aerosols treats them in three modes as shown in Table 2 in Sekiya et al. (2016); however, to calculate radiative processes on the aerosols, a particle size of 0.243 μm is assumed for simplification. In addition, the microphysics of the tropospheric sulphate aerosols is not calculated in MIROC-ESM-CHEM-AMP to avoid drift in the simulated climate.

These aspects will not change the presented results but may provide some additional explanation of differences.

Introduction:

Line 16: Rasch (2008) and Robock (2008) do not use full aerosol microphysics. E.g. Rasch (2008) prescribe the aerosols.

=>For Rasch (2008) and Rohbock (2008), we understood that the particle size distribution was not internally calculated but prescribed in their model. We added the following sentence to mention this.

Page 2, line 18–19, Section 1:
The models used in these two studies include formation, transportation, and removal of the stratospheric sulphate aerosols, but the particle size distribution was prescribed.

There are several more recent studies available: e.g. Heckendorn et al. (2009), Pierce et al (2010), English et al (2013), Niemeier and Timmreck (2015) all with full aerosol micro-physics.

=>Thank you for giving us useful info. We cited Heckendorn et al. (2009), Pierce et al. (2010), and Niemeier and Timmreck (2015). English et al (2013) was not cited because it is a study about large volcanic eruptions.

Page 2, line 19–23, Section 1
Heckendorn et al. (2009) and Pierce et al. (2010) calculated full microphysics of sulphate aerosols with an assumption of zonally homogeneous conditions. They simulated 2–20 Tg yr^{-1} SO_2 injection with a present day (year 2000) condition run as their reference simulation. Niemeier and Timmreck (2015) used models with full microphysics of sulphate aerosols, and performed a sulphate geoengineering experiment with SO_2 injection rates of 2–200 Tg yr^{-1} to counteract the anthropogenic forcing of RCP8.5.

They may provide information of the LW impact. Impact of LW radiations, particle size and meridional distribution might be discussed in the introduction.

=>We added sentences mentioning the importance of the particle size and meridional distribution in the introduction as follows.

Page 3, line 15–19, Section 1
On processes related to the SRM forcing, modelled aerosol microphysics including formation, growth, transportation, and removal may differ, and such differences result in the difference in meridional distribution of the aerosol optical depth (AOD). Even though the prescribed AOD is given, a difference in an assumed particle size for the stratospheric sulphate aerosols causes difference in the SRM forcing (Pierce et al., 2010).

Importance of LW radiation was introduced and discussed in the new Section 4.2. We consider that discussing the LW radiation in the introduction will impair the flow of sentences.

Methods:
Page 5 end of the page: 'effect on the absorption rate is negligible'. The absorption in the near infrared should be discussed prior to this point.

=>We carefully consideredthis suggestion, but to discuss influence of the near infrared radiation quantitatively, we need some measures introduced in Section 2. Therefore, this cannot be discussed at this point, and we kept the discussion about the near infrared radiation at the end of the manuscript.

Results:
Line 6: 'for a few decades' Please be a bit more specific.

=> Expression was changed to "10–25 years".

Line 17: You discuss at the end the problem of comparing ensemble mean data to single model results. This came to my mind already here.

=>Because inserting the discussion here will break the flow of the sentences, we added a sentence announcing that the discussion is given in Section 4. Here, Section 4 is newly added for Discussion.

Page 9, line 3–4, Section 3.1
One concern is that half the models used in this study have only one ensemble member, and half are MIROC-based models. The effects of this are analysed in Section 4.3 and shown to be relatively unimportant.

Page 8:
Line 10: 'except MIROC-ESM-CHEM-AMP' Why?

=> This reason was described in the next subsection. We added a short note to announce this to readers.

Page 9, line 25–26, Section 3.2
The strengths of E_{WV} and E_C are comparable in each model except MIROC-ESM-CHEM-AMP (a reason for this exception is discussed in the next subsection).

Line 22: cooling and heating effect: You may better name it positive and negative forcing.

=>We carefully consider this and also from the other comments, we recognized "cooling/heating" is misleading, since the decrease/increase of SW at the surface does not necessary causes cooling/heating of the surface air temperature in total (including the effects of LW radiation etc.). However, the expression "positive and negative" may also confuse readers because, one may read "positive" as "plus in sign in amount" or "direct proportion to ΔT" when the word is modifying feedback effect. Hence, we revised the expression of "cooling/heating" that was modifying feedbacks to, for example, "decrease/increase of net SW at the surface" through the manuscript. We consider "cooling" used for the SRM forcing and temperature is not misunderstandable, so that we remained such expression in the manuscript.

