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# *Interactive comment on* "Accelerated increases in global and Asian summer monsoon precipitation from future aerosol reductions" *by* Laura J. Wilcox et al.

**Laura J. Wilcox et al.**

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We thank the reviewers for their constructive comments. In our response, referee comments are indicated in **bold**, with our comments and changes to the manuscript in plain text. In addressing the reviewers' comments, we have added a new figure to the manuscript. Throughout our response, when discussing figures, we give both the original and revised figure number.

**Reviewer 1: Bryce Harrop**

**The manuscript makes use of the available CMIP6 SSP projection simulations to evaluate the impact of changing aerosols on the hydrological cycle over South Asia and East Asia. Despite the lack of clean experiments (non-aerosol differences occur across SSPs), the authors argue that simple and robust patterns appear that fingerprint the role of aerosol uncertainty on changes in precipitation, most notably during the first half of the 21st century. It is often difficult, however, to follow the line of reasoning used in the text of the manuscript when examining the figures presented. I have made a note of several such passages that seem to disagree with what is presented in the figures in the specific comments. There are also several points of discussion in the manuscript relating global scale and regional scale differences, but there is little evaluation presented for which scales are important for which findings. A clearer definition of what constitutes agreement with the hypotheses would make this manuscript much easier to follow. Finally, in addition to discussions about the role of GHGs vs aerosol, there is no mention of land use/land cover change and the impact of its differences between SSPs on rainfall over South Asia or East Asia in this manuscript.**

Thank you Bryce for the thoughtful and detailed review. We have added detail to the text throughout the manuscript, which hopefully makes our reasoning clearer. Where you had specific concerns about particular paragraphs, we have addressed them in the manuscript and respond to them directly below.

AR5 suggested that land use forcing was an order of magnitude smaller than that from anthropogenic aerosols, so we didn't consider it in the original manuscript. However, we have now looked into the details of the experiments in CMIP6, and the available literature, and agree that it is important to mention this. We have now included a summary of land use changes in our initial description of the SSPs, and commentary on their potential role in the manuscript.

Where data are available, we have calculated the global mean ERF due to anthropogenic aerosol changes and to land use changes. We have included these values in Table 2, alongside the ERF from greenhouse gas increases, and the Equilibrium Climate Sensitivity. The forcing from land use is much smaller than that due to aerosol. However, we note in the manuscript that it may be of more importance locally.

**Specific comments**

**1. The authors argue that, “If the magnitude of the anomaly decreases monotonically from SSP1-1.9, which has the largest aerosol reduction, to SSP3-7.0, which has a moderate aerosol increase, this indicates that aerosol changes are the main driver of the climate response.” When looking at the global emissions of BC and SO2 presented in Figure 1, this seems reasonable, but the same logic appears to be applied regionally in this manuscript. Looking at South Asia during the 2015-2050 period, SO2 emissions are highest for SSP5-8.5 and nearly equal for SSP2-4.5 and SSP3-7.0. How are we meant to disentangle the regional and global scale impacts for this region?**

Disentangling regional and global scale impacts is a study in itself, and an interesting one. It wouldn’t be possible to do with the type of experiments that we consider here. There are a number of published studies that look at the relative roles of local and remote aerosol emissions for monsoon changes. We now refer to these in the manuscript, and make clear that when we look at the monsoon response in the SSPs we are considering the effect of both local and remote aerosol changes.

**2. SSP2-4.5 and SSP5-8.5 are said to have “similar aerosol pathways,” and globally that appears to be the case (Fig 1). Again, however, over South Asia, the differences in BC and SO2 emissions between SSP2-4.5 and SSP5-8.5 appear to be as large as their differences relative to SSP3-7.0. This point is raised again in the discussion of Fig 4 where the authors state, “SSP5-8.5 has similar aerosol**

**changes to SSP2-4.5, consistent with the similar changes in emissions (Figure 1).” Given how dissimilar the regional emissions are in Figure 1, it is disconcerting that the AOD pattern for SSP5-8.5 is left off Figure 4, as this would allow readers to accurately see how similar or not the regional emissions are.**

We have added the AOD for SSP5-8.5 to Figure 4, and more clearly delineated our discussion of regional and global aerosol when introducing the SSPs.

