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Interactive comment on "Changes in clouds and thermodynamics under solar geoengineering and implications for required solar reduction" *by* Rick D. Russotto and Thomas P. Ackerman

Anonymous Referee #2

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Review of "Changes in clouds and thermodynamics under solar geoengineering and implications for required solar reduction" by Russotto and Ackerman, submitted to Atmospheric Chemistry and Physics

This manuscript presents multi-model analysis of simulations from the GEOMIP G1 experiment, in which CO2 is quadrupled while the solar constant has been reduced sufficiently (through iteration) to achieve near zero global mean surface temperature change. The work is motivated by the observation that the required solar constant reduction is greater than the value that would exactly offset the effective radiative forcing from 4xCO2 in the global mean planetary energy budget. Well-established tools

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such as radiative kernels and the APRP method are used to quantify the partial contributions of various cloud and clear-sky mechanisms to the total radiative changes in order to understand why a greater than expected solar constant reduction is required. A key finding is that there is a widespread, robust reduction in low cloud fraction in the models, which increases the necessary solar constant reduction to offset CO2-driven warming. This cloud reduction is at least qualitatively consistent with widespread reductions in two different measures of lower tropospheric stability.

This manuscript was well-written (although I did find it rather long and wordy at times). The figures are good and I find no fault with any of the analyses. My main difficulty with this work is with the framing of the central question and many of the results. Equation (3) is just a statement of an energy budget. The authors state (page 5, line 15) that they will "test the hypothesis that the solar constant reduction can be predicted using Equation 3". I don't think there is any such hypothesis, because Equation (3) has no predictive power until the adjustment terms are known. And the sum of the adjustment terms are by definition what's needed to close the budget. So I see circular reasoning. The only way to use this framework to get \Delta S_0 is to calculate the adjustments, but this is only done a posteriori by running the models. This is in fact noted by the authors several times (page 24, line5; page 25, line 13). Given this limitation, I don't see why the authors are presenting this work as a test of such a hypothesis. The fact that the budgets approximately balance in Figure 11 is really just an approximate validation of the analysis techniques (kernels, APRP) – it does not represent a conceptual validation of any physical or predictive framework.

To me a more interesting question would be to look at differences between adjustments *actually achieved by the models* (as analyzed here) and the traditional notion of *adjusted radiative forcings* for which SSTs are held fixed. These will not be the same in this context, because even though the simulations feature near-zero surface warming, there are *local* SST changes almost everywhere, which surely have interesting consequences for atmospheric stability and cloud processes. There may be (probably

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are) "rapid adjustments" to solar forcing that are quite different than the eventual comprehensive adjustment of the models after allowing the SSTs to change. These could be evaluated by carrying out Hansen-style fixed SST experiments, even with a single model. The manuscript does not say much about the role of the flattened equator-to-pole temperature gradient on the radiative effects, which seems like a missed opportunity to learn more about the relevant physics.

That said, I think the results themselves are interesting and sound, and they should be published after a slight reframing of the central questions. Keep the focus simply on answering why the required solar constant reduction is larger than expected.

My other substantial criticism is regarding timescales. Nowhere in this manuscript did I see any mention of the transient nature of the response. This seems important enough to merit some thought. As far as I've understood, these are coupled model simulations. The climate will continue to adjust long after 50 years, with implications for the spatial pattern of SSTs and consequent radiative feedback processes. The paper seems to treat the 50-year response as an equilibrium, which it surely is not. If I have misunderstood and these are actually slab ocean calculation, then the interpretation is more appropriate, but this should be clarified in the text.

Detailed issues:

- Page 2, Line 25: "One might intuitively expect...." This seems like a strawman argument. I would not expect this. Forcing and feedback are not the same thing. If others have suggested that these things should be correlated, then provide a citation here.

- Page 5, line 1: A reference to Hansen et al. (2005) would be helpful for readers who need clarification about the various concepts of radiative forcing.

- Page 5, line 11: It was not clear at first why the authors are referring to 50 years here. Later it becomes evident that that is the length of the GEOMIP simulations. That should be clarified.

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- Page 5, line 20: "ant"

- Figure 6: the results here are presented qualitatively. Why not compute a spatial correlation between EIS and low cloud changes?

- Page 13, first paragraph: This is a good discussion of cloud controlling factors.

- Page 13, bottom: I realize that these are more speculations than results per se, but I feel like this discussion confuses stocks vs. flows of water vapor. Reduced evaporation does not necessarily imply reduced boundary layer humidity.

- Page 17, line 14: I find it convoluted to describe the decreased OLR due to cooler temperatures to be "warming effect"

- Page 17, line 19: I think these statements are inaccurate. The correction is not just for cloud masking of CO2 changes. A more important correction embedded in equation (5) is that differences in CRE depend on clear sky changes as well as cloud changes.

- Figure 10 and 11: I guess I'm not sure why these are presented as separate figures? The authors are quick to point out that figure 10 is misleading (page 21, line 15). Why not combine Figs. 10 and 11 and avoid potential confusion.

- Page 23, line 5: This is the traditional "stratosphere adjusted" contribution to radiative forcing, e.g. Hansen et al. (2005)

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