Response to reviewer # 2

Reviewer comments are in bold. Author responses are in blue.

This manuscript analyzes geo-engineering simulations of sulfur injection in the ULAQ CCM. The paper is generally well written. It suffers from some minor grammar mistakes, but the scientific points are made. I am not sure how valid they are however.

We thank the reviewer for his in-depth review and for his comments. We will try to respond to all the points raised, and to show that our work is scientifically robust.

<u>Overall comment</u>:

I think the methodology may be deeply flawed, since I am not certain that applying another model SSTs, from a model with no ice nucleation and poor upper tropospheric cirrus clouds, is a sufficiently useful method to look at perturbations. I think the resulting dynamical response could just be a model bias when the SSTs from another model is applied, and I fear that this would simply confuse the literature with another dubious single model study. This study needs some major revisions to address these points, and it may not actually be acceptable for ACP given the possible methodological flaws.

We disagree on this point. At the same time, it is rather clear from the first sentence and from other remarks below, that we were not able to make the scientific structure of our work sufficiently clear to the reader. We have tried to do it much better in the revised version and in the present reply to the reviewer.

(a) Our study takes inspiration from a previous one (Kuebbeler et al, 2012), where SSTs were kept unchanged in the sulfate geoengineering perturbed G4 case (5 Tg-SO_2/yr injection in the tropical lower stratosphere), with respect to the control simulation without geoengineering aerosols. In that case, dynamical changes produced by the lower stratospheric aerosol heating become drivers of a significant indirect effect of sulfate geoengineering (SG) on ice particle formation in the upper troposphere via homogeneous freezing. The increasing atmospheric static stability, due to the lower stratospheric aerosol-induced warming, produces a reduction in synoptic scale vertical motions with a resulting decrease in ice particle formation.

(b) An important question (raised in the same paper of Kuebbeler et al., 2012) may obviously be to what extent the surface cooling produced by the increased planetary albedo in G4 conditions (i.e., the DIRECT effect of geoengineering aerosol, for which SG is actually designed) may contribute to dynamical changes together with the lower stratospheric aerosol heating.

(c) In order to tackle this last scientific question, different approaches are possible. The ideal procedure would be to use an ocean-atmosphere coupled model, with chemistry-aerosol-ice clouds on-line, fully interactive with radiation and dynamics and with a vertical extension covering both troposphere and stratosphere. Possibly in a multi-model configuration, to assess inter-model differences (i.e. MIP approach). At the moment, however, this is rather difficult to achieve, since all the above requirements are not easy to be found and adapted to a SG configuration. This would be what the reviewer calls (below) a more "definitive" study.

(d) An alternative approach would be to use an atmospheric climate-chemistry coupled model (CCM) with sulfur chemistry and sulfate aerosol microphysics on-line (Pitari et al., 2014; Visioni et al., 2018), in-depth process evaluation (Visioni et al., 2017b), ice cloud formation scheme and its evaluation (as attempted in the present work). This is exactly what we have done with the ULAQ-CCM. On the other hand, it is well known that CCMs need an external specification of SSTs (due to their intrinsic formulation). This has been extensively made in previous international model campaigns, on-going since 2006 (i.e., SPARC-CCMVal-1, SPARC-CCMVal-2, SPAR-CCMI-1). What is needed for this purpose is the output of an atmosphere-ocean coupled model run with and without SG, under a given RCP scenario. The desired SG effect on SST is indeed the DIRECT aerosol effect (i.e., surface cooling due to the increasing planetary albedo), and CCSM-CAM4 does it well. Indirect effects on both chemistry and upper tropospheric ice are secondary effects not needed at this stage, contrary to what the reviewer claims. In fact, having an external SST change sensitive also to SG aerosol INDIRECT perturbations (chemistry and ice), would actually create an inconsistency in the nudging procedure. We rely of the fact that the SG aerosol radiative perturbation is the dominant one (see Visioni et al., 2017a) and we use the resulting changes on SST predicted by CCSM-CAM4 as the first-approximation dynamical driver for a CCM designed for the same SG perturbation (i.e., 8 Tg-SO_2/yr), but also including indirect effects on ice clouds (present work) and chemistry (see Visioni et al., 2017b). We will try to address this point in the revised version of the manuscript.

