Reviewer #3

The authors utilized the perturbation experiments to investigate the global and Indian precipitation responses to external forcings, including CO2, black carbon (BC), and sulfate aerosol. Based on the analysis discussed in the manuscript, the authors demonstrated that the precipitation over the Indian subcontinent shows different responses to aerosols compared to the global precipitation changes, which can be explained by aerosol-induced circulation changes. The regional precipitation responses induced by the circulation shifts are interesting and worth investigating. However, a large piece of the conclusion discussed and summarized in the manuscript, unfortunately, is well documented by multiple previous studies. In addition, the current analysis of the circulation part stopped at a very preliminary step, further weakening the robustness of the current conclusions. Overall, the motivation of this manuscript is interesting, but the current manuscript needs careful revision and deeper analysis. It would be more valuable to the community if the authors could focus on the regional responses due to the circulation shift.

We thank the reviewer for the constructive comments and suggestions. We have addressed all the comments in the revised version of the manuscript. Please find the point-wise response to the comments in the blue italics below.

Major comments:

1. The first two conclusions in section 4 are well-documented in multiple previous studies, especially the global precipitation responses to CO2 and anthropogenic aerosol. The authors spent too many words discussing these well-known results, which leads to a redundant section 3. It would be better if the authors acknowledged the existing literature in the introduction and included more regional changes and dynamics analyses in section 3.

   We have removed the first conclusion as it is well-documented. We have modified. 2 (now conclusion no. 1) because our emphasis lies in unique energy metrics, differing from those explored in prior papers that utilized PDRMIP datasets.

   Consistent with the paper's aim to perform a comparative analysis of global and regional responses (over India) to different anthropogenic forcings, we commenced with a global analysis before exploring regional variations. Consequently, retaining the global precipitation responses was essential. Additionally, we present comprehensive results encompassing all models and aerosol perturbed experiments including $co_2 \times 2$ involved in the PDRMIP project.

In addition to the regional analysis (over India) presented in Figures 5b, 6b, 7, 8, 9, 10, and 11 (in the revised version), we have introduced new figures:

i) Figure 3 illustrates precipitation changes per unit K, calculated by taking the ratio of precipitation change (%) to the global annual mean temperature changes relative to the base simulations. This ratio, termed apparent hydrological sensitivity (HS), provides insights into the sensitivity of precipitation in India (or any region) to different forcing agents.

ii) We incorporated zonal mean annual and JJAS precipitation change in Figure 4 to elucidate the movement of the ITCZ across latitudinal regions resulting from individual climate forcers..

iii) Figures S4 and S5 showcase the annual cycle of mean precipitation (mm/day) and temperature gradient (K) over India for all perturbed experiments conducted by individual models, providing insights into model internal variability.

2. The major part of the study focuses on the annual total precipitation, which is strange. The authors already mentioned ITCZ and the monsoon activities may be important to the regional precipitation changes over India, and it is less investigated. If so, why not focus on the monsoon season? The circulation background near India varies greatly, comparing the monsoon seasons and the other months.

As mentioned in the response to comment no.1, the objective of the paper was to do comparative analysis between global and regional total precipitation responses, we discussed the first sensitivity of annual precipitation to varying the GHG and aerosol strength. As per reviewer's suggestion, we have shortened the annual precipitation responses.

Since the precipitation changes is higher during the monsoon over India (Figure S1 and S2), we also investigated the dynamical changes associated with it. The precipitation and near-surface temperature response during the monsoon period were presented the supplementary section (Figure S1 and S2). It may kindly be noted that we had presented results on circulation changes considering during the Indian monsoon season only. Please see Figure 9 (earlier 7) and Figure 11 (earlier 9). Apart from the circulation, the changes in the vertical distribution of air temperature were also shown during the monsoon season over India that strengthened or weakened the *circulations depending upon the individual climate forcer. We have also included Figure 4 which shows the zonal mean of annual and monsoon precipitation changes to identify the ITCZ movement. Please see line no. 253 to 262, 282-288 and 291-294 in the revised version of the manuscript.*

3. All the discussion based on the sulaasiared and sulred is unconvincing because of the limited ensemble sizes (actually, they should not be called an ensemble), meaning the internal variability heavily impacts the related patterns. I suggest removing any discussions on these two experiments.

