Reply to Reviewer #1

General comments:

Guo et al., 2012 discuss about the role of anthropogenic aerosols on regional rainfall over Asia using an atmospheric general circulation model. The focus is on understanding the influence of sulfate and BC aerosols, separately, on the precipitation over East Asia. The manuscript concludes that cooler surface temperature due to enhanced sulfate aerosols decreased precipitation via changing EASM circulation during September. The paper is generally well written and the effects of anthropogenic aerosols on precipitation during East Asian summer monsoon season are quite interesting. There are certain points about the parameterization for stratiform and convective clouds and precipitation, which are not clear in the manuscript. These points should be made clearly to help the reader to put the paper in context with the existing literature (e.g. papers suggesting anthropogenic aerosols may either increase or decrease precipitation). The paper should be published after addressing the following questions.

We thank the reviewer for their evaluation and are pleased that they found it well written and interesting.

Specific comments:

1) The manuscript does not provide detail about how the precipitation (stratiform and convective) is parameterized in the model. This information would be useful to the reader in the interpretation of the results.

We have added a paragraph describing the precipitation parameterization in Section 2.1.1 as follows:

"*The precipitation in HiGAM is parameterized separately as large-scale precipitation and convective precipitation. The parameterization of large-scale precipitation and large-scale cloud are as described by Smith (1990). The cloud water content and cloud amount are calculated from specific total water content and the saturated specific humidity. The precipitation is depleted from cloud water as soon as cloud water is condensed. This process is represented differently for liquid clouds and ice clouds. The auto-conversion rate of cloud water to rain water in liquid clouds is linked to the cloud condensation nuclei (CCN). However, since less CCN act as ice nuclei (IN), precipitation from ice clouds is not linked directly to IN. The convective scheme is based on the mass flux scheme originally developed by Gregory and Rowntree (1990) with a modification to explicitly couple it to the boundary layer scheme. Unlike large-scale precipitation, in HiGAM, the aerosol cannot change convective precipitation microphysically.*"

2) The manuscript does not provide information about the optical properties assigned to sulfate and BC in the model. The authors should describe the treatment of refractive index and size distribution (Mie calculation) used for sulfate and BC in both experiments. Also need to mention about the single scattering albedo used in the radiative transfer calculations. It could be useful if the authors add one discussion about how the sulfate and BC AOD differ in both experiments. Are there uncertainties that might influence the results?

The extra information suggested by the reviewer has been added partially in the paper (P23013, L7-20) and partially as supplement 1. Therefore, the text (P23013, L7-20) has been modified as follows:

"The sulphate scheme is described by Jones et al. (2001) and Woodage et al. (2003). The precursor gases (SO_2) and dimethyl sulphate ($(CH_3)_2SO_4$) undergo chemical reactions with oxidants in the atmosphere to form sulphate aerosols in both gaseous and aqueous phases. In HiGAM, sulphate aerosols are treated as hydrophilic and can therefore act as cloud condensation nuclei (CCN). Thus a change in sulphate aerosols can lead to changes in cloud reflectivity and lifetime through changes in the cloud droplet number concentration (CDNC). Sulphate aerosols are assumed to be in one of three different size modes: Aitken mode (median radius $r_{Ait}=24X10^{-9}$ m and geometric standard deviation $\sigma_{Ait}=1.45$), accumulation mode ($r_{acc}=95X10^{-9}$ m and $\sigma=1.4$) and dissolved mode (sulphate dissolved in cloud water droplets). The optical properties (single scattering albedo, mass extinction coefficient and asymmetric function) of the Aitken mode and accumulation mode vary with relative humidity, whilst there are no separately defined optical properties for dissolved mode as it is part of cloud water droplet. The optical properties as a function of wavelength are shown as Figure S1, S2 and S3 in the supplemental information. The black carbon (BC) scheme (Roberts and Jones, 2004) is different in that BC is assumed emitted as primary particles. BC emissions from surface are initially described as a "fresh mode". Two other two black carbon modes exist in the model, "aged mode" and "dissolved black carbon" in cloud droplets. Both the fresh mode and aged mode are assumed to have the same lognormal size distributions, with median radius $r=40X10^{-9}$m and geometric standard deviation $\sigma=2.0$. The fresh mode is however, hydrophobic whilst the aged mode has optical properties

representative of an internal mixture of black carbon particles with hydrophilic material. The BC is treated as insufficiently soluble to act as CCN in the model. Therefore, the optical properties of BC are not sensitive to relative humidity. Finally, some aged black carbon becomes internally mixed with cloud water, forming dissolved black carbon in cloud droplets. The optical properties of black carbon used in this paper are shown in Figure S4 in the supplement."

