Response to Referee 2

Overview:

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.

Major Comments:

(1) 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.

Response: Following your suggestion, we have emphasized the importance of analyzing the effect of SAI on surface air temperature over China in the revision (Lines 75–79). As the largest developing country in the world, China plays an important role in combating climate change. China's attitude to SAI is crucial to the international geoengineering research community. Considering the combined effect of the Tibetan Plateau and the East Asian monsoon, the climate over China would be strongly influenced by SAI. But few studies have studied the temperature response to SAI geoengineering over China explicitly. In the revision, the revised diagnosis analyses based on surface energy budget quantify the effect of downward energy fluxes changes on surface air temperature over China under SAI forcing.

China is a big country with complex topography. This feature leads to the surface albedo feedback change with large seasonal and spatial variations under SAI forcing. The negative surface albedo feedback related to increased snow cover fraction also amplifies the surface cooling, especially over the Tibetan Plateau in summer, and over northwestern and central China in winter. The land surface processes in models affect the spatial pattern of SAI-induced surface air temperature changes over

China. We believe that this revised analysis provides new scientific insights.

Specific comments:

(1) 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.

Response: We have improved this part in the revision. As you suggested, the SRM geoengineering would reduce the global mean precipitation and monsoon precipitation and slow the hydrological cycle if it is used to offset the GHG-induced global warming. This sentence has been rewritten for clarity (Lines 60–62).

(2) One 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).

Response: This foregone conclusion has been deleted in the revision as you suggested.

(3) 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.

Response: We agree that the severity of the termination effect depends on the magnitude of geoengineering deployment. This statement has been added to the revision (Lines 65–66).

(4) Your discussion of risks needs appropriate context. Another example is line 62. 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.

Response: We agree that the appropriate SRM geoengineering may lead to global cooling and benefit most regions. This part has been rewritten accordingly (Lines 69–71).

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

Response: This sentence has been rewritten for accuracy (Lines 84–85).

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

Response: We intend to indicate that although the injection of 5 Tg SO_2 per year can lead to a surface cooling over China, the climatological temperature in G4 is still higher than the present level. This sentence has been rephrased in the revision (Lines 200–201).

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

Response: In the revision, the "downward latent heat flux" statement has been rephrased accordingly (e.g., Line 230).

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

Response: The decrease in downward surface LW is mainly due to the tropospheric cooling and reduced atmospheric water vapor. The relevant explanation has been added to the revision accordingly (Lines 240–241).

(10) 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?

Response: In the original manuscript, the summer cloud cover changes are 0.10%, 0.06% and 0.04% among models exactly. These changes are pretty small and insignificant, which are mainly due to the large spatial dispersion. In the revision, we have mainly discussed the robust change for individual regions (e.g., Lines 243–246). The discussion about the regionally averaged change has been removed.

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

Response: The original "increases" should be "decreases" here. This sentence has been removed in the revision.

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

Response: We are glad you like it. This diagram has been improved in the revision (Fig. 12).