

Response to reviewer #1

Kravitz et al., esd-2015-51-discussions

Original reviewer comments in Roman typeface. **Responses in bold.**

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Reviewer #1

General comments:

This paper is of good quality and a very interesting topic. I do not feel adequately qualified to review large sections of it as the details of the feedback and control theory are out of my area of expertise and I found it difficult to follow as a newcomer to the subject. I do feel able to comment generally on the climate results.

Specific comments:

1. p.1638 line 10: Keller et al 2014 compared several carbon dioxide removal schemes but only one type of SRM (solar constant reduction) so does not seem an appropriate reference here. Crook et al. JGR 2015 and Niemeier et al. JGR 2013 also showed some common features and some differences of several SRM schemes.

**We thank the reviewer for the reference suggestions and have incorporated them.**

2. p.1638 line 13: But Niemeier et al. JGR 2013 and Ferraro et al. Environ. Res. Lett. 2014 showed precipitation changes are quite different for solar reduction compared to stratospheric aerosols.

**We acknowledge the reviewer's point, and we fully expect there to be climate differences between solar reduction and stratospheric sulfate aerosols. We believe the issue here is with our phrasing, which we acknowledge needs to be more careful. In terms of global mean near-surface climate effects, Niemeier et al. show a quantitatively similar evaporation response to solar reduction and sulfate aerosol injection; the yellow and red lines overlap in multiple places, and the maximum difference between the two lines is approximately 0.015 mm/day, or 0.5% of the base value. Ferraro et al. (2014) show that differences in precipitation change are approximately 0.1 mm/day, or 3% of the base value (assuming a base value of 3 mm/day). Moreover, Ferraro et al. used a prescribed sulfate aerosol distribution instead of allowing the aerosols to spread dynamically, which changes the spatial pattern of the radiative forcing, so it's difficult to attribute causes to differential climate effects. We do recognize that changes aloft are likely to be very different, in large part due to stratospheric heating, and also there are likely to be differential regional effects. We have rephrased this sentence to be more specific when we are discussing global vs. local effects.**

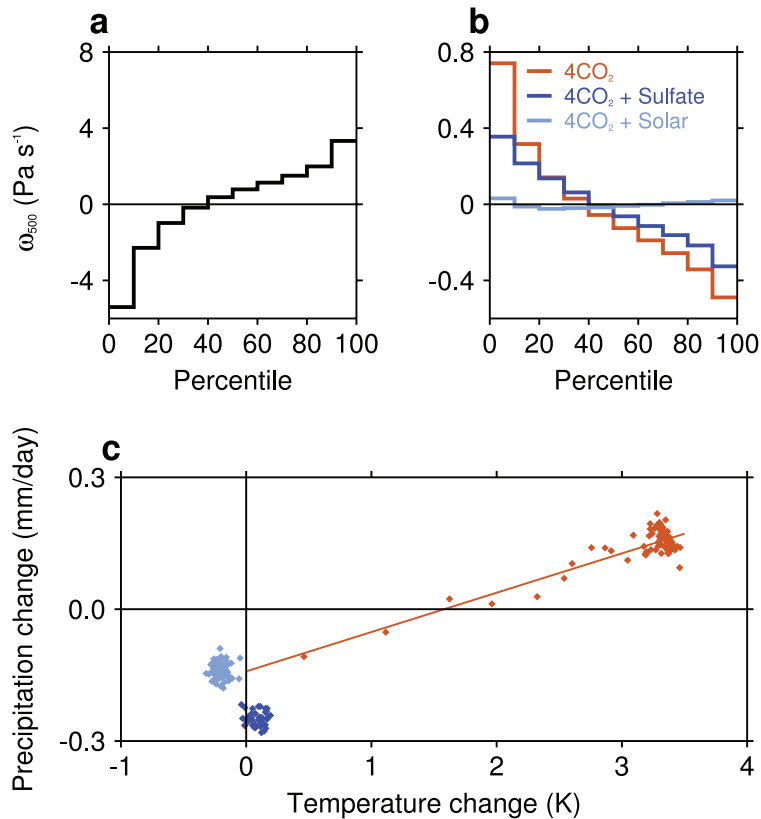
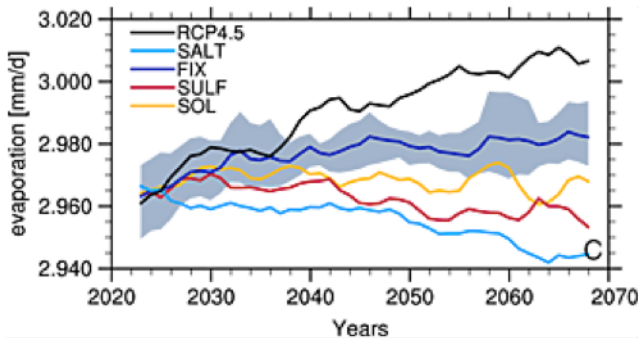