   We agree with the reviewer. This was there in our mind while carrying out this analysis. Notably, the MIROC-SPRINTARS model executed all the experiments, and we intended to avoid limiting our analysis to a single model. Consequently, we conducted a comprehensive study, encompassing all simulations carried out by each participating model in PDRMIP.

   As per suggestion, we removed term "ensemble" (_ENS) from all the figures related to sulasiared as it involved only one model. It is essential to note that responses derived from a single experiment (e.g., sulasiared) performed by one model (or, in the case of sulred, by two models) are in line with the literature for that model. Hence, we chose to retain these results in the manuscript.

   We have included new figures S4 and S5 in the supplementary section where the annual cycle of precipitation and temperature gradient is shown over India for all the experiments from all the individual models. We have now mentioned this wherever the results associated with these experiments are discussed in the manuscript.

4. The magnitude comparisons between experiments in the first few paragraphs of section 3 are less meaningful because of the totally different global energy budgets between the models. For example, the authors argued that the magnitude of temperature/precipitation changes over the Tibetan Plateau in bcx10 is greater than that in bcx10asia (lines 204–244), but this does not indicate any useful information because the bc forcing in bcx10 is much greater than that in bcx10asia. Magnitude comparisons between sulx5 and sulx10asia are also problematic because of the same issue. The authors should only look at the spatial patterns or relative changes in magnitude.

   Thanks for the suggestion. As suggested, we looked at the spatial patterns in this section. We have also analyzed apparent HS (Hydrological Sensitivity) parameter (Figure 3) which indicated precipitation changes over India and Tibetan Plateau to be more sensitive to Asian BC emissions compared to bc×10 (line no. 284-286).

5. Some of the descriptions of the spatial patterns are less convincing. For example, the “increase in precipitation over most of the continental land region in co2x2” is inconsistent with what is shown in Fig. 1a. If I read it correctly, most of the increases in Fig1a occur over the ocean, while land regions show negligible changes based on the current colormap. Similar issues occur repeatedly in this paragraph.

   We thank the reviewer for the correction. There was some mistake. Changes incorporated. Please line no. 241-243 in the revised version of the manuscript.

6. The authors kept describing the potential importance of ITCZ in forcing regional precipitation changes, which is certainly important to better understand the dynamical responses. However, no figures/analyses are included in the current manuscript. I suggest putting more effort into the analysis and discussion about ITCZ.

   We have added a new Figure 4 showing the zonal mean of annual and JJAS precipitation changes due to individual climate forcers to imply the importance and movement of ITCZ due to forcings. Subsequent discussion is also added in the modified version of the manuscript. Please see line no. line no. 253-265, 286-288, 291-294 in the revised version of the manuscript.

Minor comments:

1. Line 61: should remove ‘its’

   Changes incorporated.

2. Line 250–253: It might be better to move the literature summaries to the introduction section.

   Changes incorporated. Please see line no. 125-133 in the revised version of the manuscript.

3. Line 255: The precipitation changes during monsoon over India are not shown in the figures.

   The changes in the precipitation over India (on a global map) during the monsoon season (JJAS) were shown in supplementary Figures S1.

4. Line 270–280: Most of the discussions have been covered in previous paragraphs about Fig. 1.

   Here we quantify some extreme responses amongst the models.

5. Line 363–364: The argument about heavier air seems wrong. It is mainly due to the lower water vapor capacity and weaker upwelling.

   Changes incorporated. We have included this point in the modified version of the manuscript. Please see line no. 441-443 in the revised version of the manuscript.

6. Line 366–368: The temperature changes in Fig 7f are rather small, which means all the discussions here are unconvincing before the significance test.

   There was some issue in plotting. Earlier there were some positive temperature anomalies in Figure 7f (now 9f). We have rechecked and plotted again. The smaller values ranging from -0.01 to 0.01 are now put in white based on the also second reviewer's suggestion.

7. Line 367: The downdrafts should be more related to circulation shifts than tropospheric cooling. Please double check

   We have included this point. There is also weakening in the low-level circulations (Figure 11f) which along with the tropospheric cooling inhibits the convection. Changes implemented in the revised version of the manuscript (line no. 453-455).

8. Line 462–464: Unclear. Please rewrite the sentence.

   Changes incorporated. Please see line no. 523-525 in the revised version of the manuscript.