Response to reviewer 3

Format of responses: (1) comments from referees/public, (2) author's response, and (3) author's changes in the manuscript."

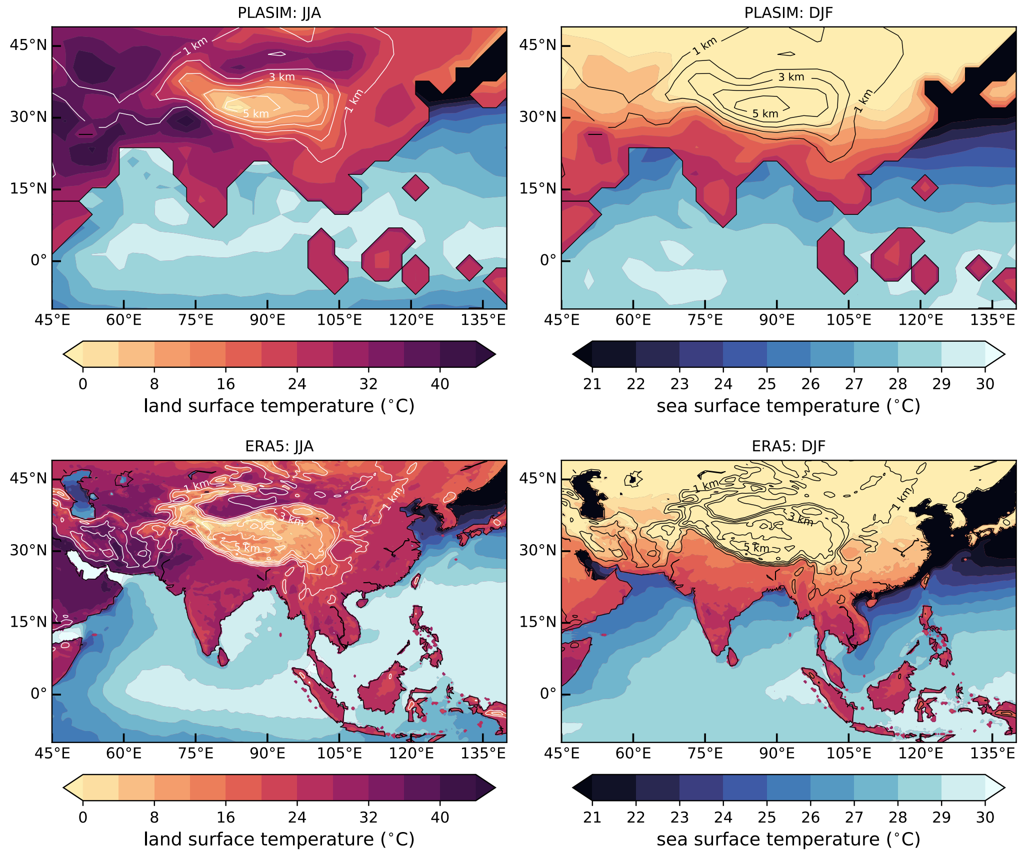
General comments:

(1) The manuscript describes the use of an intermediate complexity model to perform sensitivity experiments to prescribed aerosols and CO2 forcing over Asia, in combination as well as separately, also distinguishing between sub-regional forcing and identifying non-linearities.

The topic has been widely studied using a variety of modelling tools and simulations. Needless to say, there are still uncertainties and varied responses across models. It is a very important topic as aerosols are still very high over the region studies, and there may be large differences in the spatio-temporal evolution of regional emissions in the coming decades. The manuscript is well written and clear. I have some concerns on some of the experimental set-up and analysis that need to be addressed before acceptance.
(2) We thank the reviewer for their comments and for taking the time to review the manuscript.

Major comments:

(1) The model validation should be done by comparing the output to observations.
(2) We agree with the reviewer that this should have been discussed in greater detail. Comparison to observations was done at an early stage but not all figures/analysis were included in the manuscript due to concerns over length. Given the idealistic nature of the experiments, extensive quantification of performance against, for example, satellite data, isn't applicable. Our requirements are that PLASIM reproduces the large-scale features of the monsoons, with a clear seasonal cycle, which is hopefully now evident.
(3) We have added subplots to Figures 1-4 to show the equivalent variables from ERA5 reanalysis data, to allow for visual comparison with the PLASIM model. Updated "*Section 1.2 Model Validation*" to reference and discuss the updated figures. Example of revised Figure 1 below.