3) The model-simulated precipitation (JJA) patterns look too different compared to the GPCP patterns (Figure 2), especially over the Indian region. There is a possibility that the model could appear to give a precipitation response for the wrong reasons. Could the authors address this more carefully?

We agree with the reviewer that there are some issues with the precipitation pattern over India. The precipitation deficit over India is in fact a common bias in many climate models. The bias has been improved in CMIP5 models compared to CMIP3 models, partly due to enhanced horizontal resolution, but it still exists (Sperber et al, 2012). However, despite this bias, the model can still be used to examine the processes that lead to changes in the climate of the model. To address the reviewers concerns we have added an extra paragraph comparing the seasonal cycle of precipitation in HiGAM and GPCP. Therefore, we have rewritten section 2.3 as follows:

"*Figures 2a, 2b and 2c compare the global JJA precipitation in HiGAM with GPCP data. The 850hPa geopotential height and wind over East Asia are shown in Figures 2d 2e and 2f. HiGAM captures the global pattern of precipitation, and compared to models with lower horizontal resolution enhances the precipitation over the coast the maritime*

continent and the southern slope of the Himalaya.. However, over East Asia, the Meiyu/Baiyu/Changma front is weaker in HiGAM than in the observations, with an deficit also over India. Both these biases also exist in most of the CMIP3 and CMIP5 model participants (Sperber, et al. 2012). The weak Meiyu/Baiyu/Changma front is a result of a weak subtropical high over the western North Pacific Ocean. The excessive rainfall in the maritime continent and the deficit in precipitation over India may be related to a circulation that is too zonal across the Asian monsoon domain. However, HiGAM captures the most prominent features of the EASM, the Somali jet over the northern Indian Ocean and the subtropical anticyclone over the western North Pacific Ocean.

Figure 3 compares the annual cycle of precipitation averaged between longitude (107°-122° E). One key characteristic of the EASM is the asymmetric propagation of monsoon precipitation with an abrupt monsoon onset at the end of the May and a gradual withdrawal to the end of September (Hung et al., 2004). As shown in Figure 3, this asymmetry is well captured in HiGAM, except that the precipitation is stronger. In addition there is persistent precipitation over the southern China (between 20°-30°N) from February to May in both HiGAM and GPCP data. Tian and Yasunari (1992) refer to this as the Spring Persistent Rains over central China. The Spring Persistent Rains are caused by the interaction between low level flow and the orography over eastern China. Due to HiGAM's high horizontal resolution, the Spring Persistent Rains have been well captured compared to other models. Thus we believe that local and regional physical mechanisms

are represented well in HIGAM although there may be some biases in the larger scale circulation pattern. Since we will be changing emissions on a regional basis, this model can be used, the biases not withstanding."

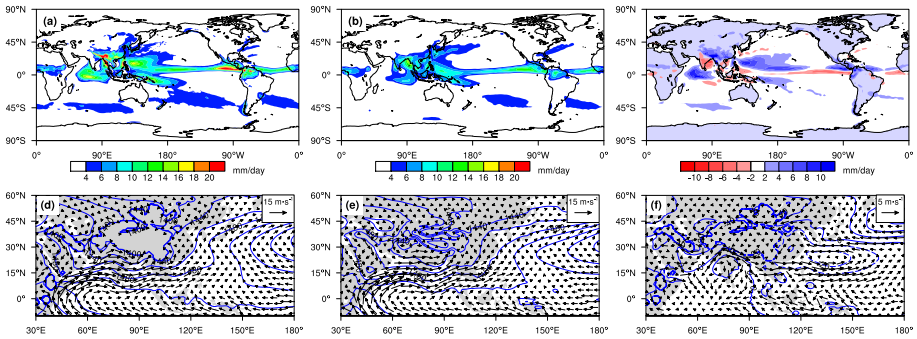

Fig. 2. (a) JJA precipitation in HiGAM (1983-2000, units: mm/day). (b) JJA precipitation from GPCP 2.5×2.5 monthly data (1979-2010, units: mm/day) (c) JJA 850 hPa geopotential height (contour, units: gpm) and wind (vector, units: m/s) in HiGMA (1983-2000) (d) Same as (c) but from ERA-40 (1958-2002).

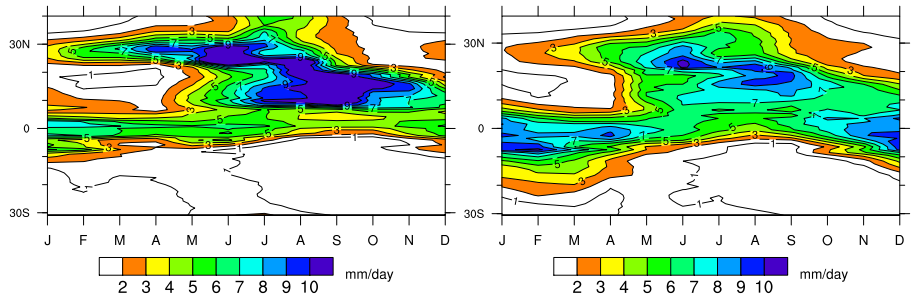

Fig. 3. Annual cycle of precipitation averaged between 107°-122°E. Units: mm·day^{-1}. (a) HiGAM and (b) GPCP.

