Dear editor and referees,

First of all, we greatly appreciate your thoughtful and valuable comments and suggestions, which have been a great help in improving our manuscript. After careful consideration and revision, we think all the comments have been appropriately addressed, and the revised manuscript could meet the quality standards of ACP. Below is a point-by-point response to all referee comments, generally including comments from referees, our responses, and changes in the manuscript. In addition, a marked-up manuscript version showing the changes made (using track changes in Word) is provided following the response.

Once again, we are very grateful for your kind help, and looking forward to your further comments and positive decision on our manuscript.

Thank you for your consideration of publication.

With warm regards,

Qin`geng Wang, Prof. (State Key Laboratory of Pollution Control and Resources Reuse, School of Environment, Nanjing University, Nanjing, 210023, China)

Jim M. Haywood, Prof. (Department of Mathematics, Faculty of Environment, Science and the Economy, University of Exeter, Exeter EX4 4QE, UK)

Response to Referee #1

General comment:

This paper examines the impact of changes in different measures of precipitation over China under a high emissions scenario and with a small ensemble of climate intervention simulations at the end of the century. One climate model (UKESM1) is used for comparison with two different realisations of stratospheric aerosol injection - G6solar, using a constant solar dimming, and G6sulfur, with gradually increasing injections of sulfur into the stratosphere. The paper is well organised and generally clearly and well written. My main criticism is that the discussion does not evaluate the results with respect to other research that was carried out from other experiments such as GLENS. Suggestions for other relevant literature is at the end together with references made in the comments below.

Major comments:

Consider reducing the content of Section 2.2. A lot of this is repetition from the literature that is cited and doesn't necessarily need to be included in this article.

Response:

Thank you very much for your suggestion. We have realized the repetitions from the literature are not necessary, and revised the section 2.2, as in lines 139-166.

To avoid confusion, consider changing any reference to "present-day" to the control period and sticking with this consistently, rather than switching between 'historical', 'baseline' and 'present-day'. The WMO has adopted 1990-2020 as the "current climate period", which suggests that the period used in this article is historical.

Response:

Thank you. We have changed "present-day" to "control period" throughout the manuscript.

Is there a benefit in the data validation against APHRODITE? The article does not include an assessment of extreme precipitation metrics with respect to observations, nor do the results or conclusions refer back to the observations. I think you could drop this, and instead refer to other assessments of the validity of extreme precipitation in climate models such as (Sillmann et al., 2013; Donat et al., 2020; Tebaldi et al., 2021)

Response:

Thank you for this important comment. Indeed, this study has got a great benefit in the data validation against APHRODITE. The observations were used to validate the direct results of the model (i.e., amount of precipitation), which we think is the most fundamental for this study. To further compare the results between simulations and observations, particularly focusing on extreme precipitation values, in the revised manuscript (line 237), a scatter plot between the ensemble simulations and observations is provided as a new panel in Figure 2 (Fig.2d). In addition, the scatter plots between the three model results and the observations are provided in Figure S1. In the scatter plots, the daily observed precipitations were classified into several intervals: P10 (the smallest 10%), P10-50, P50-90, P90-95, and P95 (the largest 5%). The scatter plots (also shown below) indicate a close relationship between the observations and the simulations. However, the simulations are generally higher than observations, possibly because of the different resolution of the data. Since our study has been mainly focused on the relative changes between the future results and that of control period for different scenarios, the systematic bias would not affect the conclusions significantly. The above explanation is also added in the revised manuscript (lines 230-235).

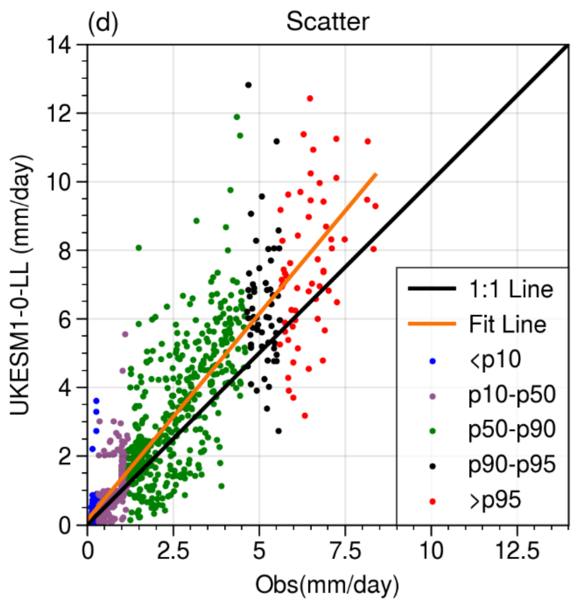

Figure 2(d) Scatter plot between the observations and simulations at different level of precipitation. The observations were classified into several level (intervals): P10 (the smallest 10%), P10-50, P50-90, P90-95, and P95 (the largest 5%).

In order to indicate the bias as a percent, relative changes (compared to the observations) for different intervals have been calculated, and the results are listed in a new Table2 added in the revised manuscript (also shown below). As expected, the relative changes are very large at small values (below the 10th percentile), both for the ensemble mean and the model members. For the results at the 10-50th and 50-90th percentiles, the relative changes are around 30%. When larger than the 95th percentile, the relative changes are relatively small, near 15%. The differences among ensemble members are not significant, which suggests the uncertainty in the ensembled results is reasonable and acceptable. Please see lines 248-255 for the table and discussions.

Table 2: Relative changes of the model results (compared to the observations)

intervals	Ensemble mean	r1i1p1f2	r4 i1p1f2	r8i1p1f2
<P10	89.81%	93.95%	89.44%	86.04%
P10-50	30.05%	30.38%	31.85%	27.13%
P50-90	30.50%	28.95%	31.36%	31.16%
P90-95	24.03%	22.79%	24.85%	24.44%
>P95	15.76%	15.09%	16.27%	15.92%

Given that this research uses one climate model, and three ensemble members for each scenario, it isn't really appropriate to state that G6sulfur/G6solar abates or ameliorates climate change as depicted by SSP5-8.5. There aren't sufficient model members to remove model uncertainty, and without observations we do not know which model realisation (if any) adequately simulates the effects of SAI. All you can state with confidence is that using this methodology and data, the SAI experiments produce results consistent with a lower emissions target. I would prefer to see all of the statements on improvements with respect to climate change removed, or at least reduce the emphasis of the statements.