(1) The magnitude of radiative forcing is definitely too large. Available estimates provide values up to 20/30 W m2 at the surface, I think it is generally OK to use larger values than observed in idealised settings (e.g., PDRMIP) to highlight the signal, but the values used here are definitely too large. I think these simulations are unrealistic.

(2) We have included additional figures for 30W/m^2 forcing as well as the 60W/m^2 forcing, to help quantify the linearity. Nonetheless, we remark that locally, in heavily polluted urban conglomerate and industrial regions, forcings of around 100 W/m^2 have been observed – see references in Section 2.2. In particular, see Table 2 in Kumar and Devara (2012), where values of -46 to -110/+46 to 115 W/m^2 are quoted for surface/atmosphere forcing in Delhi. As mentioned in the manuscript, the point of including unrealistically high forcings is to cover a parametric range of forcings. In particular, to see the behaviour leading up to a breakdown or severe weakening of the monsoon systems.

We use an intermediate complexity climate model to help bridge the gap between conceptual (e.g. box) and advanced climate models (i.e. CMIP6 standard). For our simulations, we do not require the same level of realism that CMIP6 standard models aim to provide, because we are not trying to predict a future climate state or contribute to policy guidance on climate change. The benefits of using an intermediate rather than an advanced complexity climate model are highlighted in Section 2. For future work, it is hoped that similar experiments with a gradually varying forcing might be performed with a higher complexity model, such as the WRF model.

(3) We have added Figures in Section 5.1 to include results from simulations with an absorbing aerosol forcing of 30 W/m^2. Additional sentence added to Conclusions: "Our results are limited by the low resolution and lack of explicit aerosol interactions and chemistry, but future work will aim to address these issues by repeating similar experiments using a more complex global climate model."

(1) Along these lines, I am not sure the comparison with 2xCO2 is appropriate, and especially the identification of which of the two drivers dominates.
(2) At present, 2xCO_2 is in some sense unrealistic; we are fortunately still far from a 720 ppm CO@ global CO_2 concentration. However, there are many modelling studies that use the standard IPCC forcing scenarios, which include extreme storylines. Indeed, it is typically the most extreme scenarios (i.e. RCP8.5/SSP5-8.5) that are most often represented in the literature. From IPCC6, two out of the four climate scenarios result in double or greater levels of carbon dioxide levels by 2100, making 2xCO_2 a reasonable choice when considering future greenhouse gas forcing in high to extreme scenarios. The relative dominance of greenhouse gas and aerosol forcings in the actual future scenarios depends on the path of change of such forcings, which is associated with a high degree of uncertainty. The goal is to compare a reference climatic forcing (2xCO_2) with different scenarios of aerosol forcing.

The 2xCO_2 simulations are performed with absorbing aerosol forcing from 0 to 150W/m^2, so can we consider the impact of doubling carbon dioxide levels in tandem with a range of different intensity absorbing aerosol forcing. We find a fair degree of linearity in the response to aerosol forcings of 30, 60 & 90W/m^2 with 2xCO_2, which can be seen by the similarity in left to right columns of Figures 9 & 10. In our experiments, we find that the system is more sensitive to absorbing aerosol than carbon dioxide forcing.
(3) As per previous comment, we have added Figures to show the sensitivity to both 60 W/m^2 and 30 W/m^2 absorbing aerosol forcing. Added sentences to the Abstract ("These results suggest that in the future, the anticipated reduction in aerosol concentration may have a greater impact on monsoonal precipitation than the increase in greenhouse gases.") and Conclusions ("We find that the precipitation response is more sensitive to absorbing aerosol than greenhouse gas forcing, highlighting the importance of air quality policies and the impact they can have on the future state of the South and East Asian monsoons.")

(1) Also, the prescribed forcing simulates the effect of absorbing aerosols, rather than sulfate. In reality, the latter are found to dominate the aerosol-driven monsoon changes. I think it is important to further underscore these differences and make it clearer when drawing conclusions.
(2) We agree with the reviewer (and with reviewer 1) that this needs to be clarified. The manuscript has been revised to be clear that the forcing used is representative of absorbing aerosol effects.