(e) We strongly believe that our "single model work", which is scientifically respectable as the "single model work" of Kuebbeler et al (2012), may be considered a good step forward, in the sense that the use of an externally specified SST sensitive to the direct SG aerosol radiative perturbation makes the CCM ice response more realistic than keeping SSTs fixed with respect to the baseline reference case.

(f) At the same time, we hope that in future a "more definitive" study will be conducted with on-line predicted SSTs, function of direct and indirect radiative changes produced by stratospheric SG aerosols.

General Comments:

1. I know the authors' first language is not English, and English is not an easy or kind language for the article and plural mistakes they are making, but I would suggest an edit by a native English speaker.

Following the suggestion of both reviewers, an English technical editing of the manuscript has been done.

2. As noted below, I am uncomfortable with some of the validation references. They should probably focus on papers, rather than other notes or presentations.

We are not sure what the reviewer is referring to. We have however reviewed the references used in the paper regarding the validation data we used. The only technical report in the original manuscript is Chou et al. (2001) regarding the longwave radiative transfer code; a journal paper was also cited for the shortwave radiative transfer code (Randles et al., 2013). Bosilovich et al. (2017) describes the MERRA-2 data and is a peer reviewed paper. If the reviewer asks for additional references regarding observational data, we have added two more, i.e. Gelaro et al. (2017) and Duncan and Eriksson (2018).

3. Most significantly: how does imposing SSTs from another model with an uncertain response tell us anything about the real atmosphere. You are just shocking one model with another, and you get a response. Why does the no feedback response matter, and how is it relevant? It is stated that some other models get a similar response, but I am not convinced. How would you even know if the model was self-consistent?

As explained in the response to the overall general comment, this is the normal way CCMs are used for baseline and sensitivity experiments (to RCP scenarios, solar fluxes, short lived species ground fluxes and many other components of the climate systems, along with their connection with chemistry) [see above points (d-e)]. Our results are consistent with those of Kuebbeler et al. (2012) for the upper tropospheric ice sensitivity to stratospheric SG aerosols, in the sense that the lower stratospheric aerosol longwave and solar heating rates are the major driver for circulation changes, but we go a step forward considering also the potential significance of the tropospheric cooling induced by the stratospheric aerosols [see above points (d-f)].
The reviewer often uses the argument of "self-consistency", but this does not apply in CCM experiments, because SSTs are an input parameter in this type of model. I think we have clearly explained in response to the overall comment, that we use SSTs from the CCSM-CAM4 ocean-atmosphere model for having a reliable input on "baseline" surface temperatures in a future RCP scenario and a "reliable" input for the SG aerosol perturbation to these temperatures. The latter is the "dominant direct" climate effect of SG. Indirect effects (i.e. chemistry and upper tropospheric ice) are treated consistently in the UAQ-CCM formulation, assuming SST changes produced by the SG stratospheric aerosols as a good first approximation. The CCSM-CAM4 SG stratospheric aerosol distribution used in the geoengineering simulation has been detailed in Tilmes et al. (2015).

Incomplete sentence "The goal of the present study..."

Corrected.

Relative to the clear sky net...

Changed.

How is this study different than previous work?

The only other study regarding the thermo-dynamical effects of sulfate geoengineering on cirrus cloud was that of Kuebbeler et al. (2012). In their case, however, sea surface temperatures where kept fixed. In our study, as the authors of the aforementioned paper asked in their conclusions, we try to analyze the difference between a sulfate injection with (G4) and without (G4K) the changes in sea surface temperatures due to the injected sulfate. We believe that, by showing the differences between G4 and G4K results in our model, we can gain further knowledge regarding this particular side effect.