Line 26: How are the modes of the aerosol module set up? Do you use the same mode width as described in Sekiya (2016)? The injection strength under geoengineering conditions is smaller compared to a volcanic eruption. This may cause

=>We used the same mode as Sekiya et al. (2016) for stratosphere, but unlike Sekiya et al. the calculation of the sulphate microphysics was not performed in the troposphere to avoid an unexpected drift of the simulated climate and keep the climate in MIROC-ESM-CHEM-AMP in RCP4.5 similar to that in MIROC-ESM-CHEM. This info is now described in Section 2.

Page 5, line 1–5, Section 2
In MIROC-ESM-CHEM-AMP, the microphysics module for stratospheric sulphate aerosols treats them in three modes as shown in Table 2 in Sekiya et al. (2016); however, to calculate radiative processes on the aerosols, a particle size of 0.243 μm is assumed for simplification. In addition, the microphysics of the tropospheric sulphate aerosols is not calculated in MIROC-ESM-CHEM-AMP to avoid drift in the simulated climate.

Line 28/29: Why do they differ? Horizontal distribution, particle size?

=>We checked the sulphate AOD in MIROC-ESM-CHEM and HadGEM2-ES, and compared them with the prescribed AOD. We found that the reason for the underestimate is the estimated mean amount of the AOD rather than the qualitative difference in the meridional distribution as shown in Fig. S1. We added the following sentences to the manuscript and added a new figure as a supplement. Unfortunately, we cannot separate the *stratospheric* sulphate AOD from the output data of HadGEM2-ES, since it does not distinguish sulphate aerosols in the troposphere and stratosphere.

Page 10 line 11–16, Section 3.3
It is the difference in the mean AOD rather than its meridional distribution as shown in Fig. S1 that leads to the underestimation of the AOD in G4. The globally and temporally averaged stratospheric sulphate AOD in MIROC-ESM-CHEM-AMP is 0.083 and that in HadGEM2- ES is approximately 0.054, though that of the prescribed AOD is 0.037. Note that the above value for HadGEM2-ES is the difference (G4 – RCP4.5) in the sulphate AOD for both troposphere and stratosphere because HadGEM2-ES does not calculate the sulphate aerosols in the tropospheric and stratosphere separately.

Line 33: I would expect that the average over time of the AOD is similar between the ensemble members. You may explain this better if you show a zonal mean of the AOD for the two models and, in case they differ, the ensemble members.

=>Here, we said that CanESM2 and MIROC-ESM-CHEM have no differences in SRM forcing among ensemble members, but HadGEM2-ES has. The expression might be confusing, so that we slightly changed the word. For HadGEM2-ES, we drew the mean seasonal cycles of the stratospheric AOD and attached them as a supplement file. It is clear that even averaging over 30 years, the meridional distribution of the stratospheric AOD differs among the ensemble members for HadGEM2-ES. We added the following sentence.

Page 10, line 21–22, Section 3.3
Even after averaging over 30 years, the mean seasonal cycles of the sulphate AOD can differ among the ensemble members as shown in Fig. S1.

Page 9:
1st sentence: 'varies from....' between the models.

=>The expression was added as suggested.

Page 10, line 23–24, Section 3.3
Pitari et al. (2014) have shown that SW radiative forcing at the tropopause calculated off-line by a radiative transfer code (Chou and Suarez, 1999; Chou et al., 2001) varies from around –2.1 to –1.0 W m^{-2} between the models.

Page 10:
Line 3 and 4: You list many regional details. Can we trust the model in this detail?

=>Grid intervals of the models are equal to or narrower than 2.8125 deg, so that the mentioned regions are well resolved in the model. However, properties of the Sea of Okhotsk and Hudson Bay may depend on related channels, which may be not well resolved. We added the following sentences to note about this.

Page 13, line 15–17, Section 3.5
Here, model grid intervals are equal to or narrower than 2.8125 deg, so that the geographical regions mentioned above are represented by enough grid points. However, properties of the Sea of Okhotsk and Hudson Bay may depend on related channels, which may be not well resolved.

Line 31: The difference in meridional distribution of the aerosols are an notable aspect. However, this is important in modeling because the model results differ. So the different results show possible behavior of nature. Which of them represents nature best is another question.

