

## ***Interactive comment on “Trade-offs of Solar Geoengineering and Mitigation under Climate Targets” by Mohammad M. Khabbazan et al.***

### **Anonymous Referee #2**

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Review on the manuscript entitled "Trade-offs of Solar Geoengineering and Mitigation under Climate Targets" by Mohammad M. Khabbazan, Marius Stankoweit, Elnaz Roshan, Hauke Schmidt, and Hermann Held submitted to Earth System Dynamics

The authors have discussed a temperature target-based approach for mitigating the impact of global warming on future climate using solar radiation management. The authors have introduced a concept for an integrated Solar Radiation Management (SRM) analysis and mitigation in-line with the 2°C temperature target. Therefore the paper is novel but limited by the approach adopted for the study. Authors have considered temperature and precipitation simultaneously in their approach for mitigation of the impact of CO<sub>2</sub> rise. However, it is not specified explicitly in the manuscript why temperature and precipitation have been selected for the analysis. However, after reading cited

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references, for example, Edenhofer et al., 2005; Gorgi and Bi (2005), readers may infer about it. Therefore I feel the paper needs to be rewritten for better clarity and readability.

I think the manuscript needs a major revision before its publication.

My specific comments are mentioned below.

1. Some of the earlier studies (Bala et al. 2008 and so on thereafter) indicated that alteration in solar forcing might offset temperature changes or hydrological changes from greenhouse warming but could not cancel both at once. However, the authors have designed an integrated analysis of SRM for mitigation of global warming impact in-line with the '2°C temperature target by constraining the regional precipitation changes. In line with this, the authors should provide the appropriate justification for designing these experiments to make the manuscript's physical basis more robust.

2. The manuscript's title indicates the manuscript will discuss the trade-off between solar geoengineering and mitigation under climate targets. However, the trade-off/optimization aspect has not been discussed clearly, even though it can be inferred from the discussions presented.

3. Page 3 Lines 7-12 "Here we ask: 'How much regional precipitation change, as an example of a climatic change other than temperature, would someone, who has already accepted up to 2°C of global warming, accept?' If we were able to confine regional climate change to the intervals of climate variables that would be spanned by ramping the global mean temperature anomaly (as against its pre-industrial value) up from zero to 2°C, we could augment the 2°C target by this exact set of intervals as the more fundamental target." needs simplification and clarity.

4. Page 4 lines 3-5: "For the scaling coefficients, we diagnose annual mean regional precipitation changes from linear pattern scaling (Ricke et al. (2010)) which are driven as a linear superposition (Ban-Weiss and Caldeira (2010)) of greenhouse-gas-induced

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and SRM-induced changes in global mean temperature." The authors need to provide a detailed explanation of the procedure followed for obtaining scaling coefficients.

5. The authors have used the outputs of nine atmosphere-ocean general circulation models (AOGCMs). However, details have not been produced in the manuscript. These models' configurations (physics, horizontal and vertical resolution, etc.) are different. It is not clear that how authors have normalized the impact of configuration changes? If they have considered regional averages only (over Giorgi regions), CSR<sub>M</sub> and CCo<sub>2</sub> should be measured in [%/K/km<sup>2</sup>]. It is not clear how the regional average will represent the regional change. The methodology adopted in this manuscript is quite confusing or needs a better explanation. A small change in a large area will have reasonably more impact than a large change in relatively smaller areas. Will conclusions presented in this study will differ if authors consider the regional averages by normalizing areal coverage of the regions specified in the manuscript.

6. Page 5 Lines 24-27: "Figure 3 shows normalized precipitation change for the 26 Giorgi regions for a): no-policy case (business as usual scenario 25 ('BAU') where neither SRM nor mitigation is applied); b): 2°C target activated and unlimited admissible SRM level ('REF'); c): precipitation changes when all regional constraints are binding and the extra admissible area is 5% of the standard deviation ('G0 5%'); d) similar to c) but with 10% of standard deviation ('G0 10%')". I feel a realistic analysis and experimental design are required.

7. The results presented in the manuscript rewritten with clarity and should be robust because many hypothetical statements have been mentioned in the result section. Further, the authors indicated that they had not considered the measures related to the reduction of CO<sub>2</sub> due to the impact of other policies and absorption by oceans. Therefore the results from this paper should be considered as the upper limit. However, it is not discussed in the abstract. Therefore, for the readers who will read only the abstract section, this manuscript's conclusions will be misleading.

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8. Authors should provide the links of datasets used for the present research work and comply with the journal's data policy.

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