Response to Reviewer 3 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.**

This paper presents a review of studies of sulfate geoengineering (SG). The paper selects results from a wide body of literature, of which some significant works have been left out. I agree with referee comment 1 (RC1) that more information should be given about the limitations of the studies presented, and the relative strength of conclusions possible. I have attached an annotated PDF with corrections and comments, which I summarize and expand on here.

We thank the Reviewer for his constructive comments. As discussed below point-by-point, we have tried to incorporate all the Reviewer's suggestions for improving the manuscript.

Page 2, lines 7-8: This is a dangerously false statement. If SG were applied only during the transition period to clean energy source, its abrupt halt would trigger catastrophically rapid global warming, since the negative forcing of stratospheric sulfates would be removed within a few years, while the positive forcing of carbon dioxide would remain for thousands of years. Unless humans can remove much of the carbon dioxide from the atmosphere that we have added over 170 years thus far, SG would have to be applied indefinitely on human timescales. As the paper alludes to, carbon air capture remains a very elusive and energy intensive process, and it is far from clear that it would be viable on a large scale by 2070.

Following the same recommendation of the second reviewer, we have cut this statement from both introduction and abstract.

Page 2, line 23: What is meant by "the GeoMIP experiment Robock et al. (2011)", in contrast to "the GeoMIP experiment G4" on line 21?

That was a typo: "G3" is missing. Corrected, with additional references.

Page 3, line 6: It should be clarified that the 0.5 C drop in global average temperature was a monthly average, not an annual average.

Following the same criticism by the second reviewer, we have corrected this statement, with additional references.

Page 4, lines 7 and 12: clarify at what latitude(s) SO_2 was injected, and how emissions were zonally distributed.

Clarified, for both Aquila et al. (2014) and Niemeier and Timmreck (2015).

Page 4, line 8: "proportionally" implies a linear relationship of aerosol mass injected to the period of the westerly phase. This does not see right if a permanent westerly is achieved with a finite injection rate.

We agree that "proportional" is not the right word to describe this effect. Corrected as follow: **"They found that an injection of about 8Tg-S/yr would cause a slowing of the QBO oscillation with a**

constant QBO westerly phase in the lower stratosphere with overlaying easterlies, consistently with the findings by Aquila et al. (2014a).”

Page 5, section 2.2.1: It is unclear that the attribution of reduction in O_2 photolysis as the “main” cause of the reduction in column ozone is reasonable absent experiments in which O_2 photolysis rates are unchanged by sulfate AOD. The catalytic loss rates are proportional to the amount of ozone present, so might be larger if ozone production were not reduced. The later discussion that column ozone increases with SG after 2060, when chlorine and bromine are reduced, makes this point less convincing.

We admit there was some confusing statements in the original manuscript. We have simplified our sentence as follows: **“The models used in the G4 experiment showed significant changes in the ozone profile, with a decrease in the tropical column between 100 and 30 hPa in the tropics, for the combined effects of enhanced upwelling and losses in the chemical cycles.”**

I agree with RC2 that Table 1 is unclear and requires substantial further explanation.

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.

I have included a few typographical corrections as well in the annotated PDF.

The sticky notes on the original pdf document have been properly considered in the revised manuscript.

Finally, there are a number of additional studies that could be discussed in this review. RC1 and RC2 have identified a number of these. I would suggest at least including some discussion of these papers:

Tilmes, S., R. Müller, and R. Salawitch (2008), The sensitivity of polar ozone depletion to proposed geoengineering schemes, Science, 320(5880), 1201–1204, doi:10.1126/science.1153966.

Tilmes, S. et al. (2013), The hydrological impact of geoengineering in the Geoengineering Model Intercomparison Project (GeoMIP), J. Geophys. Res-Atmos, 118(1), 11036–11058, doi:10.1002/jgrd.50868.

Tilmes, S., B. M. Sanderson, and B. C. O’Neill (2016), Climate impacts of geoengineering in a delayed mitigation scenario, Geophys. Res. Lett., 43(15), 8222–8229, doi:10.1002/2016GL070122.

These (and other references to relevant SG studies) are included in the revised manuscript.

Please also note the supplement to this comment: http://www.atmos-chem-phys-discuss.net/acp-2016-985/acp-2016-985-RC3-supplement.pdf

The sticky notes on the original pdf document have been properly considered in the revised manuscript.