4) The manuscript concludes that the precipitation is reduced by the increased anthropogenic aerosol emissions during September would this conclusion change if the all anthropogenic aerosols (BC, OC and sulfate) together considered?

This is an interesting question. OC is not represented in the model, apart from being one component of "biomass burning" aerosol. The biomass burning aerosols are mainly emitted from Southeast Asia, South America and Africa (As shown in Figure R1.). Therefore, their local impact over the East Asia region is expected to be small compared to

that of the local emissions of sulphate and BC. Secondly, increases in both sulphate and BC aerosol cause a decrease in precipitation, through a weakening of the land-sea surface temperature gradient. We therefore anticipate that the sign of any change seen would not change with the addition of OC.

We performed the experiments for sulphate and BC separately since the sign of the BC effect was not obvious at the design stage. In fact, since the tropospheric heating from BC can reduce the low level cloud, this would then reduce the impact of the indirect effect from increased sulphate and therefore the effect of combined changes in aerosol would possibly be smaller than seen here. The separate experiments therefore give a bigger perturbation to the system and a larger signal to noise ratio. We modified the text accordingly, and it is modified with the reply to technical comment 7. Please refer technical reply 7.

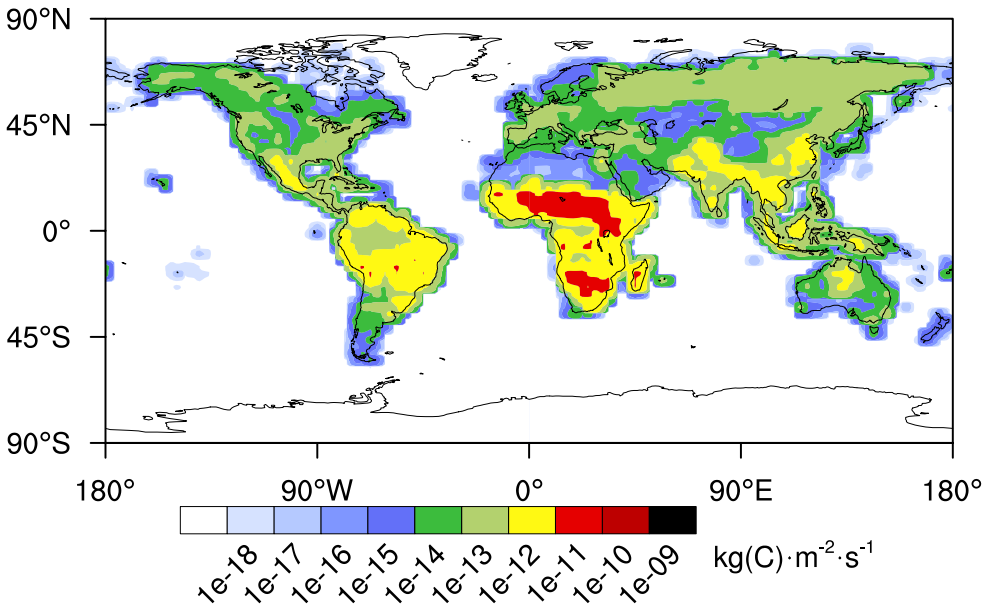

Figure R1. The annual mean emission of biomass burning aerosol used in HiGAM. Units: kg(C)$m^{-2}s^{-1}$.

5) There is no information about the data sets (e.g. GPCP, ERA-40) used in the study. The authors need to add one section describing about the datasets used to evaluate the model-simulated parameters.

We thank the reviewer for pointing out this important omission from the original manuscript. The information about data sets used in this study has been added in Section 2.3:

"*The monthly ERA-40 data (Uppala, et al. 2005) with resolution of 1.125X1.125 is used here. The Global Precipitation Climatology Project (GPCP) version 2 dataset is also used (Adler, et al. 2003). This provides is monthly data on a $2.5^\circ \times 2.5^\circ$ grid for the period of 1979-present.*"

6) Also, no information (figure) is provided about the vertical profiles of the aerosols (Page 23017, lines 11-16) - can the authors address this and how we may interpret these results relative to the 'Elevated Heat Pump' theory.