How much of these results are due to just individual model climatologies? Seems like the effects depend on how much homo v. heterogeneous ice a model has, and what a large-scale model does to create and maintain cirrus. Why would your study be any more definitive?

We don't believe our study to be definitive in any way. We show, when comparing our G4K results with those from Kuebbeler et al. (2012), that the results from the two models are comparable in that scenario, and that further differences appear when considering changes in sea surface temperatures produced by the SG aerosol perturbation. We believe that by analyzing the differences caused by only that factor (SST changes due to the SG aerosol direct effect) we can constrain one of the possible factors that might influence the dynamical response to sulfate geoengineering. This approach was also used in a previous study related to methane changes (Visioni et al., 2017b), where we compared our results (in simulations with and without changing SSTs) against results from GEOSCCM.

The goal....

Corrected.

by including...

Changed.

ULAQ model description and a reference are needed. Does the description appear later? It does. Add see below.

Following also the precious suggestions of reviewer 1, in the revised manuscript we have modified the structure of the paper in order to have the model description before anything else.

CCSM-CAM4 needs a description and the acronym spelled out. At least a reference for the simulations. Is there more model description later? Applying the cooling from another model seems problematic: presumably CCSM4 has some of the same feedbacks, operating in different ways?

In the revised manuscript we have described CCSM-CAM4 and tried to better explain our modeling approach in the use of this model SSTs. Again, please refer to our response to the overall comment.

I don't like that you have created a very arbitrary perturbation that changes vertical motion and transport in a coarse resolution GCM. The result is that I believe your perturbation is very model specific and artificial. I support attempting to understand processes in a model but this whole paper seems very dependent on a single model formulation. I'm not convinced you can or should separate all the affects this way.

As previously explained, the methodology of using externally provided SSTs as input parameter in CCM experiments is intrinsic in the CCM formulation itself. This has been done in all CCMVal-1, CCMVal-2 and CCMI-1 experiments, and more recently for the ISA-MIP Project (Timmreck et al., 2018; https://www.geosci-model-dev-discuss.net/gmd-2017-308/). As for using perturbed SSTs in case of a geoengineering scenario, we can point out to previous works where our dynamical perturbations have been compared to other models (Pitari et al., 2014; Visioni et al., 2017b).
The resulting dynamical changes are not arbitrary, but consistent with the SG aerosol dynamical drivers, i.e. perturbation of lower stratospheric heating rates and SSTs. A clear discussion is made in the manuscript on how the resulting changes in vertical motion are produced and how they are sensitive to these aerosol drivers. The results are obviously valid in the limitation of "a single model formulation" (as in the case of Kuebbeler et al., 2012, by the way), but may certainly represent a step forward and could be a valuable reference point in the literature for future multi-model experiments, possibly with ocean-atmosphere coupled models.

I think you need to describe relevant features of the cloud and transport scheme of ULAQ, and the basic features of CCSM4 here. What ice nucleation mechanisms are included and how does the cloud scheme create cirrus clouds? What radiation scheme is used? How do the volcanic emissions evolve? For CCSM4: how do its volcanic emissions evolve and how is that related.

As already specified above, a new paragraph on the CCSM-CAM4 model has been included in the revised manuscript. A full description of the ULAQ-CCM is available in the Morgenstern et al. (2017) paper, which summarizes the major features of all global-scale model participating in the SPARC-CCMI model initiative. Details on the radiation scheme are also available in this latter

paper, as well as in Pitari et al. (2014). Evolution of volcanic clouds in the ULAQ-CCM has been fully discussed in Pitari et al. (2016a) and Pitari et al. (2016b). CCSM-CAM4 is described in Tilmes et al. (2016).
A full section in the original manuscript is devoted to explaining the cirrus cloud formation in the ULAQ-CCM, via homogeneous freezing. An additional paragraph on the ice formation via heterogeneous freezing is now included in the revised manuscript.
Regarding the volcanic emissions, the simulations are in the future under a RCP4.5 scenario, so volcanic emissions are not considered.