Scattering and absorbing aerosols and their impact on the South and East Asian monsoons are discussed in the Introduction. Although several articles suggest that sulphates are the dominant aerosol species in terms of the effect on monsoonal rainfall, others suggest that black carbon has the greater effect. There remains much debate and considerable uncertainty on the effects of different aerosol species and their effect on the vertical profile of the atmosphere. The location and spatial pattern of aerosol loading is also important, and can have contrasting effects on precipitation in the local region compared to a remote region. One of our aims for future work is to repeat the experiments with a sulphate-style forcing, following Herbert et el. (2022), to compare the responses of the monsoons to different aerosol forcings.
(3) Changed various instances of "aerosols" to "absorbing aerosols".

(1) As there is not seasonality in the forcing here, can the authors speculate on what this means in terms of realism of summer anomalies, also in terms of preconditioned conditions through the previous winter and spring?
(2) PLASIM's ability to represent long-term (monthly and greater) correlations in the soil properties is limited, because of the simplified nature of the land module. Hence, we expect that whether we use a perennial aerosol forcing or an aerosol forcing that is active each year for a period that includes the summer season, the June-July-August cumulative precipitation should be approximately

the same. We plan to investigate this more in detail in the forthcoming study in collaboration with the Herbert et al. 2022 authors.
(3) Added a sentence to the conclusions regarding our intentions to investigate this matter further: “we will consider the impact of scattering aerosol forcing compared with absorbing aerosol forcing, and the effect of applying forcing over a shorter seasonal period, rather than perennially.”

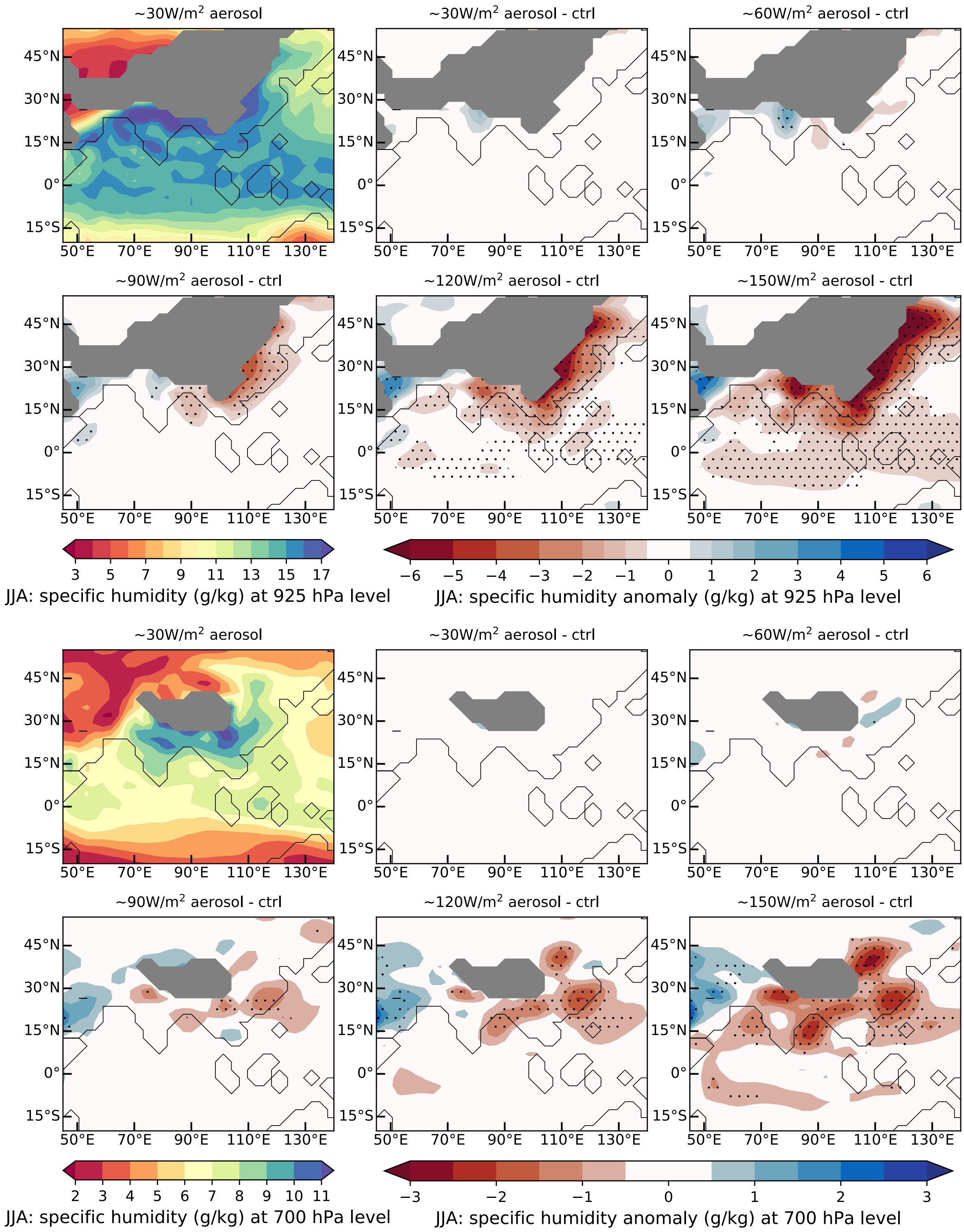
(1) I suggest also making the text a bit more concise, as I think some parts are not needed. For example, all the section on tipping points in the introduction is not really necessary. Similarly, Section 4.1 is not contribution much to the overall discussion. There are also other parts throughout the manuscript that can be shortened.
(2) The Indian monsoon has long been considered a tipping element and one of our aims was to establish the level of forcing required to cause a transition or breakdown of the monsoon system, such that the summer precipitation becomes very low. It seems relevant to put our results and the problem of the response of the system to perturbations in the context of tipping points and of the literature of safe operating spaces.