Since the vertical profile of aerosol is important for our discussion of the relevant mechanisms, we agree that a figure including the vertical profiles would be useful. This is now provided in the supplement, and a description has been added to the text (Page 23017, Line 11-14) as follows:

"*Since sources of anthropogenic emission are mainly confined to the surface, and the lifetimes of both sulphate and black carbon are short, regions of high concentrations are confined within 2km above the surface (see Figure S5 in supplement).*"

The reviewer asks that we discuss the interpretation of our results in the context of the "Elevated Heat Pump" (EHP). We would like to point out that the EHP is usually discussed in the context of the South Asian

monsoon and refers to the build up of absorbing aerosols over the south slope of the Tibetan Plateau, as shown as Figure 3 in Lau et al. (2006). Due to the elevation of the underlying topography, when compared with the atmosphere further south, the heating provided by this aerosol appears elevated, however in reality the aerosol loading itself is still confined to low levels above the ground. Thus we would argue that the concept of the EHP is not relevant to the East Asian monsoon which is discussed in this paper.

7) What is the influence of transported aerosols to the study region? Since Rest of the world emissions are kept constant at the 2000 level in all experiments (section 2.2).

Our study considers the impact of locally emitted aerosols on the East Asian monsoon. The region in which we alter emissions shows one of the largest increases in emissions globally over the period 1950-2000 and therefore we assume that these will dominate the changes in aerosol over this period compared to any changes in transported aerosol. However, aerosol changes in remote regions could play a role under certain circumstances. Firstly, if atmospheric circulation changes and emissions change in another part of the globe (most relevantly India) then a different amount of aerosol could be transported to the region – testing this is beyond the scope of the present study. Secondly aerosol changes in remote locations could cause a response in the East Asian region through teleconnections. We have examined the response to our changes in aerosol globally and see little signature anywhere other than in the region immediately surrounding the emissions change (See Figure R2). By extension therefore we suggest that the influence of

aerosol changes from other regions, were we to include them, would be small.