Response:

We agree with you, and thank you very much for the suggestion that makes our paper more rigorous and scientific. Really, considering many possible uncertainties in the climate models, as well as in the research scenarios, we cannot assert that G6sulfur/G6solar abates or ameliorates climate change as depicted by SSP5-8.5. Accordingly, relevant statements in the manuscript have been removed.

Minor comments:

L28 change impacts to efficacy

Response:

Changed (Line 32).

L30 rephrase this sentence as noted above.

Response:

The sentence "While the results from both G6sulfur and G6solar show encouraging abatement of many of the impacts on detrimental extreme events that are evident in SSP5-8.5 there are some exceptions." has been change to "While the G6sulfur and G6solar show encouraging potential abatement of the impacts from detrimental extreme events which are similar with the lower emissions target of SSP2-4.5, there are some exceptions." Please see lines 33-35.

L31 remove trends

Response:

Removed (line 35).

L42 is the higher risk of flooding associated with increased extreme precipitation? Remove the time periods studied, this is implicit.

Response:

Yes, the higher risk of flooding is indeed associated with increased extreme precipitation. This is indicated in the results by Ying et al. (2014), where the flood risk is understood as an extreme climate index. Indeed, the time periods are implicit, and has been removed (line 47).

L44-60 is this level of detail on historic events warranted? You do not examine the changes in jets or other sources of extremes.

Response:

The details on historic events are mostly quoted from published papers or media reports. To be honest, we could not warrant their reliability since we have not conducted deep investigation in this regard. Now, according to the comments by another referee (RC2), we have realized that the detailed description on the historic events is not directly relevant to the study here, and not necessary. Therefore, relevant contents have been deleted or shortened in our revised manuscript (lines 50-66).

L44 Why was summer of 2020 anomalous - it seems in keeping with the other extreme events you reported.

Response:

The using of the word "anomalous" was not appropriate. Besides 2020, flooding events also frequently happened in other years. In the revised manuscript, this has been corrected (line 49). In addition, as mentioned above, the detailed description on the historic events has been shortened.

L61 change appears to has, and cite relevant literature such as (Donat et al., 2016; Pendergrass and Knutti, 2018), which also discuss changes in the hydrological cycle.

Response:

The sentence has been changed as: "On a global scale, climate change has been influencing hydroclimatic conditions (Donat et al., 2016; Pendergrass and Knutti, 2018)." Please see lines 66-67.

L64 Should causes go before faster?

Response:

Relevant contents have been deleted as mentioned above.

L66 Update this to the more nuanced and recent research that shows extreme precipitation generally goes up everywhere (Pendergrass and Knutti, 2018)

Response:

It has been updated according to Pendergrass and Knutti (2018). Please see lines 71-72.

L79 SAI does not mitigate anthropogenic climate warming, it may mitigate some of the impacts.

Response:

You are right. The sentence has been revised as: “To some extent, SAI partially counteract climate warming by injecting reflective particles, or their gaseous precursors, into the stratosphere (Zarnetske et al., 2021).” Please see lines 86-87.

L79-89 This paragraph needs rephrasing to explain that SAI is premised on reproducing the effects associated with volcanic eruptions. However, you do not need to list the different volcanic eruptions themselves - just point to a large body of literature that supports these effects.

Response:

Thank you for this comment which has helped make our manuscript more concise and less verbose. The paragraph has been changed as: “To some extent, SAI can partially counteract climate warming by injecting reflective particles, or their gaseous precursors, into the stratosphere (Zarnetske et al., 2021). In addition to reducing the temperature, SAI also influences tropospheric and stratospheric ozone, terrestrial ecosystem, terrestrial carbon, and hydrological cycle by changing the physical climate system and atmospheric chemistry. Numerous studies support these effects associated with volcanic eruptions and their simulation through SAI techniques (e.g. Imai et al., 2020; Mclandress et al., 2011; Jones et al., 2018, 2020; Liang and Haywood., 2023; Lee et al., 2021; Plazzotta et al., 2019; Visioni et al., 2022).” Please see lines 86-91 of our revised manuscript.

L99 include other recent research that explored changes in temperature and precipitation in other SAI experiments not just the GeoMIP archive (e.g. Tye et al., 2022; Simpson et al., 2019).

L100 What about (Tew et al., 2023)?

Response:

Thank you for the suggestion. We have incorporated other recent research, including studies exploring changes in temperature and precipitation in various SAI experiments beyond the GeoMIP archive, such as those by Tye et al. (2022), Simpson et al. (2019), and Tew et al. (2023). Please see lines 113, 116, 119.

L105 remove maximise the signal-to-noise in the simulations as

L114 remove according

Response:

Removed

L126 See comment above, but at the very least remove The GeopMIP G6sulfur simulations that reduce global mean temperatures from the SSP5-8.5 scenario to the SSP2-4.5 are described in detail elsewhere.

Response:

According to your above suggestions, the manuscript has been revised. Please see lines 147-152.

L143-145 remove this last sentence.

Response:

Removed.

L154 I believe that the extreme indices were defined by the WCRP not IPCC.

Response:

Thank you for pointing out the mistake, and we have corrected it in the manuscript (line 179).

Table 1: the authors should be Frich and Klein Tank. Also refer to (Sillmann et al., 2013; Zhang et al., 2011) for the correct definitions

Response:

The mistake has been corrected (line 184).

