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Interactive Comment

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

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Anonymous Referee #2 Received and published: 20 November 2013

It is not clear what this study adds to scientific understanding. The main conclusions are not substantially different to previous work.

We appreciate the reviewer's comment. We agree that few previous studies have investigated the optimization issues but they do not provide a clear insight into a systematic variation of the hydrological cycle (for example, our Figure 1b) with the location of the mean mass of the aerosols. This is spelt out in the introduction: "However, a simple and

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Interactive Discussion

Discussion Paper

clear understanding of the effects of systematically varying the latitudinal distribution of aerosols and hence solar insolation reduction (e.g., more concentration in the tropics or high latitudes) on the hydrological cycle and surface temperature is lacking. In this study, we perform multiple idealized SRM geoengineering simulations with constant total amount of sulfate aerosols but with systematically varying latitudinal distribution."

At first I was confused that adding sulphate aerosol at the poles should warm the planet. It was not clear that the responses given were differences to the 1xCO2 simulation and therefore also included 2xCO2.

We thank the reviewer for pointing out this confusion. We clarify now that the changes discussed in the paper are calculated with respect to the control climate (1xCO2). The radiative forcing calculated for the geoengineering simulation is the net radiative forcing from a doubling of CO2 and additional aerosols. Therefore, we find a positive radiative forcing when the aerosol concentration is larger over the poles since the global mean CO2 forcing is larger than the shortwave forcing caused by aerosols in this case.

There are a large number of figures that do not add anything to the conclusions, eg Fig 2, 5, 7, 8, 9, 10 and 11. Also many of the figures are too small to read. There is a lot of repetition in the text of the main conclusions.

We appreciate the suggestion here. We agree with the reviewer's concerns: the figures lack clarity. This is partly because we have overlooked this at the proofing stage. It is also partly because of smaller font size for labels in the figures. In the revision, we have increased the font size in most figures and also made sure all the figures are clear. In the revised manuscript we have improved the quality of the figures. However, we have not removed all the mentioned figures as we believe that most of the figures are important to support the basic message of the paper. Moreover, since this study use a global climate model, the regional pattern of changes could be also inferred. Hence, we have included some latitude-longitude plots. However, we have removed figure 9 and the panels that show seasonal variation of radiative forcing in figure 7.

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Since Figure 10 has 24 panels, we agree that it was hard to read. Hence, in the revised manuscript, we have split this figure into 3 with 8 panels each. This revision has improved the readability of original figure 10. The repetitions are deleted in the main conclusions.

Line 23 p 25392: uniform distribution does not completely mitigate temperature change.

Thanks for this comment. In the revised manuscript we have changed the statement to 'uniform distribution nearly cancels the temperature change'.

Line 1 p 25393: not clear what you mean by 'heat the atmosphere only' and how this is different to CO2 forcing. Line 6 p 25393: 'Therefore the precipitation change....' does not follow on from the previous sentence and what 'fast response component' are you talking about?

We thank the reviewer for this comment. In the revised manuscript we have provided a detailed discussion with clarification on fast and slow response components. We now write "This occurs because of differing fast response (changes that occur before global mean surface temperature) in precipitation for solar and CO2-forcing (Allen and Ingram, 2002; Bala et al., 2008; Bala et al., 2009; Andrews et al., 2009): longwave absorption by CO2 in the atmosphere can contribute to increased vertical stability and suppress precipitation but this fast response mechanism is nearly absent for solar forcing because the atmosphere is nearly transparent to solar radiation. However, since the slow response (changes that are associated with global mean surface temperature change) is same for CO2 and solar forcings (Bala et al. 2010), the total changes in rainfall are larger to solar forcing than to equivalent CO2 forcing. Because of this differing hydrological sensitivity to solar and CO2 forcing, insolation reductions (in geoengineering scenarios) sufficient to offset the entirety of global-scale temperature increases would lead to a decrease in global mean precipitation."

Line 2 p 25394: 'The reduction in precipitation...' in what way are your results consistent

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with observations following Pinatubo?

Thanks for this comment. In the revised manuscript we have clarified how our results are consistent with observations following Mount Pinatubo eruption by stating: "Our "Tropics" simulations can be compared to Mount Pinatubo eruption because the distribution of aerosols in 'Tropics' simulations have reasonable resemblance to the distribution of volcanic aerosols after few weeks of the eruption (the volcanic aerosols occupied a latitude band of 20o S to 30o N (McCormick et al. 1995))."

Trenberth and Dai (2007) reported substantial reduction of precipitation over land and decrease of run-off over ocean after the eruption. Similarly, we also find significant reduction in precipitation in our 'Tropics' simulations. So, we consider that our results are consistent with the observations following Pinatubo eruption.

We have included this reference in the revised manuscript: McCormick, M. P., L. W. Thomason, C. R. Trepte, Atmospheric effects of the Mt. Pinatubo eruption, Nature, 373, 399–404, 1995.

Lines 3-16 p 25395: I don't understand this at all.

We appreciate the reviewer here since we did not discuss the reason for calculating the root mean square difference. In the revised manuscript, we have now first discussed the reason for looking at the normalized root mean square difference (NRMSD) and then discuss the results: "To further investigate the degree of departure of the different geoengineering simulations from the control, we calculate the root mean square difference between the spatial patterns in geoengineered climates and the control climate and normalize this root mean square difference by the standard deviation of the control scenario (NRMSD). A value less than 1 for this NRMSD would suggest that the geoengineered climate is indistinguishable from the control climate. Further, the geoengineering simulation with the smallest value for this quantity is the one that is closest to the control."

Section 3.2 I don't know what the points of all these figures are and what the important messages are. The significance hatching is not clear on the figures and doesn't make sense to me. Some plots show hatching over areas with smaller delta T than in other plots with no hatching. If geoengineering is working you would want the residual changes to be insignificant in as many places as possible. I can't follow your seasonal cycle discussion.

We agree with the reviewer's concerns: the figures lack clarity. This is partly because we have overlooked this at the proofing stage. It is also partly because of smaller font size for labels in the figures. In the revision, we have increased the font size in most figures and also made sure all the figures are clear.

Significance hatching depends on both mean delta T or P and the standard error. We hope the reviewer is comparing only panels of the same type (either all the left panels only or the right panels only)

Since seasonal cycle discussion is irrelevant for this study, we have removed Figure 9 and the corresponding discussion. We also remove the seasonal cycle panels in Figure 7.

Section 4: Don't introduce more figures now that don't seem very relevant.

Here, we have a figure which emphasizes how the "forcing and response" concept is able to explain in a very simple way even when there is complexity in the distribution of aerosols. We feel this is an important figure (Figure 12) to convey this message.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C11065/2014/acpd-13-C11065-2014-supplement.pdf

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