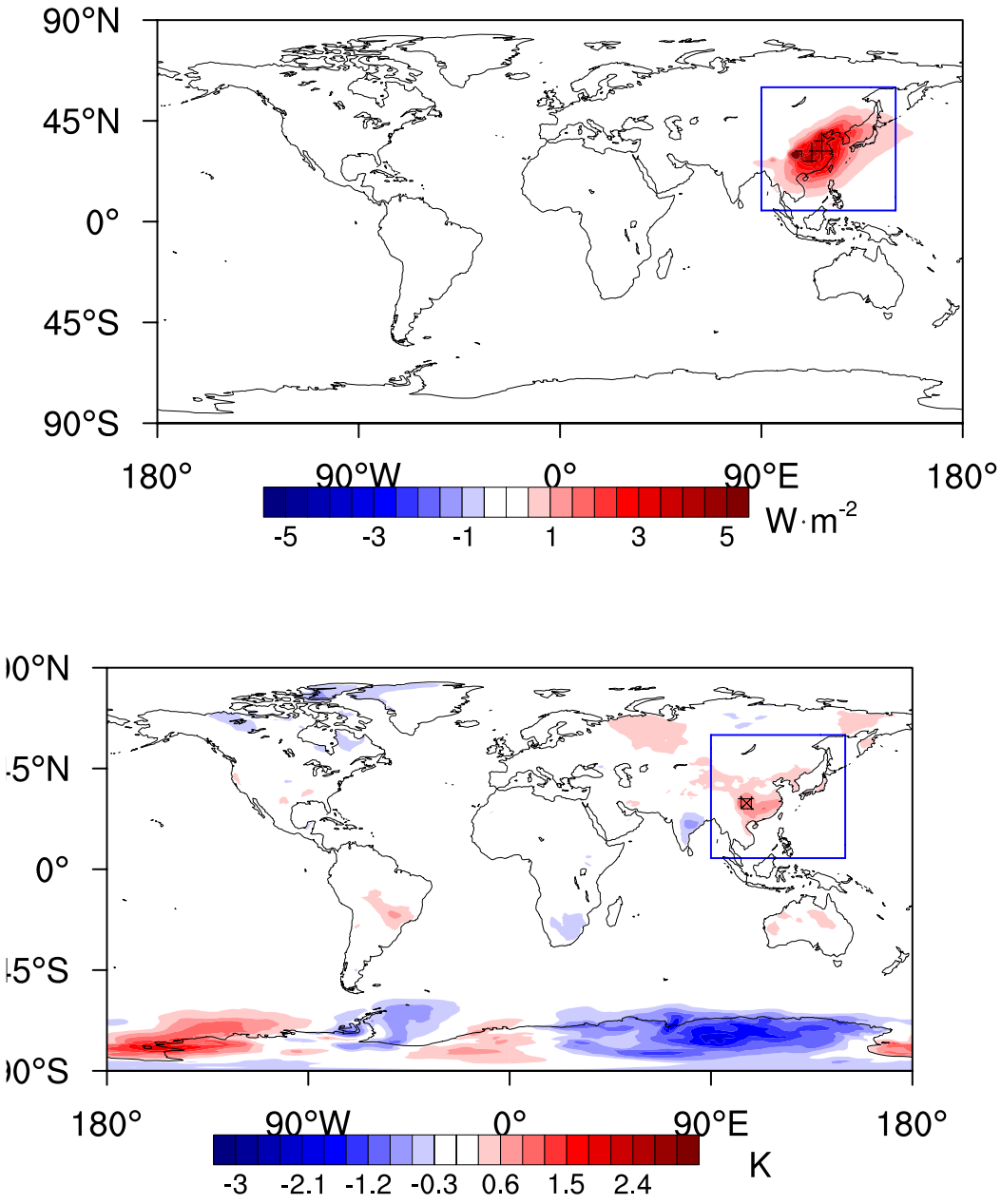

Figure R2. (Upper) The combined sulphate aerosol effect at the top of the atmosphere averaged over the experimental period. Units: Wm^{-2}. (Lower) Change of surface temperature (SO2_1950 minus Control) averaged over experimental period. Units: K. Changes excess 95%

significant test are hatched. The blue box indicate the experimental area where sulphur emissions are changed.

8) The authors should also address more specifically in the conclusion section about the improvements they have achieved from the present study in context with the existing literature

We thank the reviewer for this comment and have responded by rewriting the first paragraph of section 5 to highlight the advances made in this study, namely:

- **Process-based study of the impact of local aerosol changes on the East Asian summer monsoon region (other studies look at effect of global changes, or concentrate on the S. Asian monsoon) whereas we separate out direct and indirect effects and sulphate vs black carbon aerosols.**
- **Use of a high-resolution climate model – some of the features important for explaining the variability of the aerosol impact are not represented well in the coarser resolution models of previous studies.**

The new paragraph becomes:"*In this study, experiments using HiGAM with an interactive aerosol scheme show that the EASM in the model is altered when local emissions of either sulphate and black carbon aerosols are increased from 1950 to 2000 levels. Compared with previous studies, the intra-seasonal variation in monsoon features as a result of the aerosol changes has been looked at in more detail. It is found that the impacts of aerosols are more significant during the withdrawal phase of the EASM (September) rather than active phase (JJA). We also evaluated separately the direct, indirect and semi-direct*

***effects of aerosol thus this study is able to highlight the different mechanisms responsible for change in EASM depending on the type of aerosol considered*."**

Technical comments:

1) In abstract section Page 23008, Line 12: Quantify the precipitation change.

These numbers have been specified and the sentence now reads (modifications in italic):

'However, in September, precipitation decreases by 26.4% for sulphate aerosol and 14.6% for black carbon when their emissions are at the 2000 level.'

2) Page 23008, Lines 12-14: Why precipitation is decreased significantly in September? Whether both convective and stratiform precipitation is decreased? How the aerosol-radiative forcing different in September? The authors need to quantify how the surface cooling is different in September compared to other months?

This is the main message of the paper and we therefore welcome the opportunity to clarify our explanation. The section now reads:

Original: "However, in September, precipitation is significantly decreased when emissions are at the 2000 level. The cooler land surface temperature over China in September due to increased aerosols reduces the surface thermal contrast that supports the EASM circulation."

Modified: "*However, in September, as aerosol effects are stronger, the precipitation decrease is significant (decrease by 26.4% for sulphate and by 14.6 for BC). Over 80% of the decrease is attributed to changes in convective precipitation. The cooler land surface temperature over*

China in September (0.8C for sulphate and 0.5C for black carbon) due to increased aerosols reduces the surface thermal contrast that would otherwise support the EASM circulation."

3) Page 23010, Lines 3-5: What is meant by "some aerosols"? Is it BC? Rephrase the sentence.

We have rewritten the sentence to make it clear that the Asian Brown Cloud refers to both black carbon and organic carbon. The sentence now reads: *'The role of carbonaceous aerosols in absorbing short-wave radiation over South Asia and the north Indian Ocean (the Asian Blown Cloud, ABC) was identified by Ramanathan et al. (2001).*'

4) Page 23010, Lines 16-19: The sentence is not correct. All the cited papers showed enhanced rainfall mainly in the early summer monsoon season.

If our understanding of this comment is correct, the 'early summer monsoon season' mentioned by reviewer refers to JJA. In these previous studies, researchers tended to look at seasonal mean response of summer monsoon to aerosol impacts and the season most studies focused on is JJA. Our study however considers the evolution of the EASM month-by-month. To avoid confusion when comparing with our results, we have modified the sentence as follows:

"However, in atmosphere-only models, BC increases the precipitation over Indian subcontinent during summer monsoon season (JJA) because the ascent is intensified by atmospheric heating due to the increased BC concentration (Menon et al., 2002; Randles and Ramaswamy, 2008)."