In our discussion of the results we now refer to the different characteristics of the emission pathways over South Asia compared to the global and East Asian case, and discuss the impact of this in the context of the monsoon changes.

**3. Figures 5 and 6 show the model mean responses (as points), as well as their interquartile spread, for global (fig 5) and regional (fig 6) metrics. The temperature responses show noticeable spread between the different pathways, particularly by 2045-2054, but the precipitation responses have far less separation between pathways. I found it difficult to parse what measure the authors use to decide whether precipitation has increased or decreased between pathways. I began by assuming they were referring to the median (which I assume is the horizontal line in each bar). If that were true, then the statement, “Global aerosol reductions in SSP1-1.9 briefly cause this scenario to warm faster than the others considered over Asia and East Asia...” should be changed to refer only to East Asia, as Fig 6a (left panel) does not show a larger median temperature anomaly for SSP1-1.9 than SSP2-4.5. Additionally, the statement, “Over Asia, the largest mean precipitation increase occurs, for all decades, in SSP1-1.9...”is difficult to parse when it isn’t clear if the “mean precipitation” is even marked in the figure. Is the bar actually the multi-model mean? If that is true, then the increase in precipitation over Asia is larger in both SSP2-4.5 and SSP5-8.5 than it is in SSP1-1.9. These two figures, and their accompanying text, must be clarified before any rigorous evaluation of the conclusions can be made. I also strongly**

**recommend adding some discussion of when differences between regional precipitation changes at the decadal scale are statistically significant, or at a minimum robust across models.**

We have now included a paragraph clarifying the approach used in Figures 5 and 6 (revised Figures 6 and 7). The horizontal bars are the median, and we have now taken care to refer to this consistently in the text, rather than referring to the mean. We now include a discussion of significance and robustness throughout this section. For our sample size, the 95% confidence interval about the median is typically very close to the interquartile range, based on the empirical relation in McGill et al. (1978). To account for the asymmetry in the distribution of models about the median in some cases, we use the interquartile range to determine significance.

**4. The cooling over India is argued as the reason for suppressed precipitation in-creases in SSP2-4.5 and SSP5-8.5 relative to SSP1-1.9 and SSP3-7.0, but the cooling in Figure 7 is strongest for SSP3-7.0. How does one reconcile this? On a similar note, why are the temperature anomalies for South Asia and East Asia all positive in Figure 6a when Figure 7 shows cooling for SSP2-4.5, SSP3-7.0, and SSP5-8.5 for2025-2034?**

Figure 6 (revised Figure 7) shows an anomaly relative to 1980-2014, so includes a considerable amount of global warming. Figure 7 (revised Figure 8) shows the same for SSP1-1.9. For the other scenarios in Figure 7 (revised Figure 8), we show a difference relative to SSP1-1.9 to try to highlight the differences between the scenarios. This is the reason for the apparent change in sign between Figures 6 and 7 (revised Figures 7 and 8), and we have clarified this in the text and the caption.

We have removed the argument for cooling as the reason for suppressed precipitation since precipitation changes can also lead to temperature changes.

**5. The warming and rainfall change patterns for the two individual SSP2-4.5-aer simulations are difficult to compare to the multimodel mean, and even to the rainfall response in Figure S8 (owing to changes in both the range of the colorbar and the colors used). It would be useful to show a direct comparison of the full SSP2-4.5 response to that of SSP2-4.5-aer for each of the two models available so that an assessment can be made for how much the climate responses are indeed driven by aerosols.**

This comparison is now included. We show both SSP2-4.5-aer and SSP2-4.5 for MIROC6 in the main text (revised Figures 12 and 13), and SSP2-4.5-aer and SSP2-4.5 for CanESM5 in the supporting information (Supplementary Figures 7 and 8). We now use consistent colours for our precipitation scales throughout the manuscript to facilitate comparison between figures.

