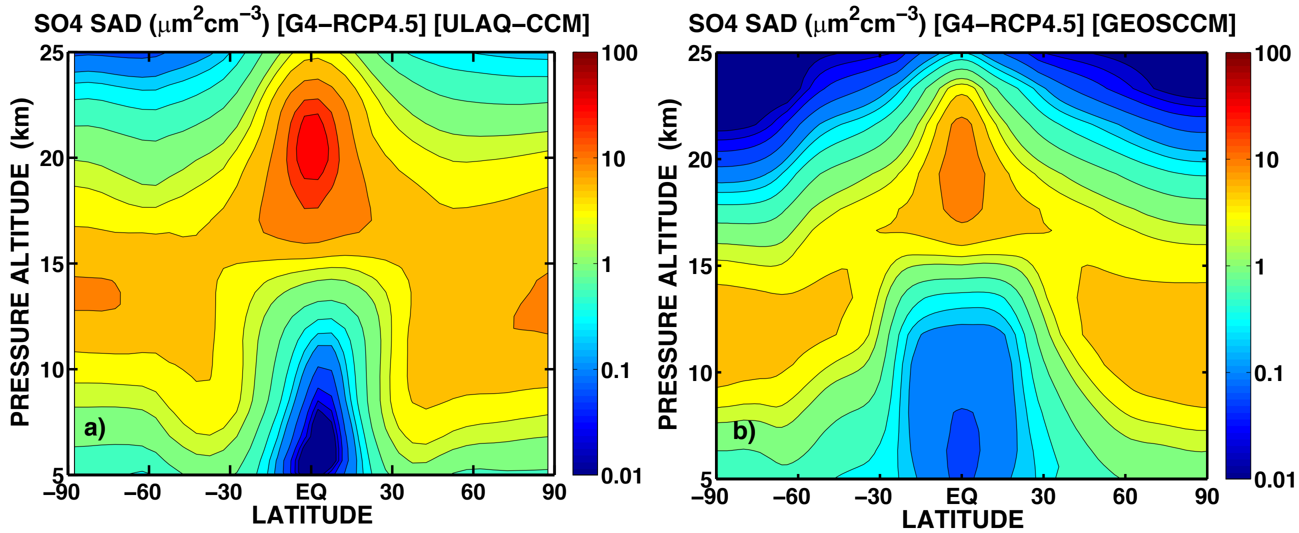
**Response to Reviewer # 1**

Reviewer's comments are in blue. Author responses are in black.

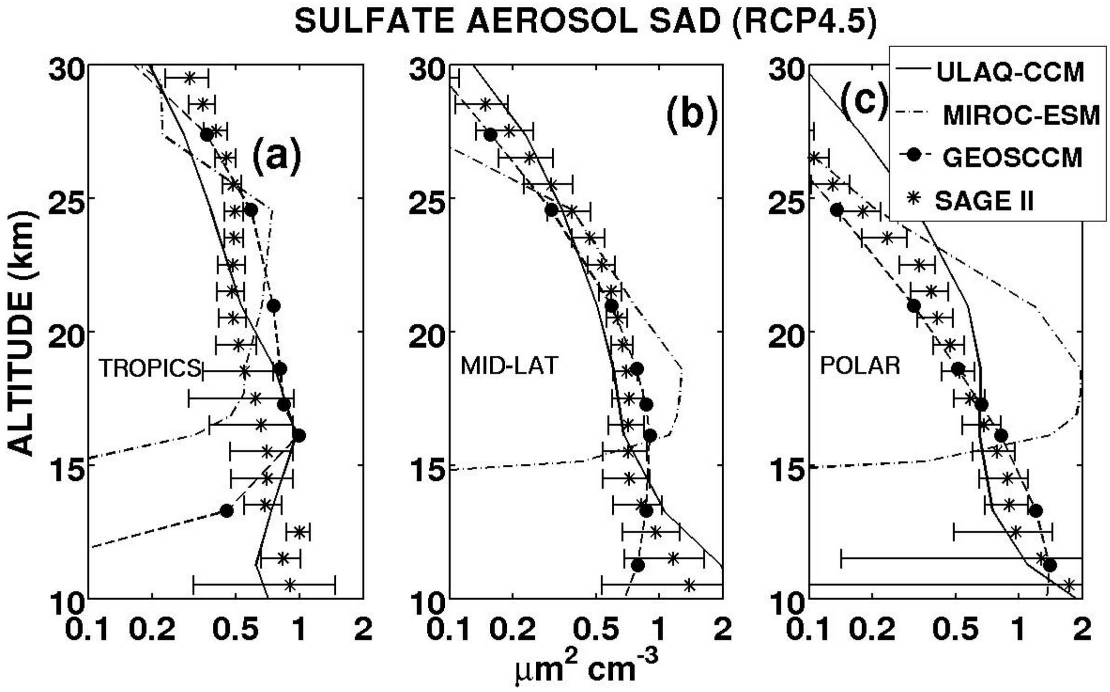
We thank the reviewer for his helpful comments, that will allow us to clarify some of the points of the manuscript.