5) Page 23011, Lines 5-7: The mechanism is not clear. Is it same as EHP hypothesis?

We thank the reviewer for pointing this out and apologize for causing this confusion. Yes, the change in East Asian rain belt in Lau et al. (2006) is associated with EHP hypothesis. Therefore, the sentence has been modified as:

Modified: *"Lau et al. (2006) suggested that rainfall is suppressed over East Asia as a result of large-scale sea level pressure anomaly, itself induced by the enhanced rainfall anomaly over India which is caused by the ``elevated heat pump'' mechanism."*

6) Page 23012, Lines 16-17: What is meant by "biomass burning material" species?

In the model we used, as in others derived from the Unified Model models, biomass burning aerosol is defined as a mixture of black carbon and other organic materials. Emissions are estimated represented by multiplying the emissions of black carbon from biomass burning by 10. Note that this is different to most other climate models which do not carry biomass burning aerosol as an independent aerosol type. We have modified the text as follows: *"There are four types of aerosol species in HiGAM: sulphate, black carbon, biomass burning aerosol and sea salt. The biomass burning material is defined as the mixture of black carbon and other organic materials."*

7) Page 23012, Line 18: What about the contribution from organic carbon? Could you add a reference to the sentence?

The only contribution from organic aerosols in this model is as a component of biomass burning aerosol. As shown in Figure 1a, Figure S6 and Figure S7, sulphur dioxide and black carbon show an emission peak over East Asia, whilst biomass burning aerosol is mainly emitted

from Southeast Asia, South America and Africa (As shown in Figure S7.). Therefore, their impact over the whole East Asia is relatively small. The omission of other organic aerosols (and indeed nitrates) could lead to an under-estimate of the scattering and indirect effects of aerosol, depending on the relative balance of organic to inorganic components that affects the increase of scattering with relative humidity. We have added sentences discussing the impact of to P23012, L17.

"*Sulphur dioxide (SO_2, a precursor gas for sulphate) and BC are two major anthropogenic aerosol sources over East Asia (See Figure 1a and S6). Biomass burning aerosol is mainly emitted from Southeast Asia and southern hemispherical continents (shown in Figure S7). Organic aerosol is only present in this model as a component of biomass burning aerosol. We anticipate that the missing organic and nitrate aerosol likely emitted from similar sources to the sulphate pre-cursors leads to our results being a slight underestimate of the radiative impact of total scattering aerosol components. However, we concentrate on SO_2 and BC as the likely major contributors to an aerosol effect in this region.* "

8) Page 23016, Line 15-18: How the water-uptake by sulfate aerosols is parameterized? Is dry sulfate column burden simulated? What is the seasonality in SO2 emissions used in the model simulations?

We thank the reviewer for this question. First of all, we need to correct a mistake in the original manuscript. The strongest sulphur dioxide emission over East Asia is not during JJA but during JFM (shown in Figure R3). Therefore, the high column burden of sulphate aerosol during JJA is due to more active photochemistry.

The sulphate column burden shown in the Fig3a in the original text is the sum of dry sulphate aerosols (Aitken mode and accumulation mode).

The sulphate aerosols are simulated in three modes with different size distributions and optical properties: Aitken mode, accumulation mode and dissolved mode. Depending on whether oxidation occurs within the cloud, the SO_2 proceeds by hydroxyl radical (OH) in gas phase to produce Aitken and accumulation mode and by hydrogen peroxide (H_2O_2) in cloud drop to produce dissolved mode.

The water-uptake of sulphate aerosols is parameterized as the nucleation of cloud drops by accumulation mode and the coagulation of Aitken mode with accumulation mode to form more of the accumulation mode.

For the above mentioned reason, the erroneous text (P23016, L13-18) has been replaced with "*The column burden of sulphate (sum of Aitken mode and accumulation mode) in the Control run (the blue solid line in Fig. 3a) shows a clear seasonal variation, with higher column burden in the summer monsoon season 15 (JJA) and lower values in the pre- and post-monsoon months. The higher column burden is JJA is likely due to more active photochemistry which produces more sulphate particles as the solar zenith angle is smaller.*"

