Reviewer #2

General Comments

The paper uses a promising framework to study the ESM-estimated effect of different climate forcers on the Indian Monsoon using the PDRMIP dataset. However, the current paper unfortunately spends far too long describing general effects, which are already well-known, devoting insufficient space to what could be a useful investigation into the specific response of the Indian Monsoon.

In addition, the framework for analysing the precipitation response is incomplete, and doesn't utilise previous work on the topic, again preventing a deep dive into novel grounds.

The paper could, with some substantial changes and additions, present a useful analysis of the response of a key climate phenomenon to anthropogenic emissions changes. These should be carried out before a further review can be done. In particular, the work has to be situated amongst the existing literature and present a new avenue of research.

The first few pages of the results present the global and spatial impacts of CO_2, BC, and Sulfate, which are already well-documented elsewhere including using this dataset. Some standard, intuitive effects – such as the effect of global perturbations being larger than that of regional ones – are noted without discussing that these are consistent. This is reflected in the conclusion section, in which the first 2 points represent well-known effects, without noting this.

The approach to modelling the energy budget is convoluted. The initial focus on dry energy leads to conclusions on the linearity between precipitation and dry energy on the global scale, without noting that this has been documented in prior work (see specific references below). Local discrepancies from linearity over India are then explored without explicit reference to the change in horizontal flow that they represent.

I would suggest that this paper be re-oriented to focus specifically on the Indian Monsoon, with little need for global analysis. With this, and a much-reduced focus on previously-documented results, more space would then be available for devotion to a deeper dive into, and discussion of, the effects of these forcing agents on the Indian Monsoon. This should include further analysis of the ITCZ and horizontal energy transfer, and more extensive situation of the results within the wider literature.

Focus should be given to error analysis; currently only multi-model means are presented in most cases, with no quantification of R squared values for linear fits, and no discussion of internal variability in the annual cycles in Figure 6. One issue which needs exploration is the differing sets of models which undertook each experiment. Many conclusions are drawn from Sulfur reduction experiments with 1 or 2 models; these merit explicit comparison to the base experiments from those same model sets, and analysis of inter-annual variability when only 1 or 2 models are available.

We first thank the reviewer for the constructive comments and suggestions. We agree with the reviewer's observation that certain findings have been previously documented on a global scale using PDRMIP. Nevertheless, our intention was to maintain a linkage between global and regional responses to greenhouse gas (GHG) and aerosol forcings to facilitate a comparative analysis, which constitutes the primary objective of this paper.

While acknowledging the suggestion to focus exclusively on the Indian monsoon, we have introduced a new subsection 3.3 dedicated to this topic. Within this subsection, we explored the responses of the Indian monsoon to aerosols (including CO_2) from a dynamic perspective.

During the Indian monsoon (June to September) period, we conducted a comprehensive analysis, encompassing changes in wind circulations at 850 hPa, vertical alterations in air temperature and meridional circulations. Figures 9, 10, and 11 (formerly 7, 8, and 9) specifically pertain to the monsoon season over India. Additionally, we incorporated changes in zonal mean precipitation during the monsoon period to emphasize substantial precipitation variations occurring in certain perturbed experiments (Figure 4b). This provides information about the ITCZ migration. Supplementary Figures S1 and S2 present precipitation and temperature responses on a global map during the monsoon season. The annual cycle of precipitation, along with temperature gradient analyses, offers valuable insights into the Indian monsoon precipitation in all the models (Figure 8, S4, and S5).

We have included discussion on energy flow in the section 3.2, where we see relationship between the dry energy budget and precipitation at both global and Indian region.

We have excluded conclusion no. 1. However, we have modified conclusion no. 2 (now conclusion no. 1) because our emphasis lies in unique energy metrics, differing from those explored in prior papers that utilized PDRMIP datasets. We have shortened some of the initial results on the global and spatial impacts of CO_2, BC, and Sulfate in the section 3.1 and modified the abstract and introduction.

We have addressed all the reviewer comments (elaborated below) and clarified the concerns. Please find below point-wise responses in light blue italics to the comments.

Specific Comments

1. L62 "The GHGs have significantly warmed the climate by 1.5°C" – this is the level that would have occurred in the absence of Aerosols, right? Present-day warming is closer to 1C.

Thank you for the correction. We have modified it in the text (line no. 67).

2. L190: what type of interpolation did you apply to each variable?

   The interpolation of data was performed using the distance-weighted average remapping method. This method utilizes the four nearest neighbor values for each grid cell from the original field. Please see line no. 201-204 in the revised version of the manuscript.

3. This part warrants expansion, with reference to other studies which have looked at this. PDRMIP was used by Xie et al 2022 (in the reference list but not referred to in the text https://www.nature.com/articles/s43247-022-00660-x) and in https://acp.copernicus.org/articles/20/11823/2020/ using a different energy budget analysis, and you need to be clearer on the novelty of your approach versus this. The motivation for focusing only on the coupled response should be made clear.

   Changes incorporated. Please see line no. 219-228. We have included these references in the methodology section.