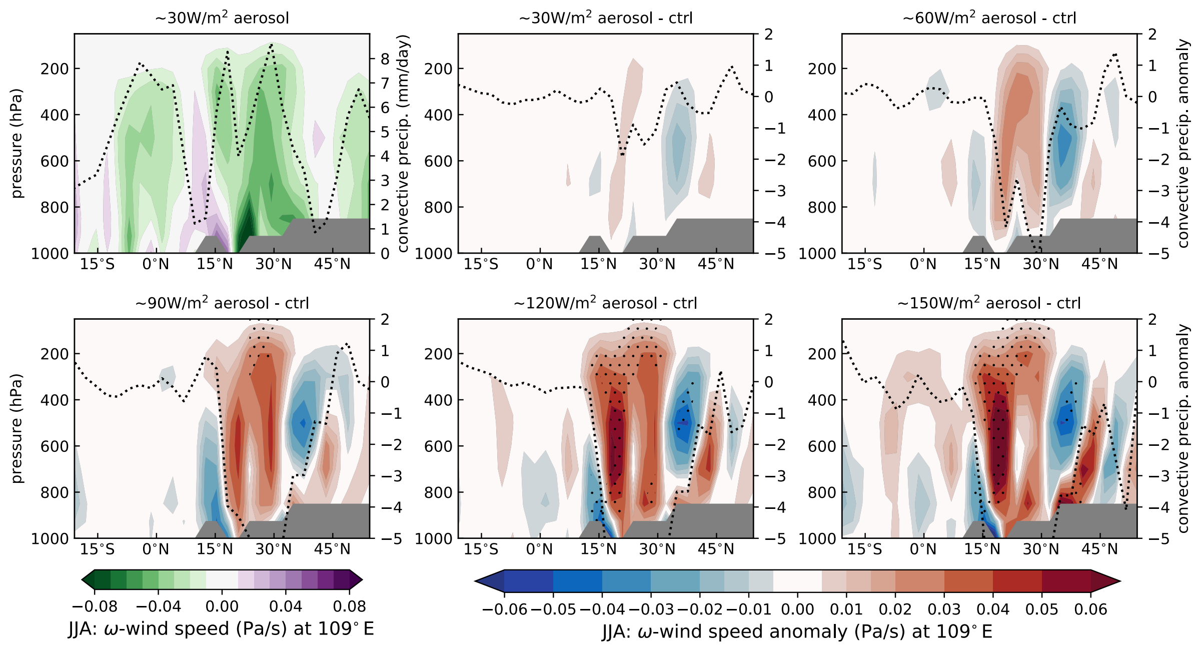
We have tried hard to make the manuscript as compact as possible and we will make a further effort in the revised version, without risking the loss of key information relating to justification of methods and analysis of results.
(3) Section 4.1 has been modified to include some more quantitative analysis, following the comments of reviewer 1, increasing the value of this section.

