Dear reviewer,

Thank you very much for your thoughtful comments and constructive suggestions. We appreciate your recognition of the potential value in our study and your encouragement of using innovative approaches that integrate measurements and models. We have considered all the comments by you and other reviewers carefully, and will revise (actually, we have mostly revised) the manuscript comprehensively. A point-by-point response to each of your comments is provided below.

Thank you again for your valuable time and work, and we look forward to your positive decision on our paper.

With warm regards,

Ou Wang, on behave of all co-authors

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 our 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 our knowledge in this area is still limited. Though it can't be fully made up for the time being, we are trying to improve it by adding some more interpretations and discussions in our revised manuscript. 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 2. We are going to add following discussions/statements:

At line 394-397: "The positive value in NC and NWC indicates that SAI experiments produce results that are closer to the PD conditions, which suggests the effect of SAI in the northwest arid regions is more significant in reducing precipitation compared to SSP5-8.5 scenario. However, the relative effect is not obvious due to the small magnitude of CWD in these regions."

At Line 457-459: "As shown in Table 2, 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 in the humid region.

At Line 469-472: "The positive value of the CDD index in the SC and SWC regions indicates that G6sulfur notably reduces the threshold for extreme CDD events compared to SSP585, thereby approaching PD conditions in these regions. This suggests that G6sulfur has the potential to mitigate the decrease of CDD in the SC and

alleviate the increase of CDD in the SWC. The ameliorating effect of DD and CDD 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 in the paper, the Tibetan plateau is indeed represented in the brown areas, 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), despite that there is no standard criteria for such divisions. We will clarify this in the revised manuscript.

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 appreciate your recognition of the method we used in the article. The description might be too lengthy, and we considering to make it more concise.

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:

Thank you for pointing this out. We'd like to clarify that 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) should be deleted. We are sorry for the confusion due to

our carelessness.

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 found 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 would make the results confused.

For this reason, a panel of scatter plots comparing the observations with the model results are added, as shown below. The new panel has 4 sub-panels, with each comparing the observations with the mean of ensemble model and the three model members, respectively. 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, as listed in following table.

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 will be included in our revised manuscript.

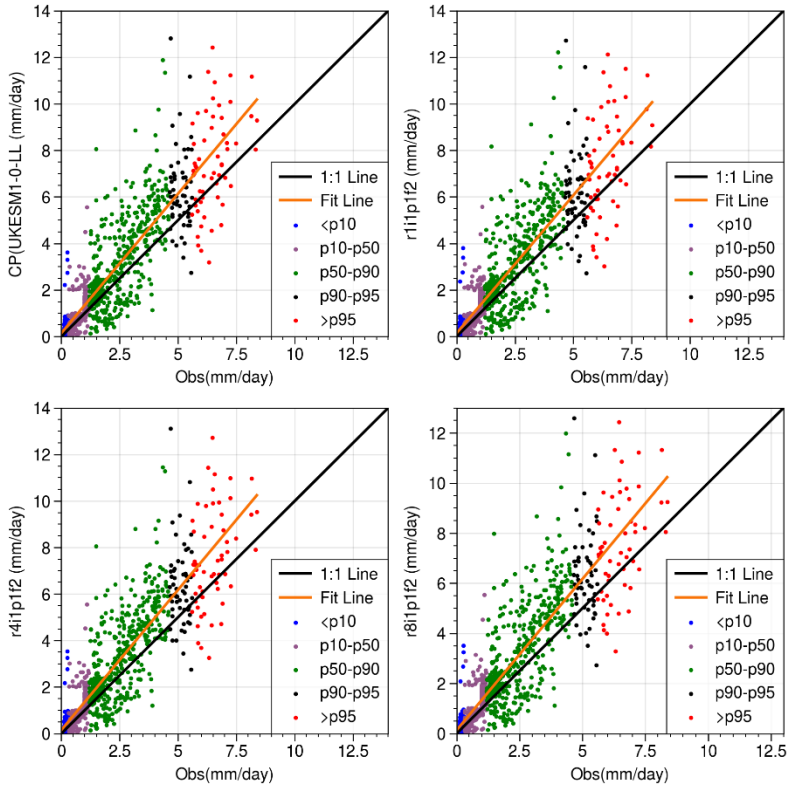

Figure 2(d) Scatter plots between the observations and model results at different level of precipitation during the control period (CP).

The observations were classified into several intervals: P10 (the smallest 10%), P10-50, P50-90, P90-95, and P95 (the largest 5%).

Table: 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 mistakes. 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,"

Line 265: Typo (depicting)

Response:

We have corrected 'dipicting' to 'depicting. '

Line 293: Aggregated how?

Response:

Sorry for the wording mistake. The "aggregated" should be "presented". The sentence (lines 291-293) should be “For each index, the differences between the maximum values under the G6sulfur and SSP5-8.5 scenarios compared to the current period are presented in Table 2.”

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”, detailed explanation is provided: “Specifically, positive values indicate that the index under the G6sulfur are closer to that under the current period, compared to the SSP5-8.5 scenario, meaning that the mitigation effect of G6sulfur is significant. On the contrary, negative values mean the enhanced effect under G6sulfur compared to SSP5-8.5 scenario. In addition, 0 values in the table represent instances where there is no difference between the maximum index values under G6sulfur and SSP5-8.5, suggesting no significant impact on extreme precipitation events.”

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

Response:

The 100 mm here is in comparison with other regions. The sentence (lines 324-325) should be revised as: “in EC and CC, there are substantial decreases of more than 100mm compared to other regions, whereas in NEC and NC, there are some increases of about 50mm.”

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 efficacy of detrimental extreme events, similar to the lower emissions target of SSP2-4.5"

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 to improve clarity and coherence: "It is notable that in NC, and SC, G6sulfur (purple) provides similar results to the SSP2-4.5. When combined with Fig.8b and c (less than 2 days), suggests that G6sulfur yields statistically similar outcome to that of SSP2-4.5 in NC and SC. In SWC, the 395 purple line and orange line also closely tracks the green line, indicating a similar cumulative distribution of CWD between G6sulfur and SSP2-4.5, and between G6solar and SSP2-4.5. However, there is an uneven spatial distribution, as seen in Fig.8b, and Fig.S5. Consequently, SRM is not expected to reach the levels of SSP2-4.5 in SWC. 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. However, the negative values of CWD in EC in Table 2 and S2 indicate that SRM strategies cannot ameliorate the high values of CWD 400 in the EC region."

Line 520: ETCCDI

Response:

Thank you so much for the careful suggestion. The "ETDCCI" has been corrected to "ETCCDI"

Reference

Liang, J. and Haywood, J.: Future changes in atmospheric rivers over East Asia under stratospheric aerosol intervention, Atmos. Chem. Phys., 23, 1687–1703, https://doi.org/10.5194/acp-23-1687-2023, 2023.

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.