

Interactive comment on “Climatic impacts of stratospheric geoengineering with sulfate, black carbon and titania injection” by A. C. Jones et al.

Anonymous Referee #2

Received and published: 7 January 2016

The authors compare impacts of stratospheric aerosol injection with three different materials, sulfate, titania, and black carbon on climate and stratospheric temperature and circulation with the help of simulations performed with the HadGEM2-CCS climate model. While possible effects of sulfate aerosol injection have been studied extensively this is not the case for the two other materials that have been proposed as potentially superior to sulfate. Hence, I see this study as a very useful addition to existing work of Ferraro et al. (2011) and Kravitz et al. (2012). However, I see a few general and a couple of more specific issues with this study that should be addresses before I can recommend publication.

The biggest concern I have is the not very cautious discussion of radiative forcing, anomalies of radiative fluxes, and the resulting impacts on temperature and precipi-

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tation. The issue starts with nomenclature as in the manuscript RF is used for both radiative forcings and fluxes (e.g. the acronym ARF is used for anthropogenic radiative forcing and TOA-RF for TOA radiative flux) which can be very different things. In this context I would also suggest not to use the term G3 for naming the experiment, as the original GeoMIP design was not only different in the use of a different baseline experiment (RCP4.5 vs. RCP8.5 as mentioned in the manuscript) but also in the goals: In GeoMIP G3 the goal was to keep the forcing constant at 2020 levels while here the goal is to “maintain TOA-RF [radiative flux] balance”. It is unclear to me what this exactly means. In 2020 the TOA-RF is imbalanced, and Fig. 3 seems to suggest that the goal was not to maintain the 2020 imbalance but to get back to the imbalance during the HIST period. In addition, it is unclear how radiative forcing and TOA flux imbalance are related. This needs to be discussed thoroughly. It is argued that the different temperature evolutions (occurring despite almost equal TOA-RFs) for the different materials are related to differences in tropopause-RF and TOA-RF, and the IPCC reports are cited. The citations are valid strictly only for radiative forcing, not for radiative flux anomalies used here. After stratospheric adjustment, these TOA and tropopause forcings should be equal. So it needs to be discussed in which sense in this context the flux anomalies represent the forcing, and if the stratosphere being permanently not-adjusted due to the changing emission rates is finally responsible for the different temperature effects of the different materials. Global mean temperature effects could be different even for the same global mean tropopause forcing if energy is distributed differently among atmosphere and ocean. Could this be the case, here? It is interesting that in the G3S case the near-surface air temperature is brought back to that of the HIST period, although the TOA net flux was constantly positive, indicating a constant energy flux into the Earth system. Where has this excess energy gone? Only into the stratosphere, or mostly into the ocean? Is energy conserved in the model? A proper discussion of fluxes and forcings is also important in order to properly explain the simulated precipitation changes. E.g. the possible effect of different temperatures on the precipitation in the different cases has been ignored.

C11245

ACPD

15, C11244–C11248,
2016

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A related issue is the choice of the HIST period as reference. There probably is no optimal choice of reference, but the choice should be motivated with the goals of the study and should fit to the (to me not clearly) defined goal of the geoengineering. To be honest, I think that an idealized experimental setup, e.g. in the sense of the GeoMIP G1 experiment with an exact compensation of an instantaneous forcing would facilitate the comparison of the three methods. Given that some of the differences (in particular stratospheric temperature changes) are large I'm still happy to accept this setup, but in the cases of more complicated signals (e.g. surface maps of precipitation) the origin of the signals as a combination of residual warming, increased CO₂ and aerosol injection of slightly different effectiveness for the three cases needs to be discussed more thoroughly. It could be an option to normalize results for the three methods before comparison.

I think it is useful that assumptions in the representation of the different aerosols are mentioned but I find it unsatisfactory that no attempt is made to estimate the effect of possible errors in these assumptions. This seems particularly important as in the standard case of Ferraro et al. (2011) titania heats the stratosphere less than sulfate, contrary to what is shown here.

Finally, I think that one potentially important side effect, the change of stratospheric water vapor related to the strong temperature increase in the tropopause region in particular for BC should be looked at. Furthermore it should be mentioned that not only the aerosols but also changes in water vapor and temperature would affect ozone and feed back to temperature.

In the following I list further issues according to their appearance in the text:

Abstract: It seems odd that the strong conclusion that “the severity of stratospheric temperature changes . . . exclude BC from being a viable option for geoengineering” is made only in the abstract. Conclusions are missing in the paper.

P44L23: Collins et al. 2013, not 2014? Furthermore, please check the reference list.

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It seems that not all cited papers are in the bibliography. I stumbled upon Dhomse et al. (2014) and Schmidt et al. (2013) but there may be more.

Fig. 1: Why don't the LW and SW coefficients do not agree when the ranges are overlapping?

P50L14: "...chosen to prolong the stratospheric lifetime ..." Prolog with respect to what?

P50L23: besides the points mentioned above, it would be useful to explain how this "altering" of the injection rate was done. Online using some sort of control algorithm?

P53L7: Recent research suggests that temperature feedbacks dominate the albedo feedback in producing arctic amplification (Pithan and Mauritsen, 2014).

P54L7: The global mean precipitation effect is difficult to read from Fig. 6f-h. Why not add precipitation time series to Fig. 3? Furthermore it might be useful in the following to provide some normalized value for global mean precipitation change (e.g. as change per degree of near surface air temperature reduction) instead of the pure anomalies in order to account for the different reductions in temperature for the different aerosol types.

P54L9: "... must be ameliorated by additional SW absorption ..." Not clearly formulated: Absorption at the surface?

P54L14: It is not clear to me how to read the forcing from Fig. S4.

P55L21: The sentence on the dependence of LW absorption on temperature could be easily misunderstood because it is mostly the LW emission (and hence the net absorption) that depends on temperature.

P56L5: I don't see a maximum warming of +7K for sulfate. Fig. 10 seems to suggest that it barely reaches 4K.

P56L7: Please add "tropospheric" to "tropical circulation".

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P57L11: “the periodicity of the QBO extends beyond the 10 year span . . .” This seems to me an odd way to express that no QBO-like oscillation can be detected in the 10-year time span.

P57L24ff: Niemeier et al. (2013) have argued that the LW absorption of aerosols affects the hydrological cycle. It may be useful to relate the apparently similar effect of SW absorption to their discussion.

P60L10: “Whilst research has shown SAI to be capable . . .” I’d formulate a bit more carefully. So far this is based on results from the volcano analogue and computer simulations. Surprises concerning the effectiveness of a potential application may seem unlikely but cannot be ruled out.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30043, 2015.

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