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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sulfate Geoengineering Impact on Methane Transport and Lifetime: Results from the Geoengineering Model Intercomparison Project (GeoMIP)" *by* Daniele Visioni et al.

Anonymous Referee #1

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My main issue is probably between minor and major. There is something that I think needs to be done but I hope can be accomplished without a great deal of difficulty (so sorry if the score looks severe).

My main concern with the paper is that they are discussing the impact of geoengineering using sulfate aerosol but never really show how their aerosol manifests itself. This is really crucial since if the aerosol is poorly depicted the rest of the results are essentially uninteresting. Is aerosol properly trapped at low latitudes above 20 km or does it run rapidly off to high latitudes (like it does in WACCM)? Looking at the aerosol

SAD anomalies, I see effectively no change in aerosol loading in low latitudes. This is at odds with what was observed after Pinatubo where a normally low aerosol region in the tropical upper troposphere is filled with aerosol for several years after the eruption (mostly due to sedimentation I suspect). In any case, I think it is critical to demonstrate that their model can produce realistic aerosol distributions for this scenario. My concern is that since they apparently see no enhancement in the tropical upper stratosphere that something unrealistic is happening with the aerosol. Please make my concerns go away.

Minor point, they seem to like to reference their own work an awful lot. This is ok but it left me with the impression that they are the only people doing key parts of this area of research.

Minor point, are they distributing the sulfur injection uniformly between 18 and 25 km? These seems impractical at best and more realistic injection scenarios would yield more realistic outcomes for aerosol distributions. Most scenarios I've seen suggest injection between 18 and 20 and counting on upward transport into the tropical pipe to distribute aerosol to higher altitudes (as observed following small and moderate eruptions and the well know water tape recorder).

Minor point, the uncertainties attached to SAGE II estimates of effective radius shown in the label for Table 1 are simply impossible or imply an impossible level of certainty in them. There are well known issues in estimating SAD with SAGE II observations at low aerosol levels which contributes to significant uncertainty in a parameter derived using it (reff). At high loading, all size discrimination of optical measurements effectively go away other than 'they are big' since the spectral dependence becomes flat and invariant for large ranges of potential sizes. Certainly the authors do not shown how they were inferred and I am wondering what they mean.

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