4. L192: what is the motivation for focusing only on the dry component of the energy budget? Will this not miss the latent heat changes?

   In this paper, we focused on the dry energy budget as it is crucial for understanding and estimating precipitation changes. The dry energy budget involves the energy balance in the absence of phase changes and it includes processes (radiation, advection, vertical motions) that affect overall precipitation distribution. Since it explicitly excludes phase changes, understanding the dry component helps in assessing the availability of water vapor in the atmosphere. Please see line no. 219 to 228.

5. L225: the relationship between northern hemisphere cooling and ITCZ shifting warrants explanation, with references.

   We mentioned that the cooling in the northern continents inhibits the northward progression of ITCZ.

   For explanation, we have added a new Figure 4, which shows that in the sulx5 experiment, there is a decrease in precipitation in the northern latitudes annually as well as during monsoon season (JJAS). We have also added relevant references based on this result in the revised version of the manuscript. Please see line no. 253 to 262.

6. L231: "Reducing the sulfate aerosols enhances the surface warming as noticed in the Figure 2h, which can alter the climate sensitivity leading to various feedbacks" – need to be more careful with terminology; it reads like you're suggesting the Equilibrium Climate Sensitivity is altered by the sulfate response.

Thanks for the suggestion. We modified the text now (lines 271-273).

7. L234: it should be noted here that this consistency between sulred and sulx5 isn't surprising

We agree with the comment.

8. L241: I don't follow the logic of "global BC aerosols contribute more to precipitation increase than the Asian emitted BC aerosols". The global BC experiment contains the Asia perturbation, and additionally increases BC outside Asia; it's not surprising that this overall effect is larger. It would be surprising if the Indian precipitation change per change in BC emissions was larger for Global than Asian emissions, but I don't think this is what you're suggesting here.

We have now added a new Figure 3, which shows precipitation change per K (normalized with annual temperature change). From Figure 3b and 3d, it can be interpreted that the apparent hydrological sensitivity is higher over the Indian region in bc×10asia compared to bc×10.

9. L263 "surface temperature (Figure 1g)." – Figure 2g

Changes implemented (line no. 302).

10. L267: with the important exception of BC perturbations! It is well worth quantifying these by displaying the gradients and R squareds of linear fits for the Global and Indian regions to back up your point

We thank the reviewer for the suggestion. Changes have been now incorporated in the figures (5,6,7) and texts in the revised version of the manuscript. Please see line nos. 307-208, 309, 328, 351.

11. L272: you should give exact values for these numbers rather than approximations, to a consistent number of significant figures

We have now provided the exact values.

12. L278: this is consistent with the effects of BC as you've described – this should be noted here

Changes implemented in line no. 319-320 in the revised version of the manuscript.

13. L281: what do you mean by "synchronous"?

    We have rephrased the sentence.

14. L287: it's interesting but not surprising that the precipitation change is linear in the dry energy change on the global scale; using the formulation in e.g. Muller and O'Gorman 2011 (DOI: 10.1038/NCLIMATE1169), and used by Liu et al 2018 that you refer to, that latent heat gain equals losses to radiative cooling, sensible heat, and horizontal dry static energy flux, i.e. Lc*delP = delQ + delH; the dry heat is delQ, and delH is zero when averaged globally so you have delP/delQ = 1/Lc; a linear relation. You need to contextualise this section with prior results such as these, noting that Figure 4a is consistent with this. This does raise the question of what's happening in the GISS 10xBC experiment, as this is some distance from the linear fit.

    We thank the reviewer for the comment. We have now mentioned it in the section while discussing the relationship between the changes in precipitation and dry energy at the global scale in the revised version of the manuscript (line no.330-337). It is difficult to pinpoint the exact reason on why GISS (10×BC) is away from the linear fit. Perhaps, delH is non-zero.

15. Given this, Figure 4b's lack of linearity can be assumed to be due to a non-zero delH over the Indian region. E.g. on L307 it is noted that under sulx10asia precipitation decreases (negative Lc delP), and dry heat decreases (which represents an increase in the losses, i.e. positive delQ); this therefore must be compensated by a strongly negative delH i.e. horizontal energy flow into the region.

    See e.g. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019GL083479 and https://acp.copernicus.org/articles/21/10179/2021/ for more discussion on this kind of effect. Generally the balance between delQ and delH depends on the latitude, with a greater role for delH near the equator and delQ more so in the extratropics; so a mix seems sensible over this 10N-50N region you're using.

    We thank the reviewer for the comment. We have included this point and the references (line no. 355-359) in the revised version of the manuscript.

16. L317: the global 5a plot should probably be in the supplement; you note it should be zero in x anyway, and you can show it on the regional plot. The near-zero locations of different models aren't key to the results. I would use a different colour/style arrow on the regional plot to point to the global line, as they're pretty similar currently.

Changes were incorporated in Figure no.7 as per the reviewer's suggestion.

17. Figure 5 seems to be exploring the non-dry energy changes i.e. delH, though not framed in this way currently. Perhaps the vertical velocity change is related to the horizontal energy flow i.e. delH?