**6. Figures are too small to be readable when printed, and the quality is so low that they are hard to read even when zoomed in on a computer. Please consider revising with vector graphics or higher DPI raster images. It would be helpful to readers to add an outline of the analysis regions (Asia, S. Asia, and E. Asia) to the map plots. Please maintain a consistent map projection for all map plots. Please also be consistent with colorscales so that metrics can be compared across figures (e.g., Fig 7 vs Fig 11, or Fig 9 vs Fig 11). Finally, please consider changing Fig 4c to be MMM-MODIS so that it is consistent with the caption.**

We have provided both vector and higher DPI raster images to ACP, and added outlines of the analysis regions to Figure 4.

All regional plots now use the same domain, except for Figure 3 (revised Figure 4), S1, and S2, where we use a slightly smaller domain. These figures show a comparison to APHRODITE, which has a limited data domain.

The different magnitudes in Figures 7-11 (revised manuscript: Figures 8-12) made it difficult to use exactly the same colour scale throughout. However, we have now standardised the type of colour scale used for each variable, so that temperature is now blue:yellow:red, precipitation is red:white:blue, and sea level pressure is brown:white:green throughout.

We have made the suggested change to Figure 4c (revised Figure 5c).

**Technical corrections**

**Page 2, line 34, "AA" is not defined Page 4, line 7 typo "has yet to be emerge" Page 6,line 6 typo "present - day" Figure 2 caption typo "180-2014" Figure 7, there is a change in font between subpanels**

All now corrected, thank you.

**References**

Robert McGill, John W. Tukey and Wayne A. Larsen. Variations of Box Plots, The American Statistician, Vol. 32, No. 1 (Feb., 1978), pp. 12-16

**Reviewer 2**

**This study investigates the possible influences of different aerosol reductions in the future on the global and Asia surface temperature and rainfall. The topic is quite important, but the method they took may have some problems, at least for some conclusions. Their writing is very unclear (with many typos, which greatly affect the reading experience) and very hard to follow. At the same time, the figures are so small and so unclear (also the captions) that I try my best to understand what they showed. Besides these, I still have several major comments**

**and I don't think this manuscript can be accepted unless all these concerns are well addressed.**

We thank the reviewer for their comments and are sorry to hear that they found our writing unclear. We have corrected the typos identified by both reviewers, made changes to the text to further improve the clarity. We have added more detail about our methodology. We have also added extra detail to the captions of Figures 5, 6, 7, 8, 9, and 10, and included either higher resolution or vector versions of all figures. We have also addressed the reviewer's detailed comments in the manuscript, and provide responses for those separately below.

**Major comments:**

**1. Due to the lack of clean experiments, the guidance to distinguish the relative importance of GHG and aerosol forcing in this study is that different scenarios may be similar in one forcing change, while very different in the other forcing change. This seems plausible, but the question is whether the other forcings (e.g.,land use) keep unchanged in different scenarios. I guess probably not. So the question is whether they are important or not for the main conclusion drawn here. I think the authors should seriously think about it and do some analysis on it.**

The SSPs do include a range of land use changes in addition to a range of aerosol pathways. We have now included a summary of land use changes in our initial description of the SSPs, and commentary on their potential role in our results. There is a limited amount of literature available that already compares the relative roles of anthropogenic aerosol and land use changes in monsoon changes, and we now refer to this in the text. This work suggests that the response to anthropogenic aerosol changes is larger than the response to land use changes over China, but that land use changes may be important over India.

