Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-985-RC3, 2016

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sulfate geoengineering: a review of the factors controlling the needed injection of sulfur dioxide" *by* Daniele Visioni et al.

Anonymous Referee #3

Received and published: 29 December 2016

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.

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.

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?

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.

Page 4, lines 7 and 12: clarify at what latitude(s) SO2 was injected, and how emissions were zonally distributed

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.

Page 5, section 2.2.1: It is unclear that the attribution of reduction in O2 photolysis as the "main" cause of the reduction in column ozone is reasonable absent experiments in which O2 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.

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

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

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-985, 2016.