How did you calculate the 95th percentile? Did you bootstrap the individual years to avoid data inhomogeneities (Zhang et al., 2005)

Response:

For each grid, the 95th percentile was calculated based on 30 years (1981-2010) of daily precipitation data. We calculated the 95th percentile directly without using bootstrapping methods, as recommended for calculating temperature indices (https://www.climdex.org/learn/indices/#index-TX90p). Relevant explanation has been added in the manuscript (line 285-286).

L161 This may not be relevant if you remove the Aphrodite data as suggested above. However, I am concerned about regridding the larger data to the smaller grid. No additional information is gained in this respect (just several grid boxes with the same values) and may show errors and biases that are not true. Instead it would be more robust to regrid the observations to match that of UKESM. See https://climatedataguide.ucar.edu/climate-tools/regridding-overview for more information.

Response:

Thank you for your valuable suggestion. The observations were re-gridded to match that of the UKESM (line 187-188).

L165 remove instead of the more commonly used Student’s t=test. Wilcoxon Rank Sum Test work as

Response:

The text was removed.

L167 change “with p-value <0.05 suggesting” to “with a 5% confidence level of”

Response:

The expression was changed (line 193).

L169-182 How did you establish the CDFs? Did you fit distributions, or are these empirical CDFs from the data? Did you examine the uncertainty in the CDFs, and were they fitted for each model member (correct) or the model mean (as the figures suggest)? I am also wary about CDFs for very small sample sizes - i.e. 30 values of the annual maximum rainfall.

Response:

Thank you for pointing out this problem that we didn't explain it clearly about how we established the CDFs. In fact, for establishing the CDFs, firstly, for an extreme precipitation index at each grid point, the yearly mean of the ensemble model members was calculated. Then, the annual extreme precipitation indices for each grid point was obtained by averaging over yearly means during the 30 years. Finally, the cumulative probability distribution of the extreme precipitation index over all grid points was statistically analysed for each of the seven regions, as well as the whole China. Therefore, we have a large number of samples for calculating the CDFs, instead of 30 values.

We computed the empirical cumulative distribution functions (ECDFs) of our data using histograms. To achieve a smoother representation of the distribution, we applied a Gaussian smoothing technique. By doing so, we were able to obtain smoothed representations of the empirical distributions, which provided clearer insights into the underlying patterns of the data.

As far as the uncertainty in the CDFs is concerned, in the original manuscript, we just placed emphasis on the uncertainty in the direct results of the UKESM, which we think is the most fundamental for our study. The model results (amount of precipitation) were validated with the observations (APHRODITE), and only means of the ensemble models were considered. In our revised manuscript, more comparisons between the model results and the observations have been provided. Please see lines 230-255.

L185-194 put this into the previous section.

Response:

Thank you for your suggestion. We'd like to clarify that since we have already employed the Wilcoxon test for significance testing, the field significance calculations mentioned here are redundant, and was not used in this study. Therefore, we have removed this portion in the revised manuscript. We are sorry for this confusion.

L193 Include this statement in the figure caption instead of the text - and check which way you have represented significance, this is opposite from the figure.

Response:

As said above, the description of the method has been removed.

L211 Comment on the increase in drought in west and Taiwan under G6sulfur.

Response:

Thank you for pointing out these detailed signals. We thank the reviewers for pointing out these detailed signals. These signals of increase in drought are possibly linked to the intensification and northward shift of the Western Pacific Subtropical High; however, due to the limited reliability of small-scale signals in the non-storm-resolving GCMs like UKESM1, we decided not to discuss these small-scale signals in the revised manuscript.

L215 Change this statement to something like projected changes are similar to those of SSP2-45, meaning that the SAI simulations are approximately successful.

Response:

The statement has been changed to "G6sulfur (Fig. 3c) shows projected changes are similar to those of SSP2-4.5 (Fig. 3b), indicating that the SAI simulations are approximately successful." Please see lines 262-263.

L246 There are no absolute values>100mm or no increases from the control period >100mm?

Response:

Thank you for pointing out this problem. We have clarified it as "In the other three G6 scenarios, the increase in RX5day is considerably smaller than that under SSP5-8.5, with none exceeding 100 mm compared to the control period (Fig.S3a-d)." Please see lines 294-295.

L250 remove (p-value <0.05) and every other instance - this has already been stated.

Removed.

L251 remove which is generally. Is there really only one research paper on increases in extreme precipitation in this region?

The words are removed. Some other research papers (e.g., Qin and Xie, 2016; Peng et al., 2018) are included in revised manuscript (line 300).

L257 Remove this sentence.

Removed.

L265 should this be depicting?

We have corrected 'dicipting' to 'depicting. ' Line 314.

L266 remove sentence “The comparisons confirm….”

The sentence has been removed.

L278 remove “This suggests that G6sulfur….” This is the results section, so discussions aren’t appropriate.

Removed.

L280 Stick to reporting the differences between the simulations in this section, then interpret (with appropriate caveats) in the discussion sections and remove the sentences on the implications or efficacy of SSPs vs G6.

Response:

Thank you for the thoughtful suggestion. To stick to reporting the differences, the sentence has been revised as “In comparison to SSP2-4.5 (Fig.S4b), G6sulfur exhibits an increase in RX5day, primarily in the region between 100°E and 120°E. For 'G6sulfur-G6solar'(Fig.S4c), positive values of RX5day are more pronounced in certain areas between 100°E and 120°E, especially in the low latitude zone between 20°N and 30°N.” Please see lines 327-331. In addition, relevant interpretations (with appropriate caveats) have been condensed in the discussion section (Lines 633-637).

L291 Remove ameliorating

Response:

"Ameliorating" has been removed.

Table 2: I am not sure that this adds to the interpretation of the results and could be removed. If retained, re-phrase as difference between G6 and SSP or something similar.

Response:

We think that Table 2 (now is Table 3) provides a useful summary of the results. If you look at Table 3, you get the general idea that SRM does 'good' or 'bad' things on the extreme indices threshold. That is a very useful take-home message. So, we would like to retain the table. Some descriptions and discussions about the results have been revised (Lines 343-349, 441-443, 509-510).