Where data are available, we have calculated the global mean effective radiative forcing (ERF) due to anthropogenic aerosol changes and to land use changes. We have added these values to the manuscript in Table 2, alongside the ERF from greenhouse gas increases, and the Equilibrium Climate Sensitivity. The forcing from land use is much smaller than that due to anthropogenic aerosol. However, as we now note in the discussion, it may be of more importance locally. Overall, it looks like the land use changes will drive monsoon changes of the same sign as the aerosol driven changes, and we have also noted this in the manuscript.

Given that the forcing from land use changes are so small compared to the forcing from anthropogenic aerosol, we think it would be distracting to include analysis beyond a comparison of the radiative forcings and a discussion of the relevant literature in this paper.

**2. From Fig. 5, it seems that for the global mean precipitation and hydrological sensitivity, the responses of most models are close to each other, except two models with totally opposite signs (one with large positive value and the other with large negative value). Could you do more analysis on these two models? With the same aerosol emission, how can these two models produce totally opposite results? To me, I know the aerosol forcing has large uncertainty (should affect the results in a quantitative way), but in a qualitative way, it should be the same result at least at the global mean. Hence, it quite surprises me. In Fig. 6, it seems that over Asia, the uncertainty is smaller, at least not opposite.**

The outlying models in Figure 5, and the large opposite responses from two models in Figure 5c, are mainly the result of our choice to show anomalies relative to 1980-2014, rather than large differences in absolute values across the models. These anomalies for each SSP include a large amount of global warming, and the difference between the outlying models is largely a reflection of different climate sensitivities, rather than differences in the response to aerosol forcing. For each scenario, the outlying models

are the same in each case, so have no influence on the relative differences between the scenarios.

Figure 1 of this response shows the temperature time series that are used in Figure 5 (revised Figure 6). SSP2-4.5 is used as an example. Panel (a) shows the absolute values of global-mean JJA-mean near-surface temperature. The outlying models from Figure 5 (revised Figure 6) are highlighted with bolder lines. Panel (b) shows the same data as anomalies relative to 1980-2014, which is what we show in the paper. Comparison of the two panels demonstrates that the models are not unusual in their mean climate, or the sign of the trend, but do warm relatively more (or less) than the other models between 1980 and 2020.

We have done some further analysis of the outlying models from Figure 5 (revised Figure 6), as suggested by the reviewer. Globally, the low outliers are MIROC6 (temperature) and CAMS-CSM1-0 (precipitation), while the high outliers are EC-Earth-Veg, UKESM, and CanESM5 (temperature) and UKESM (precipitation). These models are those with the lowest and highest climate sensitivities in our subset, consistent with them having the smallest and largest trends over 1980-2014 (as shown in Figure **??** of this response). These points are now noted in the manuscript. Maps of the precipitation responses in the individual models are shown in Figure S10.

**Specific comments:**

**1. Page2L35: Why is this case? It is hard to understand. It is better to provide an explanation here.** We now explicitly state that future warming is driven by a combination of positive radiative forcing from greenhouse gas increases and positive radiative forcing from anthropogenic aerosol decreases, so that a weaker aerosol forcing results in a more moderate warming.

**2. Page3L13: full->fully** Done

**3. Page3L14: add aperiod.** Done

**4. Page7L3-4: You should clearly state this in the figure caption to make sure each figure can be understood from the figure itself.** Details of the quantities shown in the box plots have been added to the captions for Figures 5 and 6 (revised Figures 6 and 7).

**6. Page7L18: Please add "partly". I don't think aerosol forcing explains all the weakening of Asian summer monsoon.** Changed to 'largely'. We accept that a single forcing is unlikely to explain all of the weakening, but there is good evidence that aerosol forcing is the dominant driver (relevant papers cited in manuscript).

**6.Page7L30: remove "the"** This sentence has been rewritten.

**7. Section 4.1: I don't think it is suitable to compare the SSP2-4.5-aer simulations from two models with SSP2-4.5 simulations from all models. You should compare these two simulations from the same model.** This comparison is now included. We show both SSP2-4.5-aer and SSP2-4.5 for MIROC6 in the main text (Figures 12 and 13), and SSP2-4.5-aer and SSP2-4.5 for CanESM5 in the supporting information (Figures S7 and S8).

