

Response to Reviewer 2 on “Sulfate geoengineering: a review of the factors controlling the needed injection of sulfur dioxide”

Comments are repeated in black italics. Replies are indicated in blue. Figure 4 is attached to the response to reviewer 1.

The paper summarizes geoengineering studies that discussed stratospheric SO₂ injections into climate models. The paper focusses only on a few studies. There are not that many studies in recent years that actually injected a fixed amount of SO₂ into the stratosphere. However, various studies used prescribed aerosol distributions. Those also contribute to the question of needed injections of sulfur dioxide. Therefore, I would recommend to extend this study to more papers, as listed below to justify the word “review” in the title. Also, I do not understand the last section of the paper and numbers in Table 1, and I think it needs more explanation.

We thank the Reviewer for his encouraging general comment. As discussed below point-by-point, we have tried to incorporate all the Reviewer’s suggestions for improving the manuscript.

Abstract: I disagree that the described technique would be planned for a timeframe of a few decades, while implementation of global measures of GHG emissions is achieved. This technique would likely have to be applied during and after global measures are implemented, and for a much longer period of time if aiming for temperature stabilization, since temperatures will still continue to rise after mitigation efforts have started. See for example Sanderson et al., 2016 (doi: 10.1002/2016GL069563), Tilmes et al., 2016 (doi:10.1002/2016GL070122); depending on the mitigation efforts, solar geoengineering may be required for a very long period of time.

Both abstract and introduction have been modified according to this comment. The introduction now states: **“Such geoengineering methods would need to be applied during and after global intergovernmental 10 measures on GHG emissions are implemented, in order to achieve surface temperature stabilization (Sanderson et al. (2016); Tilmes et al. (2016)).”**

Line 10: It will be very difficult to fine-tune amounts of sulfur dioxide emissions based on models, due to the range of climate sensitivity and differences in the response of surface temperatures to volcanic aerosols. All the different studies can do, is outline important factors that control the amount of sulfur dioxide to be injected.

Text modified according to this comment.

Page 2, Line 8. As commented above, it is misleading to assume that this technique would only be used between 2020 and 2070.

Text modified as suggested above.

Page 2, Line 21. Why would you only focus on the G4 type studies, why not extend this? Besides, there are other earlier studies that used fixed amounts of SO₂ injections, Rasch et al., 2006, and studies that prescribed sulfate aerosols based on fixed amounts of SO₂ injections, including Rasch et al, 2008 (doi:10.1029/2007GL032179), Tilmes et al., 2009 (doi:10.1029/2008JD011420), Tilmes et al., 2012 (doi:10.5194/acp-12-10945-2012). Those and others may be included in the review.

The reviewer suggestion has been followed in the revised version, including earlier studies with fixed amounts of SO₂ injections and also including a documented G3-type study in the ozone section.

Line 26: You can also add Niemeier et al., 2011 (doi:10.1002/asl.304), and Niemeier et al., 2013 (doi:10.1002/2013JD020445).

References added.

Page 3: Direct forcing of stratospheric sulfate: References in the first paragraph are very old and by now there are more recent papers describing that the cooling effect after Mt. Pinatubo was actually much smaller (at most 0.3 C), IPCC 2015, Canty et al., 2013 (doi:10.5194/acp-13-3997-2013). Also the radiative forcing seems to be largely overestimated in the study by Minnis et al., 1993.

References updated for the globally averaged temperature change after Pinatubo. The text now states: **“This was calculated as a monthly mean for September 1992, compared to pre-Pinatubo levels. However, more recent results with detrended analyses (Canty et al. (2013)) have shown that the Pinatubo volcanic impact on surface temperatures was probably overestimated by about a factor of 2, with a cooling estimate of 0.14 K and 0.32 K, globally and over land, respectively.”** The estimate of Stowe et al. (1992) ($\sim 2.5 \text{ Wm}^{-2}$) is used for the net TOARF.

Page 3, Line 28: The range in radiative response was likely due to the differences in AOD of the models. However, even with the same AOD distribution, models may have very different radiative responses, see for example Neely et al., 2015 (doi:10.5194/gmdd-8-10711-2015), just comparing 2 CESM versions with different radiation schemes.

Text modified accordingly. We added the lines: **“The different results are mainly dependent on the (calculated, or imposed in one case) different aerosol optical depth (AOD) and size distribution among models. It should also be considered that, in general, even with the same AOD distribution, models may produce different radiative responses depending on the adopted radiation scheme (Neely et al. (2016)).”**

Page 3, Line 13: please change to “a series of factors”.

Changed.

Section 2.2.1 Ozone. This section only summarizes findings from one paper, this is not a review. Heckendorn et al., 2009 (doi:10.1088/1748-9326/4/4/045108) and Tilmes et al., 2009 (doi:10.1029/2008JD011420), have discussed changes in ozone due to solar geoengineering.