L311 Why is this interesting? Elaborate please.

Response:

We find this interesting because it highlights a unique pattern in the data. Despite observing mitigation effects in other regions, we notice that while G6solar mitigate the overall RX1day, it exacerbates the maximum RX1day values beyond SSP5-8.5. This suggests a nuanced relationship between the G6solar and their impact on RX1day in the SC region, warranting further investigation.

L325 It might be more meaningful to look at the relative changes (e.g. in percentage terms) rather than absolute values. With regard to the "arbitrary" regions, why are they somewhat arbitrary? Surely they relate to some geographical or political definition, the point to make is that they may not correspond with climatological regions. Note that smaller regions would just emphasise noise in the results.

Response:

We agree that relative changes such as in percentage terms would be more meaningful in some cases. However, in the case of our study, a great deal of the results are small values, and consequently, the relative changes (percentages) could be very large even for minor absolute changes. For this reason, we think the absolute changes can be more appropriate here.

We agree that the conclusions would be different if based on different criteria for dividing the regions. Unfortunately, in this regard, there is no standard criteria for dividing the regions. In this study, the division of regions is a conventional way and has been widely adopted in many statistics reports and relevant studies (e.g., Luo et al., 2017; Fan et al., 2020; Yang and Shao, 2021; Liang et al. 2023). The sentence “It should be stressed here that the regions that are chosen for aggregation are somewhat arbitrary and the results could well change should smaller sub-regions be chosen for analysis.” is not expressed accurately, which has been removed.

L329/30 remove this sentence.

Response:

The sentence has been removed.

Figure 6 as noted above the uncertainty across model members should be included in these curves. Please also check the colour scheme for colour blind appropriateness, and use the same x-axis for each variable for all regions (i.e. one x-axis for Rx1day, another for R95p). This also applies to Figure 9 and 12.

Response:

Yes, it would be more informative if the uncertainty across model members be included in the CDF curves in Figure 6. We do have the results of the three model members, and we tried to added results of each model along with the curves to indicate the uncertainty. However, since the curves are closely overlapped, we couldn’t find a way to make the figure clear. The lines or colour blocks could be blended and overlapped together, and make the figure difficult to distinguish. For this reason, we gave up the idea of directly including the results of model members in the figures. Instead, we have added some statistical metrics of the model members (as a new table) in our revised manuscript (lines 230-255). In this way, we think, at least to some extent, the uncertainty across model members can be indicated.

The colour scheme has been checked for colour blind appropriateness.

For the suggestion on using the same x-axis for each variable for all regions, there is a difficulty. Because the range of index changes varies big across different regions, when plotting them on a large-scale x-axis, the curves with small range (or values) could be very close or even overlapped with each other, and difficult to be distinguished. For this reason, x-axis is not the same for all the regions. We use the same x-axis in Figure 6 and appropriately adjusted the x-axis in Figure 9 and Figure12, according to different range of index changes.

L350 Decreases in CWD do not necessarily equate to reductions in precipitation intensity. You can only make this interpretation if there is a reduction in the total number of wet days AND an increase or no change in the annual total.

Response:

Thank you for this thoughtful comment. The sentence has been removed.

L356 R50mm is not one of the formal ETCCDI indices, it is user defined.

Response:

Thank you for your reminding. We have revised the sentence as follows:

"The R50mm index is derived from the Rnnmm index, as suggested by ETCCDI. The Rnnmm index represents the count of precipitation above a user-chosen threshold. In this case, the threshold is set to 50 mm, as recommended by the China Meteorological Administration (CMA)." See lines 404-405.

L371 remove effectively ameliorates the

Response:

It has been removed.

L373-375 This is discursive and needs more references to support it (and moving to the discussion section). There is likely a combination at play including changes in the location of the jet streams and ITCZ, as well as interactions with topography and changes in maritime temperature gradients.

Response:

Thank you for your suggestion. More references in this regard have been included in our revised manuscript (lines 423-426). Because the discussion section provides a summary of the findings, we believe it is more appropriate to include it there.

L380/1 Remove this sentence.

Response:

The sentence has been removed.

L387 CWD=200 days is right at the far end of the tail, I don't think it's appropriate to make this statement without any error estimates or uncertainty information. Further, the duration estimates of CWD and CDD add up to longer than a year - this is particularly obvious in comparison with Figure 12. Please check.

Response:

Thank you for the comments. As mentioned above, some examination on the uncertainty in CDFs has been added by comparing the model results with that from the APHRODITE (for the historical period) and comparing the results among different models (for the future).

The phenomenon of the combined CDD and CWD exceeding 365 days in the same region arises because the high values of CWD and CDD may occur at different grid points, resulting in the possibility of the total exceeding 365 days.

L402 Should this refer to Figure 9? This statement would be more meaningful with uncertainty envelopes to clearly demonstrate whether there are or are not differences between each model.

Response:

The statement refers to Figure 8. Also, as mentioned previously, since the curves are closely overlapped, we couldn't find an appropriate way to include the results of model members in the figures. Instead, we have added some statistical metrics of the model members (as a new table2, lines 246-247) in our revised manuscript. Meanwhile, relative changes (compared to the observations) for different intervals have been calculated, as listed in table2(lines 248-255). In this way, we think, at least to some extent, the uncertainty across model members can be indicated.

L413 Remove, and would have the ….

Removed.

L458 It is not noteworthy that these yield similar results - that's the objective of the cooling. Remove this sentence.

The sentence has been removed.

L468 Remove last sentence of paragraph.

Response:

The last sentence of the paragraph has been removed.

L475-481 There are other aspects related to drought risk - not least evaporation - that don't show up in the dry day count, such a strong statement about changes in drought risk aren't appropriate. See (Cheng et al., 2019; Dagon and Schrag, 2019, 2016) for more results related to climate intervention.