    It could be possible that vertical velocity change is related to the horizontal energy flow but we stick to only explaining in terms of vertical velocity changes. We had provided the changes in the wind circulations at 850 hPa over the Indian region in the Figure 11 where it can be seen that there increase in wind speed is associated with increase in precipitation in co2×2, bc×10, bc×10asia and sulred experiments. The increase in wind speed at 850 hPa is related to the strong updrafts over India (negative values in Figure 7).

18. L330: I'm not sure what this sentence means with "higher than that of global perturbation experiments" – 3 of the 4 experiments listed are global experiments.

    Changes redundant.

19. Figure 6: this is interesting but there are some issues with the analysis:

    There aren't any error bars so it's not possible to know if the differences are due to noise or a real effect. This is complicated by the fact that sulasiared only has 1 model and sulred has 2. This reflects a broader issue; we aren't really comparing like-with-like when we compare different sets of models performing different experiments. Obviously, you would throw a lot of information away if you used only the one model which has done sulasiared, but the claimed effects arise from only the models which ran each the related experiment, so there has to be a direct comparison between the experiments within just these smaller model subsets, at least in the supplement to verify the effect manifests in this case.

    We agree with the reviewer. This was there in our mind while carrying this analysis. There will be a disparity while carrying out ensemble analysis as all the participating models did not carry out all the experiments to do one to one comparison. Only MIROC-SPRINTARS model had performed all the experiments and we did not want to restrict our analysis considering only one model. We therefore provide annual cycle of precipitation for all the experiments carried out by individual models in the supplementary section. In Figure S4a (MIROC-SPRINTARS), the precipitation in sulasiared as well as sulred is higher during June-July than the base, therefore an increase in precipitation is noticed during the monsoon season.

20. It seems odd that the precip cycle in sulasiared increases from May, whereas the sulred experiment features increases from July only, while both see similar

responses in the temperature gradient to which these changes are attributed. This warrants exploration, and might suggest that there is a large role for internal variability, or other drivers, here. Similarly, it is interesting that the sulx10Asia and sulx5 experiments see little response in the temperature gradient, but large reductions in precip. This strong nonlinearity needs explaining and again suggests perhaps the temperature gradient isn't the only driver here. You discuss this possibility in relation to other experiments which is useful but overall this needs expansion.

In the MIROC-SPRINTARS model, the precipitation mostly increases from May including sulasiared (Figure S4a) and in the HadGEM3 model (that had sulred), the precipitation increases in July (Figure S4e). Since the magnitude of precipitation is higher in HadGEM3 compared to MIROC-SPRINTARS, the ensemble is towards HadGEM3. This indicates a larger role in the internal variability in the individual models. We have added this point in the manuscript. In the case of sul×10asia (7 models) and sul×5 (9 models) experiments, the temperature gradient is almost similar except in the months of April and May where a slightly higher gradient in sul×10asia is noticed compared to sul×5 but still less precipitation in June-July monsoon months compared to sul×5. The decrease in precipitation in sul×10asia is more across all the models (7 models) that performed these two experiments (Figure S4). This suggests that there are other possible drivers of precipitation. This is because not only the surface temperature affects the dynamics but also the atmospheric heating profiles determine the circulations and moisture transport pathways leading to changes in the precipitation over India. Please see line no. 411 to 427 in the revised version of the manuscript.

21. L342-343: not clear what this sentence is saying.

    Changes redundant.

22. L350: you should clarify this is averaged zonally across Asia in this sentence

    We have now clarified in the sentence (line no.428-429).

23. Figure 7: this is interesting, but there is no scale for the circulation changes. The apparent warming regions under increased SO2 are odd – maybe there's essentially zero change here but the mean happens to be >0? Would suggest making the areas white if the magnitude of the change is below some threshold say 0.1K as the smallest contour is currently 0.4K.

    We have modified the figure according to the reviewer's suggestion. Please see Figure 9 in the revised version of the manuscript.

24. L377: it's good to note the ITCZ but this effect isn't explored further in this way. This could be a good avenue for digging further into the response.

We have added a new Figure 4 showing zonal mean of annual precipitation change and JJAS precipitation change indicating the ITCZ movement (line no. 253-265, 286-288, 291-294)

25. L801 "Table 1: Description of the 11 models used from the Precipitation Driver Model" – missing "Response"

Thanks for the correction. Changes implemented in the revised version of the manuscript (line no. 924).

26. In the abstract you say you present "potential underlying physical processes under a variety of climate forcings that would be useful in designing further model experiments with higher spatial resolution" – what do you mean by this? It isn't elaborated further in the main text.

The models used in this study have coarser resolution. The small-scale processes may not be well represented in these models. The responses attained in the perturbed experiments may vary in terms of their magnitude if higher resolution simulations are used. There are ongoing projects that aims to provide simulations at higher spatial resolution (~3-5 km). This advancement will facilitate future comparative analyses (responses) in similar experiment designs.

We have added this point in the main text. Please see line no. 544 to 550 in the revised version of the manuscript.