### Additional changes not requested by the reviewers

There was a problem with the secondary organic aerosol in the CESM SSPs and the data has been withdrawn: https://errata.es-doc.org/static/view.html?uid=eb69632c-a6e2-7667-a112-a98b7745e2ea We have removed these simulations from our analysis.

In the submitted version of the manuscript there were data points with a temperature anomaly of 0K in Figure 5a. These were erroneous, and have been corrected in the revised version (Figure 6a).

As part of our attempt to improve the readability of the manuscript, we have replaced the JJA mean interhemispheric temperature gradient originally shown in Figures 2b and 5d with the annual mean, making it consistent with the other panels in the figure. We had originally included JJA here to give a closer link to the monsoon results discussed later in the manuscript. However, the pattern of the response across the SSPs is similar in both seasons, and the use of the annual mean for this panel means that all discussion in Section 3 is for the same season. The panels from Figure 5 (revised Figure 6) for the annual mean (a) and JJA mean (b) are shown in Figure 2 of this response. There is no qualitative difference between them when comparing the relative position of the median across SSPs.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1188, 2020.

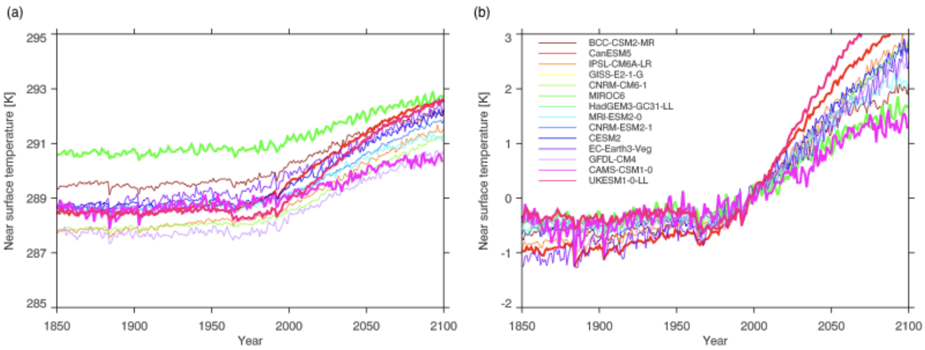

**Fig. 1.** (a): Annual-mean global-mean temperature time series for the historical simulation and SSP2-4.5. (b): The same data as shown in panel (a), but presented as an anomaly relative to 1980-2014.

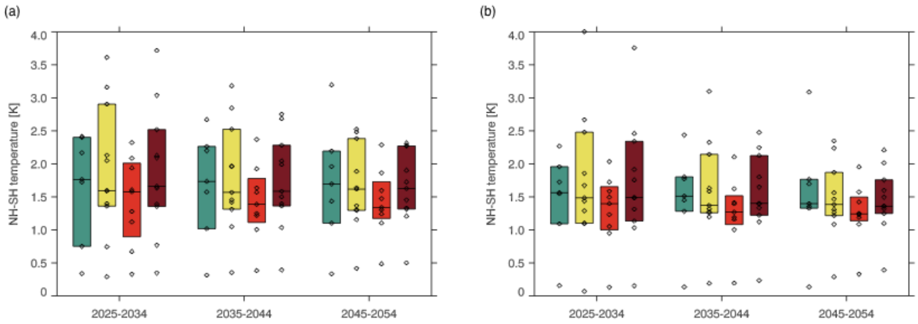

**Fig. 2.** (a): Annual-mean interhemispheric temperature gradient anomalies relative to 1980-2014 from SSP1-1.9, 2-4.5, 3-7.0, and 5-8.5 (as shown in the revised manuscript). (b): As for panel (a), but for JJA