The inconsistency here I think is problematic for the study. I'm not convinced you should look at this perturbation turning on and off surface temperature perturbations, and expect that the resulting impact on the model has any reference to reality since the system breaks any feedbacks that might modify the surface temperature.

Most of the available works on sulfate geoengineering have been performed using models with prescribed SSTs (as an example, Kuebbeler et al., 2012; Niemeier and Timmreck, 2015; Niemeier and Schmidt, 2017). We believe that showing what happens when turning on and off the surface temperature perturbation might be a valuable way to understand some of the feedbacks.
In addition, we would like to remind that the primary perturbation driving dynamical changes in the atmosphere is the lower stratospheric heating due to SG aerosols (see Kuebbeler et al., 2012). We show that SST changes end up increasing the atmospheric stabilization, which is primarily produced by the lower stratospheric aerosol warming.

Why should the surface temperature pattern be believed? CCSM-CAM4 does not have interactive chemistry or a stratosphere. How are the emissions put in? Wouldn't this be different than ULAQ? Especially at high latitudes, impacts are dependent on a stratospheric circulation that I don't think CCSM-CAM4 does correctly at all.

We believe that the surface temperature predicted by CCSM-CAM4 in case of a sulfate geoengineering injection can be used as long as it is clear that it is a first order approximation, because it responds to the direct SG effects (i.e. aerosol increased planetary albedo), allowing the ULAQ-CCM a more realistic study of the atmospheric response to the indirect effects (chemistry, ice) with respect to a case in which SSTs were kept fixed at the RCP4.5 reference values (G4K). This is clarified in the revised manuscript. In addition, in order to be more specific regarding CCSM-CAM4, we have asked the scientist responsible for those SG G4 simulations (Simone Tilmes) to give her contribution to the manuscript by further explaining some of the aspects of the model (as was done for Visioni et al., 2017b). Regarding the modeling of the stratosphere in CCSM-CAM4, we will also reference Lamarque et al. (2012), Neale et al. (2013) and Tilmes et al. (2016) in the revised manuscript.

Why not use WACCM4 Geoengineering experiments, which are at least based on a stratospheric model with interactive sulfur emissions.

The available WACCM4 Geoengineering simulations have not been performed using a fixed injection from 2020 and 2070 as prescribed by the GeoMIP protocol.

So how is what you are doing different than Kuebbler et al. (2012)? Why is this novel or unique?

As we explained before, we believe that including the two direct effects of SG aerosols in the CCM, as primary drivers for dynamical changes, it allows a more complete assessment of the SG impact on upper tropospheric ice formation, with respect to previous study by Kuebbeler et al. (2012) where SSTs were kept fixed at the reference RCP values.

Updrafts responsible for....

Corrected.

So most of the vertical velocity is heavily and crudely parameterized by gravity waves and TKE. The TKE is probably linked strongly to the temp gradients. Does the model actually use this vertical velocity in advection? Or ice nucleation? Please explain what is going on. It is not possible for the reader to understand whether the model formulation is realistic, though I am pretty convinced the perturbation (applying SSTs from another model) is NOT realistic for reasons described above.

Vertical advection of trace species in the model is treated using the large scale vertical velocity calculated in the dynamical core of the CCM. Ice formation via homogeneous freezing in the upper troposphere is produced by updraft on sub-grid scales (see Kärcher and Lohmann, 2002; Lohmann and Kärcher, 2002). The latter is parameterized using the TKE formulation, as explained in the same referenced studies. For what concerns the SST specification see above in response to the overall comment and in other specific comments.

Is a 3% change in a parameterized vertical velocity significant? Is 10% significantly different from 3%? From Figure 6, I don't think any of this is significant.

