Atmos. Chem. Phys. Discuss., 13, C11058–C11064, 2014
www.atmos-chem-phys-discuss.net/13/C11058/2014/

Atmospheric Chemistry and Physics
Open Access
Discussions

Interactive comment on "Sensitivity of simulated climate to latitudinal distribution of solar insolation reduction in SRM geoengineering methods" *by* A. Modak et al.

A. Modak et al.

amatcaos@caos.iisc.ernet.in

Received and published: 15 January 2014

Anonymous Referee #1 Received and published: 15 November 2013

I recommend that this paper be rejected for four reasons: 1. The proposed latitudinal and vertical distributions of sulfate aerosols are impossible to produce with any known technology, as stratospheric circulation would move aerosols poleward and then remove them. Therefore, I don't understand why the experiment was done. Also, see more discussion in item 6 below.

We agree with the reviewer that the practical implementation of the proposed latitu-

dinal distributions of sulfate aerosols in the stratosphere is not feasible. In the original manuscript as well as in the last paragraph of 'Introduction' section in the revised manuscript, we state: "We caution that our simulations are highly idealized and they are not meant to represent realistic latitudinal distribution of aerosols in geoengineering scenarios. Rather, they are designed to elucidate the fundamental properties of the climate system when the latitudinal distribution of aerosols and hence solar insolation reduction is systematically altered."

2. The results are obvious, and do not represent new scientific understanding. It is obvious that if you block sunlight in the tropics you will get more cooling than if you do it at the poles, since there is more sunlight in the tropics. You do not have to conduct GCM simulations to get this result.

We appreciate but disagree with the reviewer. This is the first study that quantifies the effect of a systematic variation of aerosols for SRM schemes. In this study, we find that for a fixed total mass of aerosols prescribed into the stratosphere with different latitudinal distribution considered in this study (that is, even with non-uniform distribution of solar insolation reduction), it is not possible to offset both surface temperature and precipitation changes simultaneously in a geoengineered world. We also find that the global mean climate is warmer and wetter when aerosol concentration is maximum over the poles relative to the uniform distribution case (which mitigates global mean temperature change) while the opposite is true when aerosol concentration is maximum in the tropics.

3. The authors use an old model, and it does not simulate enough of the climate system to learn anything fundamental (as claimed) about the climate system. The authors do not include an oceanic GCM nor do they include a proper stratosphere and mesosphere. If you are going to look at the impacts of sulfate aerosols, you need to include a good upper atmosphere, so as to get the proper dynamical response. Furthermore, to get the correct hydrological response, you need an ocean.

We appreciate the comments again but all these limitations and caveats are discussed in the original manuscript itself. The model (CAM3.1 coupled to CLM3.0, slab ocean model) used in this study is not very old. It is comprehensive enough to use it to study the basic climate variables: radiative forcing, surface temperature and precipitation. Also, almost one and half years back we have performed the simulations for this study and this model was reasonable to use at that time.

We agree with the reviewer that our model lacks an oceanic GCM. This we have stated in the 'Discussion and conclusion' section, "Our model lacks a dynamic ocean and sea ice components, and thus the effects of deep ocean circulation are not modeled here". However, we have included slab ocean model and a thermodynamic sea ice model which we feel is sufficient to do a geoengineering study.

We also agree that proper stratosphere and mesosphere is absent in the model. In the revised manuscript we have included this as one of the limitations of this study. Since this study mainly focus on the global mean climate so we believe that presence of proper stratosphere and mesosphere would not make much difference on our results on temperature and precipitation.

4. The authors use the wrong effective radius for sulfate aerosols, and thus produce the wrong impact on radiation (line 15, p. 25391). The number they use is for volcanically quiescent times, when there would be tiny sulfate particles. After the 1991 Pinatubo eruption, the effective radius was about 10 times larger than this, and some research (e.g., Heckendorn et al., 2009) suggests that the size should be even larger during geoengineering since the particles would grow as SO2 is added.

We thank the reviewer for the comment on the effective radius of the sulfate aerosols. The radius used is exactly the same as in previous studies. In the manuscript we state: "Sulfate aerosol particle size is prescribed and is assumed to be log-normally distributed with dry median radius $\approx 0.05\mu$m and geometric standard deviation ≈ 2.0 (as used in a geoengineering scenario in a previous study (Rasch et al., 2008b))".

Further, we have devoted an entire paragraph (3rd para) in the last section to discuss the limitations related to prescribing aerosols in our study.

Furthermore, the paper has the following additional issues:

5. WG I IPCC report. Radiative forcing needs to allow rapid climate responses, andis measured as the change in net radiation at the tropopause. (lines 21-22, p. 25390).

We have used Hansen's fixed-SST (sea surface temperature) forcing method to quantify the radiative forcing. In this method we prescribe sea surface temperature and sea-ice concentration and run the model. This allows the fast adjustments of the atmosphere to occur. Then we compare simulations with and without the forcing to calculate the radiative forcing.