Response:

The sentence has been changed to "This reflects a potential decrease in drought risk in northwest regions and an increase in extreme drought events in low-latitude southeast coastal areas in the future according to four G6 simulations. Changes in precipitation affect soil moisture, thereby influencing evapotranspiration (ET) and ultimately precipitation patterns. Assessing whether changes in DD and CDD affect drought risk also requires consideration of variations in ET and soil moisture (Cheng et al., 2019; Dagon and Schrag, 2016). Furthermore, solar radiation management (SRM) increases drought risk compared to SSP5-8.5 and SSP2-4.5 scenarios in northern regions (NEC, NC, and NWC)." Please see lines 535-540 of the revised manuscript.

G6solar and G6sulfur are different ways of simulating a possible climate, we have no way of knowing whether one or the other is more valid without observations, and so can't be described as outperforming each other. See (Bednarz et al., 2022; Visioni et al., 2021) for more discussion on this.

Response:

We agree that we cannot tell which one is better between G6solar and G6sulfur, and removed the related sentences (lines 329, 520-522, 545, and 582). What we did in our study is comparing both the results of G6sulfur and G6solar in extreme precipitation events against the lower emission target (SSP2-4.5) or that in control period. We have added more discussion on this according to the references you suggested. Please see lines 609-610.

Section 4 I suggest condensing the bullet points to short concluding statements, which can then be followed by the explanations. Given that the focus of this article is on the climate intervention, it would also make more sense to emphasise those results rather than the future projections that have been published elsewhere. This is also the point to discuss how valid the results are with respect to other research - including experiments outside the GeoMIP project.

Response:

According to your thoughtful and valuable suggestions, we have revised our manuscript. The concluding statements have been shortened, and more emphasises have been placed on results on the climate intervention, instead of published future projections. For relevant revisions, please see lines 547-559 in the revised manuscript.

Response to Referee #2

General comments:

This study uses the UK Earth System Model (UKESM1) simulation results to examine the effect of solar radiation modification (SRM) geoengineering on precipitation extremes in China. As part of the GeoMIP project, UKESM1 was used to conduct two sets of SRM simulations: stratospheric aerosol injection (G6sulfur) and solar constant reduction (G6solar). Both G6sulfur and G6solar simulations are designed in such a way that global mean surface temperature under the scenario of SSP5-8.5 was brought down to the level under SSP2-4.5. Using a set of precipitation extreme indices, the authors investigated the effect of G6sulfur and G6solar on precipitation extremes for different regions of China. The authors found that compared to SSP5-8.5, both G6sulfur and G6solar ameliorate precipitation extremes over different parts of China, but increase drought risks in some northern part of China. The authors also compared the similarities and differences between precipitation extreme response to G6sulfur and G6solar for different regions of China. The analysis of this paper itself is largely sound, but I do not recommend its publication in ACP in the present form for the following reasons:

I see little science in this study. I have to say that I have not carefully examined the Results part, which is just the description of figures with little scientific insight. What the authors did is just to compare simulated precipitation extremes over different regions of China under SS5-8.5, SSP2-4.5, G6solar, and G6sulfur. Regional climate extremes are strongly dependent on the SRM scenarios (location, timing, and intensity of SAI and solar reduction). Also, regional climate extremes are strongly dependent on climate models. If one uses another climate model and/or another SAI strategy, most results presented in this paper might be different. At least, the authors should use multiple model results from GeoMIP instead of just one model. Also, the authors should try to investigate some science underlying the presented precipitation extreme comparisons. For example, why the difference between G6sulfur and G6solar? In the present form, this paper just presents simulation results from a specific model with little interpretations. At least for me, I see little science here.

Response:

We greatly appreciate your time and effort in reviewing our work and providing detailed and insightful comments on the manuscript, which are very helpful for improving the quality and clarity of our paper.

Indeed, in our present study, we mainly dedicated to compare the simulated precipitation extremes over different regions of China under different scenarios (SS5-8.5, SSP2-4.5, G6solar, and G6sulfur), with special focus on potential impacts of the SAI on precipitation extremes. We admit that there lack of sufficient interpretations on the results, particularly in terms of mechanism linking the response to the impacts. This is partially because of our limited knowledge in relevant fields. However, considering relatively scarce research and limited knowledge on the impact of SAI over East Asia, we think our findings are useful for deepening some understanding of the potential mitigation strategies of climate change. Thanks to your valuable comments, more interpretations on the effects of G6sulfur (e.g., in lines 423-426) and relevant mechanistic analyses (e.g., in lines 466-468, 537-538) have been added in our revised manuscript. We hope this could make up the deficiency to some extent.

We understand your concern regarding the use of only one climate model. We agree that the one model may not capture the diversity among different models, and employing multiple models for analysis could enhance the robustness of the findings and provide a more comprehensive perspective. Given this limitation in our study, we have realized that the paper title was rather over-reached in terms of what was presented in the paper and has been changed to "Projected future changes in extreme precipitation over China under stratospheric aerosol intervention in the UKESM1 climate model".

We chose the UKESM1 model due to its extensive validation in prior studies and its reputation as a reliable tool for simulating climate dynamics. UKESM1, as described by Sellar et al. (2019), represents a significant advancement over its predecessor, HadGEM2-ES, with enhanced complexity in its components and internal coupling. The model performs admirably, maintaining a stable pre-industrial state and demonstrating strong agreement with observations across various contexts (Sellar et al., 2019). Furthermore, we conducted a validation of precipitation against APHRODITE data, which demonstrated that the model has a very credible performance. Previous studies also utilized the standalone UKESM1 model to evaluate the physical and biogeochemical state of the global ocean component (Yool et al., 2020), to assess the impact of both SAI and MCB on standard meteorological variables (Haywood et al., 2023), and to research other meteorological related (Haywood et al., 2022; Wells et al., 2023; Jones et al., 2020; Jones et al., 2022; Visioni et al., 2021).