Figure 6 showed the variability of the calculated vertical velocity (large scale + f(TKE) mesoscale contribution from synoptic scale and gravity wave motions) and the time-averaged mean values. Changes in temperature and wind profiles produced by the SG aerosol forcing are related to a TOARF of the order of -1 W/m^2 and produce a change in TKE of the order of -120 cm^2/s^2 in the tropical upper troposphere in G4 relative to the Base case, i.e. close to -20%. Following the parameterization developed in Lohmann and Kärcher (2002), w is taken as the sum of the large-scale term (of the order of 0.2 cm/s in the tropical UT) and $0.7\times TKE^{0.5}$ (of the order of 17 cm/s in the tropical UT) (see Eq. 1 in the revised manuscript). A change in TKE of approximately -120 cm^2/s^2 translates in a change of -1.8 cm/s of w, i.e. close to -10%. The G4K vertical profile of w is intermediate between G4 and Base, because TKE changes result only from the lower stratospheric aerosol heating, with surface temperature kept fixed at the reference RCP scenario. This ends up in a w change of approximately -3% in the tropical UT. The SG perturbation of the temperature profile is obviously small relative to baseline atmospheric conditions, both in G4 and G4K, but these small changes are exactly those impacting the atmospheric static stability and vertical motions. And differences in G4K and G4 are proved to be significant from this point of view.
To better clarify, we note that the variability of w in Figure 6 is essentially due to seasonal changes and non-zonal asymmetries of the TKE. But if we isolate a given month in the time series, the vertical velocity change due to SG is more comparable to the w variability in the time series. We attach a figure below (Fig. R2_1) showing this quantity, to show the reviewer what we mean.

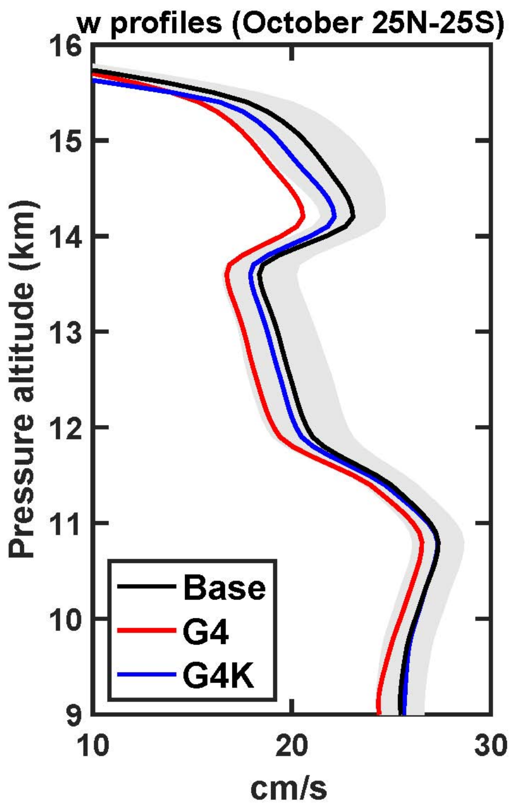

Fig. R2_1. October monthly mean of the upper tropospheric tropical profiles of vertical velocity (cm/s) in G4, G4K and Base experiments (years 2030-39). Shaded areas represent ±1σ for the ensemble over the October month in the 10 year period 2030-39.

MODIS ice effective radius is not a reasonable product, especially for thin tropical cirrus, unless you have a validation paper that says otherwise.

As per the reviewer request, we have tried to add some peer reviewed references to the MODIS ice effective radius, in particular Yang et al. (2007). We will also discuss some of the limitations regarding the retrieval of the ice effective radius (Delanoe and Hogan, 2008; Zhang et al.,2010).

The mention of what looks like a maximum updraft velocity here is an indication that the ULAQ ice nucleation needs to be better explained.

Ice nucleation is now presented in a more complete way in the revised manuscript.

This section needs to go before all the results presented earlier.

We have done what the reviewer suggested, also following the recommendation of reviewer 1.

It's not clear to me what fraction of ice formed in situ (T<238K) is from homogeneous and heterogeneous freezing. It would be useful to note the fraction homogenous (or heterogenous). This looks like it is in Figure 10c, but I don't think that is what I am interested in. What fraction of ice is heterogenously formed?