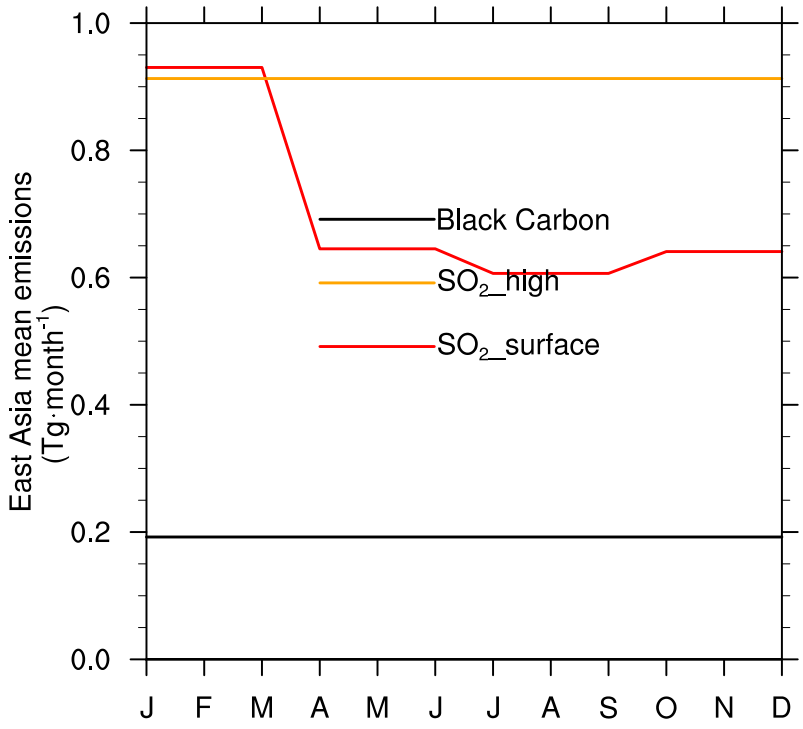

Figure R3. Annual cycle of the aerosol emissions averaged over East Asia.

9) Page 23013, Lines 7-10: The oxidant fields for sulfate chemistry is prescribed or calculated? Could the authors specify the oxidant fields more clearly?

The oxidants OH (for gas phase) and H_2O_2 (for aqueous phase) are prescribed in the model. We have added text explaining this to the manuscript

Modified: "*The sulphate scheme is described by Jones at al. (2001) and Woodage et al. (2003), in which the precursor gases, SO_2 and dimethyl sulphate ($(CH_3)_2SO_4$), undergo chemical reactions with oxidants in the atmosphere to form sulphate aerosol in both gaseous (with hydroxyl radical OH) and aqueous (with hydrogen peroxide H_2O_2) phases. The oxidant fields are kept constant in HiGAM. They are monthly average 3D fields, produced from simulations using the Lagrangian chemistry model STOCHEM (Collins, et al. 1997, Stevenson, et al. 1997).*"

10) Page 23015, Lines 14-25: Could the authors be more quantitative in their presentation. I would like to see a more rigorous statistical analysis related to evaluation of the model, perhaps a consideration of the mean bias and correlation.

The biases between HiGAM and GPCP have been added in Figure 2. The text has also been modified and combined with the modification of specific comment 3.

11) Section 2.3, mentions the evaluation of model-simulated JJA seasonal rainfall? Could the authors add one figure in SI about the evaluation of monthly-mean rainfall for the period from April to September?

The monthly mean rainfall from May to September has been shown in supplement as Figure S8.

12) In Figure 2, the authors compared the rainfall for two different periods. Could the authors plot this for the same period (1983-2000) and same resolution? This would be useful for the reader to interpret the results.

The Figure has been modified to show the same period (1983-2000). Please refer Figure 2 in specific command 3.

13) Page 23016, lines 15-18: Whether dry aerosol burden is mentioned? What type of seasonality used in fossil fuel SO2 emissions?

The text has been modified and combined with the modification in technical comment 8.

14) Page 23016, lines 21-28: Could the authors add the cloud droplet number concentration figure into Figure 3? This information could be useful for the reader to understand the aerosol indirect effects.

We agree that this information could be useful to the reader. The cloud droplet number concentration has been added to Figure 3.

15) Page 23017, Lines 5-7: The authors used no seasonality for BC emissions in the simulations. BC emissions from biomass burning emissions (forest and crop residue) exhibit significant seasonal variations. Would the conclusion from BC aerosols change if the emissions seasonality is included?

If the seasonal variation of black carbon due to biomass burning is considered in the experiment, the change of response of EASM might be different in magnitude but the conclusion should still be the same for two reasons (also see Figure R5). Firstly, in HiGAM, the BC emitted from biomass burning over East Asia is much smaller than BC emitted from industry. Secondly, the annual cycle of BC from biomass mass burning has peaks in spring and autumn, and would therefore have less impact during the summer monsoon season.

Figure R4 shows the BC emissions over East Asia at year 2000 and 1950. The solid lines are emissions used in our experiment, the dashed lines are emissions used for recent IPCC AR5 simulations. The BC emissions used in models generally do not show a strong seasonality, the values between our experiment and IPCC AR5 are close, and therefore we believe our experimental set-up is valid.