(1) Why is relative humidity used instead of specific humidity? I do not think RH at 200 hPa (Fig. 3) is really useful. The way winds are plotted is rather unusual (scale the arrows by the magnitude instead of plotting magnitude and direction separately). Also, it may be useful to calculate vertical integrated moisture transport.
(2) At 850 hPa, relative humidity seems relevant because we wish to understand where we are closer to saturation, and hence closer to conditions conducive to convective precipitation. At 200 hPa, where the temperature is extremely low, specific humidity is virtually zero by the Clausius-Clapeyron relationship. Thus, in our opinion, plotting specific humidity at 200 hPa provides very little information. However, we agree that specific humidity is an important variable to consider and we have added a figure to show the specific humidity at 925 and 700 hPa.

For Figures 2 & 3, wind vectors have been scaled by magnitude, to allow for plotting over coloured contours of relative humidity. This reduces the number of overall figures and shortens the manuscript, without losing visual representation of key variables. In Figure 7, the wind speed and direction are plotted separately, so that we can more clearly focus on the changes in wind speed.

With the additional figures of specific humidity and vertical velocity (following reviewer suggestions), we can infer moisture advection horizontally and vertically. With concerns over length, we feel that another figure to show vertically integrated moisture transport is superfluous.
(3) Added extra figures of specific humidity (shown below) and vertical velocity (omega), and updated the text to include analysis of added figures. Example vertical cross section of vertical velocity at 109°E shown below, with the dotted line showing convective precipitation.


~30W/m² aerosol
~30W/m² aerosol - ctrl
~60W/m² aerosol - ctrl
~90W/m² aerosol - ctrl
~120W/m² aerosol - ctrl
~150W/m² aerosol - ctrl
45°N
30°N
15°N
0°
15°S
50°E 70°E 90°E 110°E 130°E
3 5 7 9 11 13 15 17
JJA: specific humidity (g/kg) at 925 hPa level
−6 −5 −4 −3 −2 −1 0 1 2 3 4 5 6
JJA: specific humidity anomaly (g/kg) at 925 hPa level
~30W/m² aerosol
~30W/m² aerosol - ctrl
~60W/m² aerosol - ctrl
~90W/m² aerosol - ctrl
~120W/m² aerosol - ctrl
~150W/m² aerosol - ctrl
2 3 4 5 6 7 8 9 10 11
JJA: specific humidity (g/kg) at 700 hPa level
−3 −2 −1 0 1 2 3
JJA: specific humidity anomaly (g/kg) at 700 hPa level



(1) Differences should have plotted a corresponding statistical significance.

(2) We agree with the reviewer. We have updated the figures to show stippling where the changes are statistically significant. Here, statistical significance is determined by where the anomaly (e.g. 60W/m^2 forcing – control) is greater than 2 times the interannual variability of the June-July-August mean, with respect to the reference simulation (e.g. 60W/m^2 forcing).

(3) Figures have been updated with stippling to show statistically significant changes. In conjunction, the text has edited to include discussion of the statistical relevance of the anomaly signals. Example of figures with added stippling can be seen in response to some of the comments above.

(1) The analysis of regional responses and the reciprocal influence between India and China is, in my opinion, the most interesting part. A dynamical analysis of the upper level circulation will also be useful.

(2) The fact that the presence of an aerosol forcing over East China leads to an increase of precipitation over Northern India is an interesting phenomenon. Partly comparable results had been found in Herbert et al. 2022. Understanding this process is highly nontrivial and we are collaborating with the Herbert et al. team exactly to discover the mechanisms in action.

Section 3 includes some analysis regarding the upper level circulation, but we have extended this to include discussion of vertical velocity and specific humidity, so that horizontal and vertical transport of moisture can be considered in a holistic way.

(3) Analysis of results in Sections 3 & 4 has been extended to include discussion of specific humidity and vertical velocity. Added stippling to figures (see revised Figure 11 below) and modified a sentence in Section 5: "The precipitation response of India to forcing applied over East China is nearly as strong as when the forcing is applied locally, albeit with opposing trends. Similar asymmetry in the teleconnection between East China and India in relation to local absorbing aerosol forcing has been shown in Herbert et al. (2022); however, further is required to fully understand the underlying mechanisms."

(1) What are the limitation of this study and in particular of using PLASIM? This should be clearly stated in the conclusions.

(2) We agree with the reviewer that it is important to consider the caveats of using an idealised modelling scenario, which we have discussed in detail in Section 2.2. We have also added a sentence to the Conclusions (see below).
(3) Following this comment and comment by reviewer 1, we have added a sentence to the Conclusions to highlight the limitations of our study, and our future work aims: "Our results are limited by the low resolution and lack of explicit aerosol interactions and chemistry, but future work will aim to address these issues by repeating similar experiments using a more complex global climate model."