

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

Anonymous Referee #1

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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.
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

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GCM simulations to get this result.

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.
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.

Furthermore, the paper has the following additional issues:

5. The authors use an old, wrong definition of radiative forcing. See Chapter 8 of the AR5 WG I IPCC report. Radiative forcing needs to allow rapid climate responses, and is measured as the change in net radiation at the tropopause. (lines 21-22, p. 25390).
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 sulfate cloud outside the atmosphere, and to not include the important climate responses that this cloud would produce.
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 wrong

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diffuse radiation because they use the wrong size for the sulfate aerosols.

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 a good enough job balancing the forcing. The balance should have taken this feedback into consideration, as all the other feedbacks were.

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.

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).

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

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C9109/2013/acpd-13-C9109-2013-supplement.pdf>

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