Cirrus ice formation in the ULAQ-CCM results from both homogeneous and heterogeneous freezing mechanisms and their competition. However, in our first draft of the manuscript we had decided to turn off the heterogeneous freezing mechanism, in order to focus on the SG aerosol induced perturbation to ice formation from homogeneous freezing only.

We acknowledge this specific point of the reviewer. The reduced updraft will affect ice formation from homogeneous freezing in a different way if part of the available water vapor goes to ice particles formed via heterogeneous freezing, which requires smaller supersaturation ratios, both on mineral dust and black carbon particles. Following the reviewer suggestion, we have decided to perform again our simulations with both mechanisms turned on, allowing their non-linear interaction. Results are substantially affected, with a resulting smaller indirect RF due to upper tropospheric ice changes induced by sulfate geoengineering.
Looking at the revised results of our numerical experiments, we are really indebted with the reviewer(s) for making this specific scientific point, helping us in a more correct assessment of the ice perturbation due to SG aerosol and its indirect radiative forcing.

This is a decent summary that the changes are due to changes in vertical velocity and tropospheric temperatures. How model dependent do you think these quantities are?

Results of our numerical experiments are obviously dependent on model features and design. However, one major point of our work was to systematically compare our results with observed ice-related quantities, on one hand, and to an independent modelling work (Kuebbeler et al., 2012) for the SG-related ice perturbation, on the other hand. It is shown that our results are consistent and that inclusion of SST changes may be significant, following a suggestion explicitly made in Kuebbeler et al. (2012).

Why the 5/8 scaling of the RF results?

We wanted to compare our results to their Clear Sky RF, and as a first approximation we scaled our results to their injection rate. However, we recognize that this might be confusing to the reader, and we now compare the direct results of our model with those from Kuebbeler et al. (2012).

How realistic is the decrease in updraft? Is it consistent with the overall circulation? I am concerned that fixing SSTs from another model will not yield a reasonable result, and it is likely to be a single model configuration, not even a general result. How can you convince me and other readers that the mechanism in ULAQ is reasonable, especially since it is imposed from another model and not-interactive, and from a model with no stratosphere.

We believe that we have widely responded above to these specific points. In particular, the use of SSTs as input parameter in CCMs is intrinsic in the CCM nature and formulation itself. A comparison of the SG aerosol optical depth and extinction from CCSM-CAM4 is presented in Fig. R2_2 and Fig. R2_3, respectively (attached below), with those predicted and fully interactive in the ULAQ-CCM (Visioni et al., 2017b). This proves that the two aerosol latitudinal and vertical distributions are consistent, so that the aerosol direct radiative forcing applied in CCSM-CAM4 and regulating SST changes due to SG is consistent with that in the ULAQ-CCM. Finally, it is not true that CCSM-CAM4 has no stratosphere.