In our study, while some biases were observed, UKESM1 reasonably captures precipitation patterns, particularly in eastern China, when compared with APHRODITE data from 1981-2010. Additionally, as noted in previous research (Liang and Haywood, 2023), UKESM1 is currently the sole model capable of providing outputs of pressure-level winds and specific humidity data every 6 hours, satisfying the requirements of the ARDT (Atmospheric Radiation Detection and Tracking) method. Furthermore, Tian et al.(2021) validated the UKESM1-0-LL simulation, demonstrating robust agreement between simulated and observed precipitation in China from 1961 to 2014, surpassing that of the CMIP6 multi-model ensemble (MME). Although acknowledging that a single model may not fully encompass the complexity of all climate variations, we believe that UKESM1 offers a valuable initial assessment of the potential impacts of SRM strategies in different regions of China.

Certainly, future research could benefit from incorporating a broader range of models to validate our findings and further explore inter-model differences. This would contribute to a more comprehensive understanding of the effects of SRM on precipitation extremes and yield more robust conclusions.

About the difference between G6sulfur and G6solar, G6solar serves as a parallel experiment to G6sulfur, aiming to compare the effects of solar reduction with those of stratospheric aerosols. G6solar adopts the same setup as G6sulfur, but geoengineering is achieved through solar irradiance reduction. Specifically, the inter-model differences in the spatial distribution of forcing are expected to be smaller in G6solar than in G6sulfur, offering valuable insights into the effects of uncertainties in stratospheric sulfate aerosol transport (Kravitz et al., 2015). These have been explained in the revised manuscript (lines 152-159).

Additionally, we would like to note that, when assessing impacts, it is common to focus on the most relevant metrics that are influenced. For example, the recent paper by Mari Tye (https://esd.copernicus.org/articles/13/1233/2022/):

Tye, M. R., Dagon, K., Molina, M. J., Richter, J. H., Visioni, D., Kravitz, B., and Tilmes, S.: Indices of extremes: geographic patterns of change in extremes and associated vegetation impacts under climate intervention, Earth Syst. Dynam., 13, 1233–1257, https://doi.org/10.5194/esd-13-1233-2022, 2022.

In Tye et al. (2022), a single model (Community Earth System Model, CESM1) is used for assessing extremes in temperature, precipitation and vegetation. While it should be more meaningful to delve into the causal mechanisms linking the response to the impacts, simply documenting impacts in extremes (as in Tye et al. (2022)) also appears worthy, at least to some. Other examples of published work that use a single model and focus on the impacts include, but are not limited to:

Muthyala, R., Bala, G., & Nalam, A. (2018). Regional scale analysis of climate extremes in an SRM geoengineering simulation, Part 2: temperature extremes. Current Science, 1036-1045.

Tilmes, S., Sanderson, B. M., & O'Neill, B. C. (2016). Climate impacts of geoengineering in a delayed mitigation scenario. Geophysical Research Letters, 43(15), 8222-8229.

Jones, A.C., Hawcroft, M.K., Haywood, J.M., Jones, A., Guo, X. and Moore, J.C., 2018. Regional climate impacts of stabilizing global warming at 1.5 K using solar geoengineering. Earth's Future, 6(2), pp.230-251.

Specific comments:

Lines 36-59: This first paragraph of the Introduction part is very lengthy and most part is not directly relevant to the study here. For example, the detailed description of extreme precipitation in Zhengzhou and Beijing is not needed at all.

Response:

Thank you very much for pointing out the redundancy of the introduction part. We have made the first paragraph more concise and pertinent to the study, including shortening the description of extreme precipitation in Zhengzhou and Beijing. Please see lines 40-66 in the revised manuscript.

Lines 61-72: This paragraph can also be substantially shortened and combined with the first paragraph.

Response:

According to your suggestion, we have substantially shortened the second paragraph, and combined it with the first paragraph. Please see lines 66-79.

Line 85: check the grammar here. ‘,the climate’

Response:

It should be “on the climate”. However, this sentence has been removed for brevity.

Lines 90-101: The use of ‘prediction’ in this paragraph is not appropriate.

Response:

We have replaced "prediction" with "projection" in the paragraph.

Lines 98-99: Whether SAI would decrease precipitation depends on the scenario of SAI deployment. Also, instead of Pinto et al. 2020 and Liu and et al. 2021, more influential papers on the climate effect of SAI should be cited.

Response:

Yes, we agree that different scenario of SAI deployment would have different effects on the climate, in particular, the spatial and temporal distribution of precipitation. In light of this consideration, the statement “SAI will exert a negative radiative forcing and reduce near-surface air temperature (including temperature means and extremes) (Pinto et al., 2020), and precipitation (Liu et al., 2021)” has been revised as: " However, the climate effects in terms of magnitude as well as spatial and temporal distribution depend largely on the scenario of SAI deployment. Furthermore, as suggested by some studies, although SAI can effectively counteract anthropogenic global warming at the global scale, it cannot fully offset the effects at regional scale (Tilmes et al., 2013; Niemeier et al., 2013; Tye et al,. 2022)”. Please see lines 113-116 of the revised manuscript.

Line 120: Why only use results from a single model? Why not use multi-model results from GeoMIP?

Response:

Thank you for pointing out this issue. In the response to your comment at the beginning, we have answered this point. Please refer to our explanation there.

Lines 186-187: I don’t quite understand this sentence.

Response:

We are sorry for the confusion due to our carelessness. Since we have already employed the Wilcoxon test for the significance testing, the field significance analysis mentioned here are not necessary, and in fact, it was not used in this study. Therefore, the description (line185-194 in original manuscript) has been deleted.

Line 199: The word of ‘accurate’ is not appropriate here.

Response:

The "accurate" was replaced with "similar to the observed precipitation" (line 224).

Line 133: ‘reducing the solar constant or increasing SAI’. Check grammar and spelling here.

Response:

The sentence has been removed for brevity.

Lines 225-226: I don’t understand what ‘SAI is sensitive to global warming’ means.

