

Interactive comment on “Sulfate geoengineering: a review of the factors controlling the needed injection of sulfur dioxide” by Daniele Visionsi et al.

Anonymous Referee #1

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This is a review paper on sulfate geo-engineering and the factors controlling “the needed” injection of sulfur dioxide. The authors reviewed the direct radiative effect of sulfur injection that may lead to troposphere cooling and stratospheric warming, and the indirect radiative effect that caused by induced changes in ozone, CH₄, stratospheric water vapor, and upper tropospheric cirrus clouds. They compared the effect of GHG warming and the resulted changes by the direct and indirect effects of sulfate geo-engineering in order to estimate the best amount of sulfate to be injected.

A critical review article that integrates and evaluates published literature is potentially very useful both for geo-engineering researchers and the broad atmospheric modeling community. Therefore, the effort the authors have made in this regard is greatly appreciated. However, I think the current manuscript needs to be substantially improved. The reasons are listed below:

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1) A few sections (2.1, 2.2.1-2.2.3) in the current review only passively summarize the findings from previous studies, but they don't point out the weakness/gaps and suggest potential improvements and future directions. For example, many studies cited in the manuscript are based on the atmosphere-only model simulations forced by prescribed SST, so the interaction with the ocean is not considered. Another example is that the estimates from Cirisan et al. (2013) are based on box model simulations and radiative transfer model calculations, and it doesn't consider the dynamical impact and the feedback to microphysics. Some careful discussions are needed for such cases.

2) The authors did a good job in making connections between relevant studies, but in my opinion some of the discussions were presented with a bit too much detailed information (e.g. page 4 section 2.1), and the big picture was hidden behind some mixed topics. For example, I would suggest the authors to divide section 2.1 (and possibly 2.2.x) into two parts: 1) direct effects of sulfur injection (changes in microphysical properties, aerosol lifecycle, and optical properties) and the associated heating and cooling; 2) changes in circulation and its feedback. Also, as a review article, I think it is necessary to draw some schematic plots showing the major findings (mechanisms) from the literature (e.g. one each for sections 2.1, 2.2.1-2.2.4), so that the readers can have a quick overview of those studies. This is particularly important when the authors want to deliver comprehensive messages and opposing points from different studies.

3) I think there are major flaws in Table 1 and the associated discussions (section 2.3). It seems to me that the authors are trying to project a net SG effect to compensate the RCP "forcing" (I think the authors should define their definition of forcing at the beginning) estimate. First, I am not clear how the authors derived the RCP RF numbers (not explicitly available in Moss et al. 2010), but it seems to me the "forcing" data presented in the paper are not calculated by CMIP models, but rather calculated using Integrated Assessment Models (IAM). Therefore, they might be very different from the real "forcing" estimated by the global climate models used in GeoMIP. Second, I think it's unacceptable to simply calculate the arithmetic mean the "forcing" numbers

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obtained from studies on different (direct/indirect) SG effects and the RCP estimates. Even if these numbers are estimated from the same model, the non-linear effect between the GHG warming, sulfate scattering, and cirrus cloud formation would result a very different estimate. I suggest to eliminate this part.

4) Some additional literature need to be cited. For example, when discussing the impact on ozone, Tabazadeh et al. (2002) and Tilmes et al. (2008) should be cited and discussed.

References:

Tabazadeh, A., Drdla, K., Schoeberl, M.R., Hamill, P. and Toon, O.B., 2002. Arctic “ozone hole” in a cold volcanic stratosphere. *Proceedings of the National Academy of Sciences*, 99(5), pp.2609-2612.

Tilmes, S., Müller, R. and Salawitch, R., 2008. The sensitivity of polar ozone depletion to proposed geoengineering schemes. *Science*, 320(5880), pp.1201-1204.

Minor issues: I saw quite some formatting problems and typos (especially RCP numbers in table 1). Please correct them.

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