

## ***Interactive comment on “Climate extremes in multi-model simulations of stratospheric aerosol and marine cloud brightening climate engineering” by V. N. Aswathy et al.***

**V. N. Aswathy et al.**

johannes.quaas@uni-leipzig.de

Received and published: 31 May 2015

**General Comments** This study analyzes model simulations of two solar radiation management (SRM) schemes with regard to changes in mean and extreme temperature and precipitation and compares them to the RCP4.5 scenario simulations of current climate (i.e. year 2010). Results are based on 3 different global climate models and a comparison of 10-year time slices to illustrate the effect of the two different SRM schemes compared to future climate change as represented by RCP4.5 scenario for year 2060 and the termination of SRM. The introduction and motivation for this paper are reasonable and well elaborated. However, the discussion of results as well as

C13296

the conclusion is highly questionable regarding the methods used and illustrated in the paper. If more carefully assessed, some of the conclusions could potentially be very relevant (e.g., extremes are more affected by SRM termination than means, and the warming of the temperature's lower tail is not sufficiently offset by SRM), but the current paper is unable to robustly support these conclusions.

*We thank the reviewer for her or his thorough review of our paper and hope that we were able to address the concerns satisfactorily.*

**Specific comments**

The data basis for the analysis of extremes is insufficient to obtain robust results or support the strong conclusions drawn. 10 years are too short for this kind of analysis, even if simulations are in equilibrium and some models have more than one ensemble member. Information on the latter is not provided in the text, apart from a half sentence in section 3.1. Also I would argue that 3 models are not representative to conclude robust results just from at least two agreeing on the same sign of change (although this kind of measure is used in many GeoMIP papers).

*We have changed the analysis to now assess longer time periods. 30 years of daily data (10950 days) are now analysed; except for the termination-effect analysis, in which case we investigated 20 years (7300 days). Results and conclusions are changed including more years in corresponding sections.*

*Information on the number of ensembles included in the analysis is now provided in Section 2, Page 6, Line 161-166. We also substantially revised the analysis of the statistical significance and performed statistical tests to assess the robustness of the*

C13297

results and is discussed as a new Section 3.1.

The authors claim that T90 is representative for summer and T10 for winter season, which would reduce the effective sample size even further.

*Since we didn't perform a seasonal analysis in the first version of the manuscript, this was just an additional statement to the annual mean analysis. In order to address this comment comprehensively, and following also the suggestion from reviewer 1, we have now performed the analysis also on a seasonal basis.*

The signal-to-noise ratio in RCP4.5 is low, even in year 2060, to clearly distinguish the SRM effect from internal natural climate variability. The authors did not take this into account (e.g., could at least show variability between ensemble members of a single model). And again 10 years are fairly short to assess the contribution of natural variability.

*Variability between ensemble members are shown in supplementary material. As mentioned also above, we have now redone the analysis to investigate 30 year-periods, and performed thorough statistical significance tests.*

The discussion of results (and respective conclusions) are merely based on a qualitative assessment of maps and numbers in various tables. The authors did not make effort to apply any statistical test to support the robustness or significance of their results. For instance, whether the change in the extremes really follows the mean cannot simply be concluded by looking at global maps and numbers provided in the tables. (There are many papers on this issue!)

C13298

*The reviewer is right, and the revision in light of this statement helped sharpen the discussion. Statistical tests are now applied to assess the robustness of the results. In addition to the geographical distribution of annual and seasonal variations and also following suggestion from reviewer1, we have also created tables summarising the results with refined regional values of (tropic, mid-latitudes and high latitudes) are shown in Table 2 and 3*

There is no information on the baseline of the extremes indices indicated in the paper (i.e. values for RCP4.5 in 2010) to put the changes indicated in the tables into perspective to the overall magnitude of the respective index in the reference climate.

*Values of the baseline of the extremes are now provided as supplementary material.*

Results for consecutive dry days (CDD) are highly uncertain. Given the large disagreement between models, and the insufficient data basis, I don't think that such small changes as for instance 0.68 or 1.88 days/yr should even be discussed. See also my comment above. If the definition in Table 1 (i.e. of consecutive dry days per time period (=10years???) is correct then one would expect very large numbers in some regions (e.g., Sahel zone) and then a difference of less than 1 or even 10 would be insignificant. Furthermore, the global mean of change in CDD is pretty much meaningless given the very heterogeneous distribution of positive and negative changes as illustrated in figure 3.