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.

An in depth validation of both models regarding aerosol SAD changes due to SG and sulfate transport was already given in the Pitari et al. (2014) paper; we felt that adding a similar model evaluation would have lengthened the paper too much. However, in the reviewed manuscript we will add to Fig. 12 two additional panels (now (a-b), attached below) highlighting the aerosol SAD in the lower stratosphere for the two models, whereas the original Fig. 12 (a-b) (becoming (c-d) in the revised version) will remain to highlight the changes in aerosol SAD in the upper troposphere that are closely related to the discussion in Section 5 (tropospheric chemistry changes).




As can be seen by the two new panels, the aerosol distribution in both models is in agreement with several other models that have performed sulfate geoengineering simulation, and with observations after the Pinatubo eruption (for instance, SAGE II). Both models show a pronounced confinement in the tropical lower stratosphere, with an aerosol sedimentation-driven increase of both SAD and mass density in the tropical upper troposphere, gradually approaching low values when penetrating downwards (due to irreversible removal mechanisms, namely ice particles sedimentation and wet deposition; see also Visioni et al. (2017)). A significant mid-latitude aerosol concentration is also predicted in both models due to strat-trop exchange associated to the lower branch of the Brewer-Dobson circulation. Differences between the two models in the distribution of the aerosols are due to intrinsic model differences in the size distribution (imposed for GEOSCCM and calculated for ULAQ-CCM) and the adopted radiation scheme (with impact on heating rates and hence on circulation changes). Large scale transport differences may also contribute, and the reasons are well summarized in Table 1 (treatment of QBO, SSTs, horizontal/vertical resolution). Nevertheless, both models still remain well within the range of the SAGE II measurements after the Pinatubo eruption (see Pitari et al., 2014). For good measure, we attach below a copy of Fig. 6 from Pitari et al. (2014), showing this comparison.




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.

We apologize if this is the impression we have given. We have tried to include all possible published works related to the topic, and if we have failed to do so we will be glad to accept any suggestion regarding an enrichment of our bibliography. Often we cite Visioni et al. (2017) because it is a review paper where we discussed various side effects of the sulfate injection, such as effects on ozone depletion and UV changes at the surface. However, all the relevant papers presented in that one paper are also cited here when needed, and we feel none have been left out.

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).

We agree that there might be more realistic injection scenarios, but the injection scenario we used is the one prescribed by the GeoMIP G4 experiment. However, we will further expand the text in the revised manuscript about the differences in injection between the two models: the GEOSCCM model injects aerosol in the 16-25 km layer in a uniform way, the ULAQ-CCM model inject the aerosol in the 18-25 km layer, but with a Gaussian distribution that puts 80% of the sulfur mass in the altitude layer from 19.5 to 22 km. This is because the GeoMIP G4 experiment suggested to inject the aerosol in a way to mimic the way any single model handles the Pinatubo eruption (Kravitz et al., 2011).

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.

We agree with the reviewer that there are large uncertainties in the SAD estimates with SAGE II, and we will add a caveat in the caption of Table 1. However, the values that we use for the effective radius (not the SAD, anyway) are the ones that have been made available by the SAGE group at the Langley Research Centre. We will however change the Table 1 caption in order to clarify that we are showing the standard deviation for the measurement given by SAGE II, and not an uncertainty estimated by ourselves (see also Pitari et al., 2014).