Response:

The sentence has been revised as “This suggests that the effect of SAI on future precipitation is more widespread and remarkable compared to that of SSP5-8.5.” Please see lines 273-274.

Line 245: Where are ‘the other three G6 models’?

Response:

Sorry for our carelessness. The “models” should be “scenarios”, and it has been corrected in the revised manuscript (line 294). By the way, the other three G6 simulations refer to that of SSP2-4.5, G6sulfur, and G6solar.

Response to Referee #3

General comments:

I think this could be a useful study with some work. I always like creative ways of integrating measurements and models. The analysis is also carefully done and focuses on clearly important issues (extremes).

While I don't dispute any of the findings, my biggest issue is with the explanations. There is a lot of reporting of the results but not much interpretation other than (sometimes) speculating about mechanisms. Given that the authors have a great deal of climate model output at their disposal, they could look into some of these mechanisms. I would point out specific examples, but this seems to be a general issue in Section 3.

Also, there is a lot of discussion of different indices, but they mostly show the same thing. That's not a problem, but the way you're describing them makes it seem like you're going through a laundry list of indices. I'd like to see more insight. Digging into the results in Table 2 would be interesting. For example, _why_ does CWD not behave like the other indices? What's special about those two regions that have the opposite sign?

Response:

First of all, we'd like to note that we have realized that the title of the original manuscript was rather over-reached in terms of what is presented in the paper and changed it to "Projected future changes in extreme precipitation over China under stratospheric aerosol intervention in the UKESM1 climate model", which is more relevant to the results of the article.

We have to admit that interpretation of the results and speculation about the mechanisms are insufficient, partially because of our limited knowledge in relevant areas. Though it can't be fully made up for the time being, we are trying to make improvements by adding some more interpretations and discussions in our revised manuscript (e.g., in lines 423-426, 466-468, 537-538). Nevertheless, the main purpose is to demonstrate the possible impacts of SAI by comparations among different scenarios. Really, it would be more interesting to discuss on causal mechanisms linking the response to the impacts, considering the relatively scarce research and limited knowledge on the impact of SAI over East Asia, we think simply documenting the impacts in extremes (as in Tye et al. (2022)) also appears worthy, at least to some.

Thank you for your suggestion for digging into the results in Table 3 (the Table 2 has been changed to Table 3 in the revised manuscript). We have added following discussions/statements in the revised manuscript:

At lines 441-443: “In the regions projected to experience an increase of CWD in NE and NWC, the positive value (in Table3) indicates that SAI experiments produce results of threshold that are closer to the CP conditions. However, the relative effect is not obvious due to the small magnitude of CWD in these regions.”

At Lines 509-510: “As shown in Table 3, the DD is positive in the SC region, meaning G6sulfur effectively lowers the threshold for extreme DD events compared to SSP5-8.5. This suggests that the SAI is more effective for DD maximum in the humid region.”

At Lines 522-529: “The positive value in Table 3 of the CDD index in the SC and SWC regions in Table 3 indicates that G6sulfur notably closes the threshold of CP extreme CDD events compared to SSP5-8.5, thereby approaching drought extremes of CP in these regions. This suggests that G6sulfur has the potential to mitigate the CDD extremes. The ameliorating effect of DD and CDD compared to SSP5-8.5 in the SC region under G6sulfur may be related to the strengthening of the anti-cyclonic circulation associated with the subtropical gyre, which appears to increase under G6 compared to SSP5-8.5 (Liang and Haywood, 2023). This intensification results in an increased inflow of moist air from the ocean at 850hPa and a greater supply of moisture to the southern region of the area.”

Specific comments:

Figure 1 and Section 2.1: Any reason you don't include the Tibetan plateau?

Response:

As shown in Figure1, the Tibetan plateau is included and represented as the brown areas, which is divided into two parts in SWC and NWC. The division of the regions follows a conventional approach that has been widely adopted in many statistical reports and relevant studies (e.g., Luo et al., 2017; Fan et al., 2020; Yang and Shao, 2021; Liang et al., 2023).

Lines 169-182: This seems like a long way of saying that you used survival functions, which are a perfectly reasonable thing to use for what you want to do.

Response:

We have revised it more concise. Please see lines 194-207.

Lines 186-187: This is not consistent with my understanding of what field significance does. I would appreciate more description as to what you mean.

Response:

We are sorry for the confusion due to our carelessness. Since we have already employed the Wilcoxon test for the significance testing, the field significance analysis mentioned here are not necessary, and in fact, it was not used in this study. Therefore, the part has been deleted.

Figure 2: Can you add a panel showing the bias as a percent instead of an absolute value?

Response:

Thank you for your valuable suggestion. Really, it would be meaningful to show the bias in terms of relative changes, in addition to the absolute value. When trying to do so (adding a panel showing the bias as percentages), a problem we met is that, since a great deal of the results (daily precipitation) are small values, the relative changes (percentages) could be very large even for minor absolute changes (particularly for those in western and northern areas), and this could make the results confused.

For this reason, a panel of scatter plot comparing the observations with the simulations (mean of the ensemble model) is added as Fig.2d in the revised manuscript (line 237), also shown below. In addition, comparisons between the observations and each of the three model members are also provided as supplementary material (Figure S1). The observations (daily precipitation) during the control period in China were classified in to several intervals: P10 (the smallest 10%), P10-50, P50-90, P90-95, and P95 (the largest 5%). In order to indicate the bias as a percent, relative changes (compared to the observations) for different intervals have been calculated, and listed in a new table2 added in our revised manuscript (lines 246-247), which is also provided below.

The scatter plots indicate a close relationship between the observations and the model results. However, the model results are generally higher than observations, possibly because of the different resolution of the data. Since our study has been mainly focused on the relative changes between the future results and that of control period for different scenarios, the systematic bias would not affect the conclusions significantly. As expected, relative changes are very large at small values (below the 10th percentile), both for the ensemble mean and the model members. For the results at the 10-50th and 50-90th percentiles, relative changes are around 30%. When larger than the 95th percentile, relative changes are relatively small, near 15%. The differences among ensemble members are not significant, which suggests the uncertainty in the ensembled results is reasonable and acceptable. What mentioned above have been included in our revised manuscript. Please see lines 248-255.