In the revised manuscript, in the 2nd paragraph of section 2, we now write "This method allows the rapid adjustment of the atmosphere and land components before radiative forcing is evaluated."

6. I don't understand why sulfate is added at the top of the atmosphere. This is impossible to do. What the authors are really doing is just reducing the solar insolation. If they are using sulfate, it has to be introduced into the lower stratosphere, so that it can produce stratospheric heating, ozone depletion, and the resulting changes in stratospheric and tropospheric circulation. It makes no sense to claim that you are putting a sulphate cloud outside the atmosphere, and to not include the important climate responses that this cloud would produce.

We agree with the reviewer. However, we have already stated this in the "Model and Experiments"section, "Since aerosols are prescribed at TOM, the effect is essentially equivalent to making latitudinal changes to the solar constant." One of the previous studies, Ban-Weiss and Caldeira (2010), has also prescribed additional aerosols at the top layer of the model.

In the revised manuscript, at the end of section 2, we now write "An experiment where the same total mass (12.6Mt) of aerosol is distributed uniformly over the globe between 61hPa to 9.8hPa (15 – 30km) with a maximum at 30hPa (22km) showed that the radiative forcing is nearly the same as in our uniform distribution geoengineering case and hence the main conclusions reached in this study are unlikely to be altered."

7. The authors discuss the CO2 physiological response to plants, but ignore the impact of diffuse radiation from sulfate forward scattering. Anyway, they will have the wrongdiffuse radiation because they use the wrong size for the sulfate aerosols.

We have not discussed the impact of diffuse radiation from sulfate aerosols because interactive land carbon cycle is missing in the model. In the revised manuscript, we write in the penultimate para of the last section, ". Further, in this model an interactive land carbon cycle is not included and hence the impact of changes in the diffuse fraction of surface solar radiation due to stratospheric aerosols could not be investigated. We intend to use a later version of the model that includes carbon cycle to investigate the impacts of altered diffuse radiation in a future study."

8. On page 25392, they claim that they do not get a balanced global average surface temperature because of a CO2 feedback. But this just means that they did not do agood enough job balancing the forcing. The balance should have taken this feedback into consideration, as all the other feedbacks were.

Thanks for this comment but we have not discussed any CO2 feedback here. We are discussing the direct effect of CO2 called the Physiological effect which involves stomatal closing and associated warming. This is discussed in that paragraph.

In the last paragraph of Model and experiments we state "Our choice of 12.6 Mt for Q is dictated by the uniform distribution case which had near-zero global mean temperature change relative to the control case." Therefore, the TOA radiative balance is well taken care in the uniform case. In the paragraph (line 13-20, page 25392 which the reviewer refers to) we discuss about another geoengineering scenario (Polar1) with more aerosol concentration at the poles and not the uniform distribution geoengineer-

ing case. For Polar1 case we have prescribed same amount of total additional aerosols but with different distribution. In this Polar1 case we have not designed the latitudinal distribution to achieve a near cancellation of surface temperature or the radiative forcing; the only constraint is the fixed additional total mass (12.6Mt). So, we claim that though the forcing is almost zero in Polar1 scenario, we observe slight warming which is because of the CO2 physiological forcing. In the revised manuscript, we clarify now by explicitly referring to Polar1.

9. That temperature and precipitation changes cannot both be kept at zero is a well known geoengineering result and not new. There are many papers that have found this. The claim that this is interesting, p. 25394, lines 5-7, is not correct. The authors do not cite previous papers that show this, with the latest being Tilmes et al.(2013), The hydrological impact of geoengineering in the Geoengineering Model Intercomparison Project (GeoMIP). J. Geophys. Res. Atmos., 118, 11,036-11,058, doi:10.1002/jgrd.50868.

We thank the reviewer for this comment and the suggestion for the published work. In the revised manuscript we have cited this latest work.

We are considering our finding to be interesting because this study show when we fix the total mass of additional aerosols prescribed in the stratosphere and vary only the latitudinal distribution it is not possible to offset both surface temperature and precipitation changes simultaneously not only in global mean but also for ocean or land means in all geoengineered cases. From past studies we only know that we cannot counterbalance both surface temperature and precipitation simultaneously in global mean.

10. The panels in Figs. 7-10 are so tiny that they are illegible. Figures need to be bigger than postage stamps (sorry for the old 20th Century technology reference).

We have improved the quality of all the figures in the revised manuscript. We have spilt figure 10 into 3 figures with 8 panels each. This revision has improved the readability of original figure 10. Since seasonal cycle discussion is irrelevant for this study, we have removed figure 9 and the panels that show seasonal variation of radiative forcing in figure 7.

11. The attached annotated manuscript has a number of other comments.

Thanks for the comments. We have answered the comments in the manuscript itself. It is attached as a supplement.

(Note: Also please see the attached supplement pdf contains revised manuscript)

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C11058/2014/acpd-13-C11058-2014-supplement.pdf

---

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25387, 2013.