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

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Note: For the sake of transparency and a point-by-point reply, each original comment by Anonymous Referee #2 is repeated in italic shape, and the Author Response follows it in upright shape.

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

We are very grateful to the reviewer for providing such a detailed comments on our

manuscript, which will certainly add to the quality of our paper.

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 2C 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 CO2 rise. However, it is not specified explicitly in the manuscript why temperature and precipitation have been selected for the analysis.

Thank you very much for this comment. We will strive at a clearer description of this point. Temperature (Asseng et al. (2011)) and precipitation (Portmann et al. (2010)) are highly relevant for agricultural productivity.

However, after reading cited 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 inline with the '2C 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.

This is an excellent point. We will add the mathematical framework in an explicit form. The key point of our ms is to extend the decision-analytic framework of global mean temperature-based decision-making to a situation were global mean temperature ceases being a good predictor of regional climate.

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

Thanks for your comment. We realize that the title in fact does not properly reflect our main point any more. We will change it accordingly – see also the above point.

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 2C 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 2C, we could augment the 2C target by this exact set of intervals as the more fundamental target." needs simplification and clarity.

Noted. We expect that adding the mathematical framework will deliver the necessary clarity. The above §represents the key innovation of our paper and apparently requires reformulation. It is about the extension of a temperature target to a situation in which it ceases being a good predictor for regional climate.

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

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

Surely, this will be added.

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), CSRM and CCo2 should be measured in [%/K/km2]. 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.

Thank you for this nice comment. We will present our data as well as AOGCMs in a properer and detailed way. Regarding the methodology to calculate CSRM and CCo2, we will explain precisely the procedure.

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): 2C 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.

As our analysis is in the tradition of explicating the consequences of ethical assump-

tions (see e.g., IPCC AR5 WGIII Ch6 (IPCC (2014))) and not of predictions, we are unsure what 'realistic analysis' does refer to. We would be grateful if the discussion phase could be utilized for a clarification on this particular issue or in general on our entire response.

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 CO2 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.

We will sharpen the abstract accordingly. We indeed want to study the inclusion of one important category of side-effect of SRM into an ethical framework coherent with a temperature target. This is our innovation and its effects can be studied best if applied to the combination SRM+mitigation.

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

We will comply with journal's data policy and present a proper reference to our data.

References

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