Figure 4. Percentiles of tropical-mean pressure vertical velocity at 500 hPa ( $\omega_{500}$ ) for (a) control and (b) response compared to the control simulation in  $4\text{CO}_2$  and geoengineering simulations. (c) Scatter plot of annual-mean tropical-mean surface temperature and precipitation anomalies relative to the control simulation, showing the fast radiative response to  $\text{CO}_2$  where the linear fit intercepts the temperature axis.

3. section 3: I don't have enough familiarity with the mathematical terms and control theory in this section to comment on this. I found it very difficult to follow.

**We intended this section to be a “practitioner’s guide” to control theory for geoengineering applications, and we have found that it becomes clear when actually attempting to design a feedback loop. We would be interested in addressing any specific concerns the reviewer may have.**

4. p. 1660 lines 13-14 : why do you have repeated symbols?

**We now realize that xi and zeta look too much alike, so we have changed zeta to**

**lambda. Hopefully this improves readability.**

5. Section 3.7: It would have been helpful to see an influence matrix for this 3x3 case as in equation (20).

**Added.**

6. Equation (25): what do the dashes mean? Why are these blank if they are indistinguishable from zero, why not just zero?

**Thanks for pointing out this oversight on our part. We have added “where dashes in the matrices indicate that the estimate is indistinguishable from error and does not have a strong physical connection”.**

7. Equations (27), (28) and (29) what are  $u_1$ ,  $u_2$  and  $u_3$ ? Are these the amounts of the 3 patterns of solar reductions described in equation (3)?

**We have changed these to  $\Delta L_0$ ,  $\Delta L_1$ , and  $\Delta L_2$  to harmonize indices and notation.**

8. Figure 14: it's quite hard to see the changes to precipitation centroid.

**We have narrowed the latitude range to improve clarity.**

9. p. 1672 lines 18-19: how does converging in 9-10 years indicate a 2-3 year timescale. I don't understand why  $L_1$  isn't held constant after some period rather than going toward zero. Surely the CO<sub>2</sub> warming is continuing to warm the NH more? I am not sure what the figures are showing now. Is it the change each year in  $L_0, L_1, L_2$  which are added on top of the previous year solar reductions, or the actual solar reductions?

**We thank the reviewer for pointing out that our description was lacking. We have now added a paragraph that discusses these issues in more detail.**

**The values shown in this figure are the actual changes. If they were cumulative changes, the reduction in  $L_0$  would exceed 1.0 by year 70, meaning there is no solar flux.**

10. Table 1: I am not sure why PI control has such large RMS. Why is it not 0? Surely the PI control is your reference climate and that's what you want to achieve in term of Arctic temp and precipitation centroid?

**This was insufficiently described in the manuscript. We have corrected it to say, “...as the RMS of interannual deviations from the long-term preindustrial control...” By this definition, we would not expect the RMS value to be 0 due to internal variability.**

11. Same applies to Table 2.

**Also changed.**

12. Why did you plot maps for the mean of only the last 10 years? Why not 30 years? Would it make a difference?

**There is a balance here. Shorter averaging periods will be noisier, but against a transient simulation, shorter periods will show better signal by avoiding the early parts of the simulation where there is less departure from the control run. Given that the difference between a 10-year average and a 30-year average is not answer-changing (except for the issues just described), we opted for a 10-year averaging period.**

Technical corrections: 1. There are two Kravitz et al 2015 papers in the reference list differentiated as 2015a and 2015b but all references to these in the text are just 2015. Please clarify which reference you mean.

**Corrected.**

2. p.1644 line 7: change ‘a a’ to ‘a’. 3. p. 1645 line 10: change ‘would need be’ to ‘would need to be’.

**Corrected.**

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