*The unit for the consecutive dry days is days/year. This unit is now clarified where the CDD metric is introduced in Section 2.1, Page6, Line 194-195. For the revised work, as stated earlier, we have included more years and the robustness of the results are shown using statistical significance.*

C13299

What about a model comparison with observation? Maybe the differences found between SRM schemes and RCP4.5 are just as big as the model bias compared to the observations.

*We acknowledge that this study is a pure modelling study, and that ideally we should also investigate observations. However, a detailed model evaluation or observations-based study is beyond what we could possibly do in this study.*

Why are there no figures illustrating the termination effect if it is so important to draw conclusions from this analysis? The termination effect is not carefully assessed in this paper; see also my minor comments below.

*Figures for the termination effect are now included in Figure 11 and 12 in the revised manuscript. We also respond to the minor comments below.*

I highly criticize the conclusion drawn on the implications for the assessment of social costs of SRM (i.e. entire last paragraph of paper). This is pure speculation, not well-grounded in any literature reference, methodological approach or quantitative assessment. "There is no substantial indication for costly side effects" -How do you know? Given the substantial changes in spatial patterns of the extremes indices considered and associated uncertainties, I would not make such a strong statement without reconsidering the data basis and complementing the simple analysis of climate model data with some socio-economic data assessment and modeling approach.

*We have now changed the conclusion in the revised manuscript. New conclusion is included in Section 4, Page 19, Line 623-631. The conclusion reads as "Overall,*

C13300

*we conclude that the climate-change driven increases in the upper extremes of temperature and precipitation are simulated to be rather well mitigated by the two SRM climate engineering methods. However, we also find that the potential to mitigate effects of climate change by means of SRM differs around the globe and seasonally. Not very well dampened are in particular the increase in the mean temperatures in the Arctic, and especially the increase in the lower temperature percentile in the Arctic winter. At the same time, it is not easily possible to locally engineer the climate by SRM methods, as the analysis of the SALT scenario shows. These findings indicate additional conflicts of interest between regions of the world if it should come to discussions about an eventual implementation of SRM.*

More specific points:

Section 2.1, p. 32399, line 25: Aren't the extremes based on daily time series of minimum (2-m) and maximum temperature? It is mentioned further down (i.e. p.32400, line 5) but completely out of context as TX and TN are not further used in the paper.

*Daily mean temperature and precipitation are used to discuss the percentile based extreme indices. However for Summer days and Frost days, TX and TN are used. Definition of the extremes are now defined more properly in Section 2.1, Page 6, Line 186-189.*

Section 3.1, p.32401, line 12: "...models that simulated more than one..."??? Shouldn't that be "...models with more than one ensemble member..."?

*Sentence removed since we have changed the analysis.*

C13301

p.32402, line 6: "...SRM schemes are simulated to substantially narrow..."??? That sentence does not make any sense. Please rephrase.

*Rephrased the sentence in Section 3.2, Page 10, Line 312, into "Overall, both SRM schemes tend to substantially narrow the temperature distribution in the Arctic."*

p.32402, lines 13-16: This whole paragraph is unclear and does not make any sense as written. Please rephrase.

*Removed the paragraph based on the new analysis.*

Section 3.3.1, p.32406, lines 7-9: "The termination of the SRM ... compared to G3-SSCE method." Makes no sense as G3-SSCE is a SRM method, isn't it?

*It is a typing error, changed in the sentence in Section 3.6, Page 16, Line 517.*

p. 32406, lines 12-14: "The models simulate drying ...". The models don't even agree on the sign of change in fig. 3, I doubt they do for the termination period. See also my major point on CDD above. "North of Africa"??? Is that Europe?

*Sentence has been rephrased in Section 3.6, Page 16, Line 524. Fig 12 shows the change in precipitation for termination and the results are statistically significant.*

Section 4, p. 32406, line 26: "...mean global warming caused by the RCP4.5 scenario..." Global warming is not caused by RCP4.5!

C13302

*Sentence has be rephrased in Section 4, Page 16, Line 544.*

p. 32407, line 3: "10-year temporal distribution" of what?

*Sentence has be rephrased in Section 4, Page 16-17, Line 548-549.*

p. 32407, lines 4-6: "In the simulations investigated, ..." This sentence makes no sense as written. Please clarify and rephrase.

*Sentence has be rephrased in Section 4, Page 17, Line 552.*

---

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32393, 2014.

C13303