Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-503

Anonymous Referee #1

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-RC1, 2021

This paper analyzed changes in surface air temperature over China in response to stratospheric aerosol injection (SAI) geoengineering. The authors did so by analyzing existing G4 multi-model SAI simulations from the Geoengineering Model Intercomparsion project. The authors used the method of Kashimura et al. (2017) to decompose SAI-induced changes in shortwave radiation into components of solar reduction, albedo change, cloud cover change, and water vapor change. Using this method, the authors further examined spatial pattern of surface shortwave radiation change and its contributors over China.

This paper itself is straightforward and scientifically sound. However, it lacks scientific insight. I see little new scientific findings this paper offers. It essentially applies an existing method and uses existing multi-model dataset to a regional study. To put it in another way, if similar studies are done for another country/region, does it merit another publication?

Speific comments:

Lines 31-32ï¼☐ Recent reports such as IPCC special reports on 1.5 degree warming treat CDR and SRM separately and no longer lump them together as geoengineering.

Lines 51-52: SAI does not necessarily reduce precipitation. It only reduces global mean precipitation if SAI is used to compensate GHG-induced global mean warming.

Lines 58-59: Possible ozone depletion due to SAI is not a result of SAI induced cooling.

Line 66: It is important to note that volcanic eruptions are just imperfect analog of SAI.

Lines 70-76: It is not clear what is the novelty of this study. The authors state that no previous studies have analyzed effect of SAI on surface air temperature over China. However, this is a very weak justification of the novelty of this study. What new scientific insight we can obtain from this study? In other words, if someone writes another paper, stating that no previous study has focused on surface air temperature response over another region/country, can it be justified for publication?

Lines 108-113: Everything stated in this paragraph is self-evident.

Lines 114 Is it necessary to repeat the method and equations of Kashimura et al. (2017) in detail? What is new here?

Line 222ï¼□ Why surface cooling increases upward LW radiation?