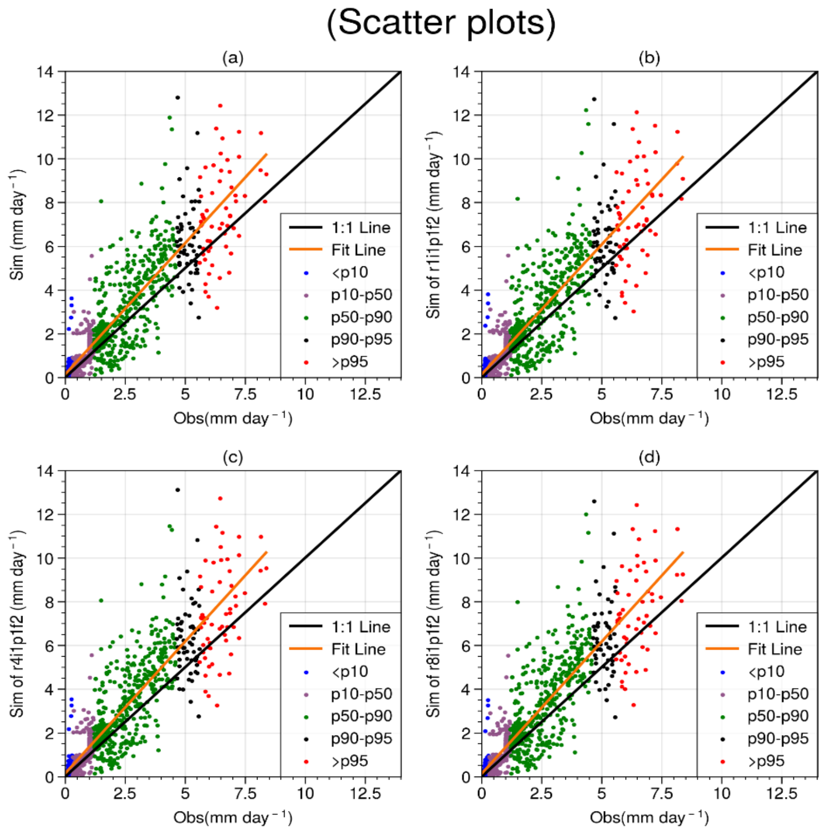

Figure Scatter plots between the observations and model results at different level of precipitation during the control period (CP). The first panel is provided as Fig.2d in the revised manuscript, and the other three panels are provided as Fig. S1 in the supplementary material. The observations were classified into several intervals: P10 (the smallest 10%), P10-50, P50-90, P90-95, and P95 (the largest 5%).

Table2: Relative changes of the model results (compared to the observations)

intervals	Ensemble mean	r1i1p1f2	r4 i1p1f2	r8i1p1f2
<P10	89.81%	93.95%	89.44%	86.04%
P10-50	30.05%	30.38%	31.85%	27.13%
P50-90	30.50%	28.95%	31.36%	31.16%
P90-95	24.03%	22.79%	24.85%	24.44%
>P95	15.76%	15.09%	16.27%	15.92%

Lines 225-226: I'm not sure what this means. SAI is sensitive to global warming?

Response:

We are sorry for the mistake. The sentence has been revised as "This suggests that the effect of SAI on future precipitation is more widespread and remarkable, compared to that of SSP5-8.5". Please see lines 273-274.

Line 265: Typo (depicting)

Response:

We have corrected 'dicipting' to 'depicting. ' Line 314.

Line 293: Aggregated how?

Response:

Sorry for the wording mistake. The "aggregated" should be "presented". The sentence has been revised, please see lines 340-349.

Line 295: Be more specific about "the opposite". Also, what are 0 values in the table? (I can figure it out, but you need a description.)

Response:

To be more specific about "the opposite" and "0 values", an explanation is provided: "A positive difference suggests a mitigation effect of SAI, while a negative difference indicates exacerbation in index thresholds for projected increase regions. In regions where the projected index is decreasing, the meaning of positive and negative signs is opposite to that in regions where the index is projected to increase. In addition, the 0 values indicate there is almost no difference between the maximum index values under G6sulfur and SSP5-8.5, suggesting negligible impact of SAI on indices threshold." Please see lines 345–348 of the revised manuscript.

Lines 324-325: It's difficult to put these numbers in context. Is 100 mm a lot for these regions?

Response:

The sentence has been revised as: "in EC and CC, there are decreases of more than 100mm, whereas in NEC and NC, there are some increases of about 50mm." Please see lines 374-376.

Line 341: I don't know if "effectively mitigates" is the correct phrasing. Be more specific.

Response:

The sentence "SRM effectively mitigates the increase of RX1day, RX5day and R95p compared to SSP5-8.5 scenario in all regions" has been changed to "SRM results are encouraging, showing a reduction in the detrimental extreme events, similar to the lower emissions target of SSP2-4.5". Please see lines 392-393.

Lines 391ff: I'll be honest, I had a hard time with this entire paragraph. I'm really not sure I understand it.

Response:

Thank you for your feedback on this paragraph. We have revised it as: "In the regions projected to experience an increase of CWD in NE and NWC, the positive value (in Table3) indicates that SAI experiments produce results of threshold that are closer to the CP conditions. However, the relative effect is not obvious due to the small magnitude of CWD in these regions. It is notable that in NC, and SC, G6sulfur (black) provides similar results to the SSP2-4.5. Interestingly, for EC, SSP2-4.5 yields almost identical statistics to SSP5-8.5, while both G6sulfur and G6solar show an increase compared to SSP scenarios." Please see lines 441-451.

Line 520: ETCCDI

Response:

Thank you for pointing out the mistake, it should be “ETCCDI”. However, this sentence has been removed for brevity.

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