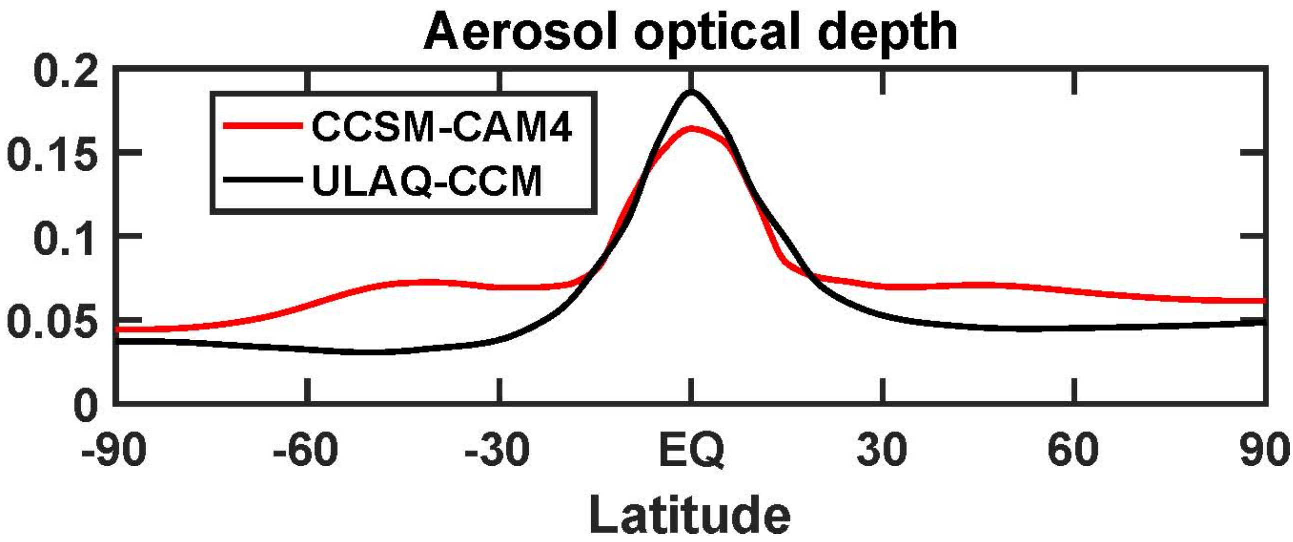

Fig. R2_2. Annually and zonally averaged SG aerosol optical depth at λ=0.55 µm used in CCSM-CAM4 and calculated in our study with the ULAQ-CCM.

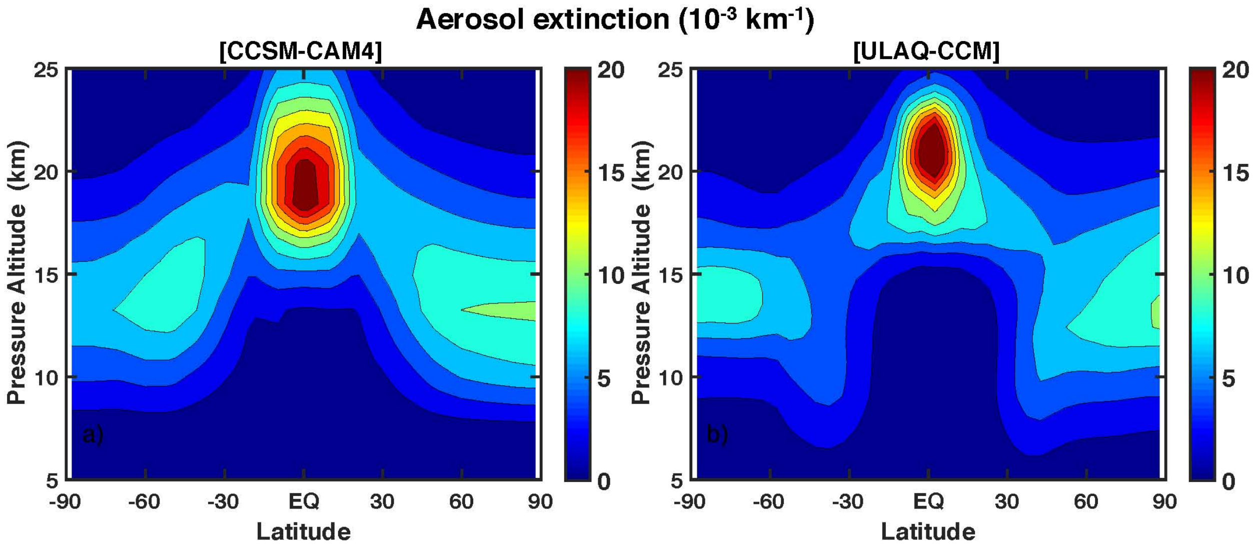

Fig. R2_3. Annually and zonally averaged SG aerosol extinction at λ=0.55 µm (10^{-3} km^{-1}) used in CCSM-CAM4 (left panel) and calculated in our study with the ULAQ-CCM (right panel).

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