

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-503

Anonymous Referee #2

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Referee comment on "Impact of stratospheric aerosol intervention geoengineering on surface air temperature in China: a surface energy budget perspective" by Zhaochen Liu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-503-RC2>, 2021

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This is a carefully done and well written study. I appreciate that the authors are able to use old simulations to do new science. I have a few minor comments and one major one.

My major comment is similar to that of Reviewer #1 about the novelty. I think there are good reasons to look at specific regions and try to understand them better. Such investigations could warrant a new paper if they provide insight. My problem is that the authors have not provided much insight that is specific to China. The analyses they did could easily be applied anywhere in the world. It would be much more useful to add some discussion about something specific to China that requires more in-depth analysis. I won't decide for the authors what they should focus on – there are lots of things to choose from.

I would appreciate a much more nuanced picture of geoengineering than you're providing. On lines 52-53, you talk about slowing of the hydrologic cycle. That's true, but climate change accelerates the hydrologic cycle, with some pretty bad consequences for a lot of people. On lines 54-55, you talk about overcooling of the tropics and undercooling of the poles. That is not a foregone conclusion – see Kravitz et al. 2016 (ESD) or 2017 (JGR). On lines 55-57, you talk about termination. That is a risk, but it's less of a risk for lower magnitude deployments of geoengineering and a greater risk for higher magnitude deployments. Your discussion of risks needs appropriate context. Another example is line 62ff. What you say is true, but it depends on the amount of geoengineering. Irvine et al. (2019) found that with only a little bit of geoengineering, most regions would benefit under a wide variety of metrics.

Line 69: China has been studied in several papers, but not explicitly or in much detail. Also see my major comment above.

Line 175: This is a strawman argument. It wasn't designed to return the temperature to climatological RCP4.5 levels.

Lines 200ff: Instead of increased downward LH, it should be decreased upward LH. The actual LH doesn't become negative.

Lines 204ff: Why does LW decrease? (My guess is water vapor, which you talk about later in the paper. But you should say so here.)

Line 212: These changes are quite small. Or did you mean these to be 10, 4, and 6%, respectively? (Same comment for lines 224-225.) In either case, are the changes statistically significant?

Line 221: I don't understand this explanation. Cooling should reduce upward LW, not increase it.

I like Figure 11. I've never seen something that clear before.