=>For the present anyone cannot answer, “Which of them represents nature best?” because there are no field experiments on SAI in the global scale and a long period. Comparison with the observational data of volcanic eruptions is useful but there are significant difference between SAI and natural volcanic eruption (e.g., continuity of injection, amounts and particle sizes of aerosols).

Page 10/11:
Do the results agree with previous studies?

=>Geographical distribution of ΔT agrees with previous studies (e.g., Robock et al., 2008), and that of E_{wv} is consistent with decrease of precipitation reported by Rasch et al. (2008) and Robock et al. (2008). For other measures, we could not find the previous studies that can be fairly compared with this study (i.e., simulation of sulphate geoengineering; not by reducing the solar constant.). We added the following sentences to mention that our result of ΔT and E_{wv} are consistent with previous studies.

Page 12, line 10, Section 3.5
Such features agree with previous studies such as Robock et al. (2008).

Page 13, line 11–12, Section 3.5
The slight increase of E_{WV}, which implies less water vapour, in the equatorial region is consistent of decrease of precipitation reported by Rasch et al. (2008a) and Robock et al. (2008) under SRM.

Discussion:
Page 11:
Line 18-20: You may add references.

=> We added Rasch et al., (2008b) and Kremser et al., (2016) for the references.

Page 15, line 30–Page 16 line 2, Section 5
Inter-model variations comprise a substantial range, and narrowing this uncertainty is essential 30 for understanding the effects of sulphate geoengineering and its interactions with chemical, micro-physical, dynamical, and radiative processes related to the formation, distribution, and shortwave-reflectance of the sulphate aerosols introduced from the SO_2 injection (Rasch et al., 2008b; Kremser et al., 2016).

Page 12:
Line 10 to 15: This is a serious concern. Would your results differ when you use one

simulation of each model, e.g. always r1? You can test this to give a less broaden statement here.

=>As suggested, we tested how the multi-model mean results differ when using r1 data only. This result is shown in Fig. S2 in the supplement file. We also checked how the multi-model mean results differ when adding a weight of 1/3 to MIROC-based models to remove the bias that 3 out of 6 models is the MIROC-based model. This result is shown in Fig. S3. In both cases, we did not find significant difference compared with Fig. 9 in the manuscript, so that we can state that inequality in the number of ensemble and participating models have no significant effects to our results. These are described in the new Section 4.3.

Page 15, line 4–12, Section 4.3
4.3 Inequality in the number of ensemble and participating models
One concern in this study is the half of the models used have only one ensemble member, and half are MIROC-based models. Because the numbers of ensemble members differ among models as listed in Table 1, each member in each model is not equally weighted in calculation of the multi-model means described in Section 3.5. Responses to the SRM forcing in the three MIROC-based models should be similar to each other as shown in Fig. 6, so that the results of multi-model mean can be biased to that of the MIROC-based models. Therefore, we re-calculated multi-model means are calculated by using only one run for each model (Fig. S2), and also tested multi-model means with a weight of 1/3 multiplied for the MIROC-based models (Fig. S3). There are no significant difference among Figs. 9, S2, and S3. Therefore, inequality in the number of ensemble and participating models has no significant effects on our results.

Figure 7:
Line thickness differs in the zonal mean plot. Does this show ensemble mean and single results? Please note it somewhere.

=>Black line is thicker than others, because black line shows the multi-model mean. Other coloured lines have the same thickness. We add “thick” and “thin” in the expression.
The ensemble mean and single results are not distinguished by line thickness. Readers need to remember which model has an ensemble, but we think this is not difficult for the readers.

Caption of Fig. 8: the black thick line on the right-hand side shows the zonal mean of the multi-model mean. Other coloured thin lines display the ensemble mean

You hatch regions were the models agree. Do you mean disagree? The hatching is so strong that it would make no sense to hatch the regions were the models agree.

=>Hatching indicates the region where 2 or more models disagreed on the sign. Namely, the region where 6 all models show the same sign and where 5 models show the same sign are not hatched, but the regions where only 4 or 3 models show the same sign are hatched. The previous expression might be unreadable, so that we changed the expression as follows:

Caption of Fig. 8: Hatching indicates the region where 2 or more models (out of 6) disagreed on the sign of the difference.

What do you mean with ’The color tone shows the horizontal distribution’?

=>This is just an expression problem. We mean colour shading on the maps.

Caption of Fig. 8: The colour shading shows the horizontal distribution of the multi-model mean

References

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