In the original manuscript we were focusing only on the topic of the indirect RF due to ozone changes, which was extensively reported only in Pitari et al. (2014). But we agree that in a review article the discussion should be extended to all relevant physical and chemical processes involved. A more complete coverage of the recent literature for the SG effects on stratospheric ozone is now made in the revised manuscript. We have added the following phrases: **“Early studies of the potential impact of SG on stratospheric ozone are those of Tilmes et al. (2008), Tilmes et al. (2009) and Heckendorn et al. (2009). Tilmes et al. (2008) focus on polar ozone and estimate that SG could favor stratospheric ozone destruction and delay the recovery of the Antarctic ozone hole by 30-70 years. In addition, this ozone depletion produces a significant increase of erythemal surface UV,**

up to 5% in mid- and high latitudes and 10% over Antarctica (Tilmes et al. (2012)). The polar ozone depletion is favored by enhanced NO_x removal via heterogeneous chemical reactions on the surface of stratospheric sulfate aerosols, as in the case of major volcanic eruptions taking place with high atmospheric levels of chlorine and bromine species (Tabazadeh et al. (2002)). Tilmes et al. (2009) and Heckendorn et al. (2009) analyze the SG impact in chemical ozone loss rates and find that the chemical ozone changes are significantly impacted by the strong reduction of the NO_x cycle, due to the efficient NO_x to HNO₃ conversion on the surface of sulfate aerosols. The NO_x depletion, in turn, favors an increase of HO_x, Cl_x and Br_x loss rates: the net effect on column ozone column will then be time-dependent and regulated by the amount of halogen species in the lower stratosphere. Heckendorn et al. (2009) have calculated a global ozone reduction of 4.5% (i.e., ~13 DU), for an injection of 10 Tg-SO₂/yr and assuming halogen concentrations appropriate for year 2000. Pitari et al. (2014) have run the GeoMIP G4 experiment from 2020 to 2070: despite the constant stratospheric aerosol loading, the magnitude of the geoengineering aerosol induced ozone depletion is found to decrease in time, due to the decreasing atmospheric concentration of chlorine and bromine species. Two of the models used in this study (ULAQ-CCM and MIROC-ESM-CHEM) even show a global ozone increase starting from about 2050, when the NO_x driven chemical ozone increase is no longer overbalanced by the HO_x, Cl_x and Br_x driven ozone loss.”

Page 5, Line 13: Do the numbers -1.1 to -2.1 DU include the model that did not consider heterogeneous chemistry? How do those numbers compare to earlier studies? Same for the RF, what models are included in this number?

A more complete and precise discussion is now made in the revised manuscript, with appropriate citations to previous studies.

Section 2.2.3. Do you mean “Upper tropospheric ice”?

Thank you for catching this typo. Corrected.

Page 8, Line 12; Please note, tropospheric UV shows a net reduction in the tropics, correctly stated in the text. However, this is not the case of mid- and high latitudes. Methane lifetime is mostly influenced from OH changes in the tropics, therefore the methane lifetime is increased with geoengineering.

A sentence has been added to make it clear that the high-latitude UV increase has little effect on the methane lifetime.

*Line 23: typo: today’s, also what do you mean by today’s levels, what period? Could you explain the numbers given in Section 2.3 and Table 1? For example, to offset certain levels of RF, one would need to identify how much sulfur injection is required, which is model depended. For instance, Niemeier and Timmreck, 2015, calculated an efficiency of 0.30 – 0.35 W/m² per TgS injection. Since 5 Tg SO₂ are equal to 2.5 TgS, this results in about 0.3*2.5 = 0.75 W/m² per 5 Tg SO₂ injection. Can you do the same calculations for the other studies? It is not clear how you get to the value of -1.45 W/m² +/- 0.65 in this study. Also, for example the RF of RCP 4.5 between 2020 and 2070 is about 2.2-2.3 W/m². Where does the number in Table 1 (0.8 W/m²) come from? If the RF needs to be set off by geoengineering in 2070, much more forcing is required than 0.8 W/m².*

Today's level is now specified: RF is estimated for year 2100 relative to year 2011. Table 1 has been eliminated. We agree that our attempt to quantify a net residual from the RCP net RFs over the "50 year period of SG application" minus the net RF from SG is not clear and not fully justified, on light of the previous criticisms. For this reason, we simply summarize the IPCC findings on the net RFs following different RCPs and we present our findings on the breakdown per component of the SG RF in a "stand-alone" figure, taking into account the estimates published in the recent literature and separately discussed in sections 2.1 and 2.2.

For the cirrus forcing, why do you only state one number for cirrus impacts and not the lower number from Pitari et al., 2016b? Particle sizes from sulfate geoengineering are likely not large enough to have any significant effect, while dust particles have a larger effect. In Table 1, at least give a range for cirrus cloud effects.

The RF summary plot (Fig. 4) now includes whiskers for all the components, including cirrus ice. We thank the reviewer for the specific suggestion. By the way, SG particles are inefficient IN, mainly because they are supercooled liquid particles, contrary to (solid) dust particles.