

Reply to the comments from Anonymous Reviewer-1

I reviewed an earlier version of this manuscript that was submitted to another journal. I think this version is far improved over the previous one. Many of the results are perhaps not all that surprising, especially given that other studies (which the authors cite) have looked at the different effects of injection altitude. However, I have not necessarily seen all of these results in one place, which makes this paper interesting. The addition of Section 3.5 is very interesting, and I learned quite a bit. I am recommending just a few minor revisions.

We thank the reviewer for the time spent on evaluating our manuscript.

General comments:

As the authors say, they don't include any dynamics or transport. However, radiative forcing and climate response to stratospheric aerosols definitely depend on dynamics. I would appreciate the authors adding some description as to how this compromise might have affected their results.

We have written that only the transport of aerosols is not modelled. However, the stratospheric dynamics is included in our simulations.

Specific comments:

In the last paragraph on Page 5 (going into Page 6), some context for these results is needed. Do these numbers make sense, and why? (I think they make sense, but I'd like for you to say so.)

The TOA radiative imbalance discussed in the paragraph are actually the prescribed-SST radiative forcing as discussed in several previous studies (Bala et al., 2010; Modak et al., 2014; Nalam et al., 2018). This imbalance is corrected for the land surface temperature change in the prescribed SST simulations, to obtain the TOA radiative forcing in the two-point method as discussed in Modak et al., (2018) and Duan et al., (2018) and in the supplemental sect. S1. We discuss this in the revised text.

Page 8, line 13: Can you phrase this in a different way? 1xCO₂ is your baseline, so it doesn't cause any cooling.

We have rephrased this line in the revised version as “.....which attains only 70% of the cooling in 1XCO₂ relative to 2XCO₂.”

Figure 4: I'm not sure hatching is necessary. All of the regions are statistically significant, so just say that.

We have adjusted the transparency of the hatching in the revised version.

References:

Bala, G., Caldeira, K. and Nemani, R.: Fast versus slow response in climate change: Implications for the global hydrological cycle, *Clim. Dyn.*, 35(2), 423–434, doi:10.1007/s00382-009-0583-y, 2010.

Duan, L., Cao, L., Bala, G. and Caldeira, K.: Comparison of the Fast and Slow Climate Response to Three Radiation Management Geoengineering Schemes, *J. Geophys. Res. Atmos.*, doi:10.1029/2018JD029034, 2018.

Modak, A. and Bala, G.: Sensitivity of simulated climate to latitudinal distribution of solar insolation reduction in solar radiation management, *Atmos. Chem. Phys.*, 14(15), 7769–7779, doi:10.5194/acp-14-7769-2014, 2014.

Nalam, A., Bala, G. and Modak, A.: Effects of Arctic geoengineering on precipitation in the tropical monsoon regions, *Clim. Dyn.*, 50(9–10), 3375–3395, doi:10.1007/s00382-017-3810-y, 2018.