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

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The paper by Aswathy et al. is investigating climate extremes in climate engineering simulations. The paper is well written and structured and is another important contribution to the discussion on how climate engineering would affect climate extremes with focus on temperature and precipitation. The impact of two different climate engineering methods are compared using three different Earth system models.

We would like to thank the Reviewer for her work in helping us improve the manuscript.

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I have one major comment to the paper. One new finding of this study is that temperature and precipitation extremes are more or less equally affected by climate engineering as the mean values. I am somewhat concerned about this conclusion, since this has implications for the calculation of social costs of geoengineering (as stated in the text) and I am not convinced that this statement is true. Some more analysis or at least discussions would be helpful to support this conclusion. The study does only investigate annual changes and does not look into seasonal variations, in particular important for precipitation. Is this sufficient? Would an extension of the analysis to different seasons and somewhat refined regions (tropics, mid-latitudes, high latitudes) change the result?

We changed the conclusion of social costs of geoengineering. New conclusion (Section 4, Page 19, Line 623-631) is “Overall, 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.”

We have included seasonal analysis for both temperature and precipitation analysis and discussed in Section 3.5. A new table with refined regional values of (tropic, mid-latitudes and high latitudes) are shown in Table 2 and 3.

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Another caveat of the findings is that the models used here do not simulate the effect of volcanic aerosols on dynamics and chemistry, which may change the results.

Thank you for pointing out the caveat, and a new text is included in the text in the Section 2, Page 5, Line 139-145. New text added now reads: "In the SULF simulation, the aerosol effects on radiation is included in the models via their optical properties (Niemeier et al., 2013). This is achieved by prescribing aerosol optical depth (AOD) and effective radius, which were calculated in previous simulations with an aerosol microphysical model ECHAM5-HAM (Niemeier et al., 2011); (Niemeier and Timmreck, 2015). This approach allows an impact of the aerosol heating on the dynamic of the ESM, while the feedback process of the dynamic on the areosols was only included in the previous simulations with ECHAM5-HAM ."

Specific Comments:

Introduction: Line 7: There are many studies following Crutzen's paper, I suggest to add "e.g.," before the citation list.

"e.g.," added before the citation list in Section 1, Page 2, Line 36.

Section 2: Line 25: It seems that all the climate models used in this study prescribe AOD and effective radius for the G3 experiment. So, these models do not inject SO2 directly. Please clarify if any of these models simulate the impact of aerosols on stratospheric dynamics or chemistry, and if not, could this change the results?

We do not treat chemistry. However, dynamics may respond of course. As mentioned

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above a text is added in Section2, Page5, Line 139-145

Section 2.1 The climate extreme analysis may be misleading, since there is no separation between seasons. Please comment. Also, from Table 2, P10 and P1 is not shown for precipitation. As shown in Tilmes et al., 2013, P10 and P25 indicate changes in light precipitation and a reduction is an indication for droughts, while the increase of higher intensities, like P90 or P99, indicates increasing heavy precipitation and therefore flooding.

We now added, also in reponse to the major comment, a seasonal analysis for mean and extremes of temperature and precipitation in order to address this suggestion and discuss it in Section 3.5. We tried to also investigate P10 and P1. However, based on the results, we believe that the analysis of the consecutive dry days is a more reliable way to investigate the lower extremes of precipitation.

Page 32400, Line 5: Please clarify how maximum and minimum temperatures are defined, are these daily minimum/maximum temperatures or maximum and minimum temperatures of daily mean temperatures?

Daily minimum and maximum temperatures are used and the secentence is rephrased. Sentence now changed to "Data for daily maximum (TX) and daily minimum (TN) 5 temperature are directly provided from the model" in Section 2.1, Page 6, Line 189.

Section 3: Tables 2-4 only show multi-model mean values. Adding values from single models would be helpful to see how those vary. Also, a separation between Tropics, mid-latitudes (North and South) and global would be interesting, as well as the corresponding discussion in the text.

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Values for the individual ensemble members are provided as supplementary material. Area separated i.e., Tropics, mid-latitudes North and South and high latitudes North and South values are computed and shown as two separate tables Table 2 for temperature and Table 3 for precipitation and discussed in Section 3.2, Page 10, Line 320-331 and Page 12, Line 384-392.

Section 3.1: Page 32401, Line 15: Reference in brackets.

Changed the reference to brackets in Section 3.2, Page 9, Line 278.

Line 22: Instead or in addition to Figure 1, it would be helpful to show a PDF for example for the northern mid-latitudes over land, to easier identify the statement that there is “no shift in the tail of the temperature distribution”. It seems to me that there is more warming over northern Europe and Canada in looking at P10. Again, differentiating in seasons may show a stronger signal than the annual average.

Thank you for this suggestion. Yes with the seasonal analysis it is clear that warming occurs over the northern hemisphere midlatitudes during SALT experiment. Hence we subsequently remove the statement that there is no shift in the tail of the temperature distribution and the new sentence (Section 3.2, Page 9, Line 292) now changed to “In SALT, the pattern for the upper percentile temperature (T_{90}) values are similar to those for the mean values in the northern hemisphere”. As pointed out earlier, seasonal analysis of the percentiles are provided in Section 3.5

Line 32402, Line 4: Is this really only the case for northern high latitudes, or also

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mid-latitudes (30-60N)?

Thank you for the correction, to some extent it is also there in mid latitudes as well. Change is included in Section 3.2, Page 10, Line 308 as “For the Northern hemisphere high latitudes and continental regions in the Northern mid-latitudes as well as sea-ice regions in the Southern hemisphere mid-latitudes, a much stronger increase in the lower percentile of the temperature distribution (T10) is simulated.”.

Line 13ff: This statement needs the addition, that only annual averages were considered and seasonal changes may be much larger.

Suggestion included and a new sentence added to the paragraph in Section 3.2, Page 10, Line 317, “This aspect of the SRM is more detailed in the Section:season”.

Line 18: Reference in brackets.

Changed the reference to brackets in Section 3.2, Page 11, Line 344.

Figures 6-8: these are too small to read. Maybe 4 rows and two column would work better?

In the revised manuscript we have combined the figures into one single figure (Figure 3) for easy comparison.

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