Therefore, We've modified the text (P23017, L5-7) as followed:

"*This is because BC emissions in HiGAM are derives from industries, therefore have no seasonal variation (BC emissions from biomass burning are small compared to from industrial emissions), also that BC is emitted as primary particles in HiGAM and its concentration depends more on physical conditions of the atmosphere rather than photochemistry.*"

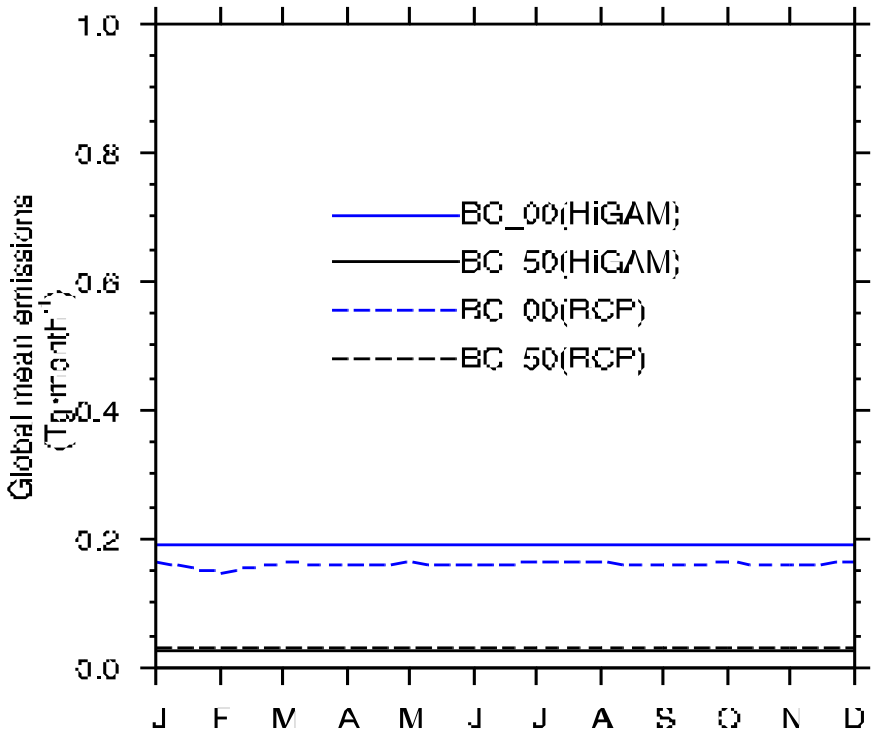

Figure R4. Annual cycle of BC emissions used in HiGAM (solid lines) and RCP (dashed lines). Blue lines is at level year 2000, and black lines is at level year 1950.

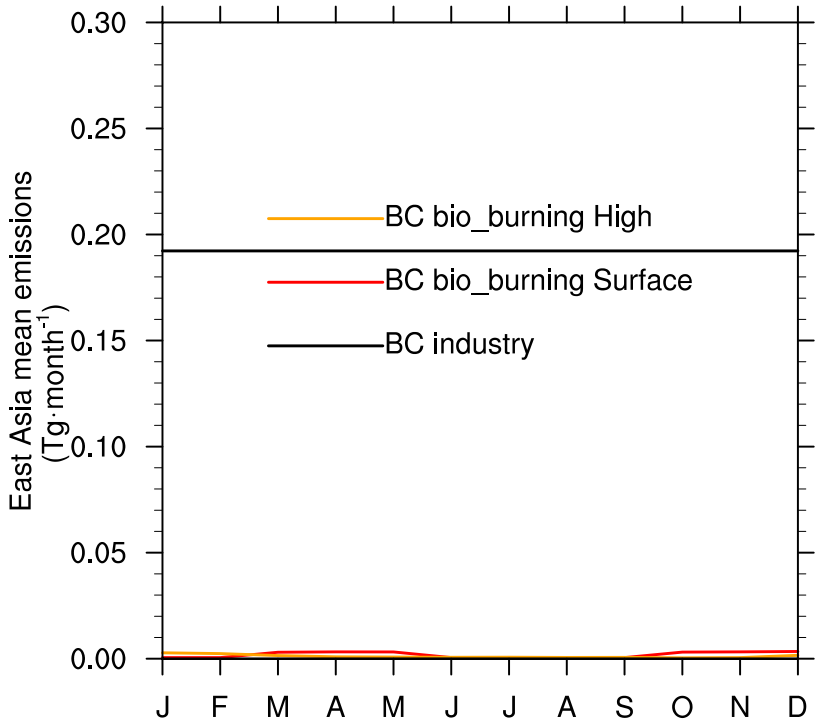

Figure R5. Annual cycle of BC emissions from industry and biomass burning. Average over East Asia.

16) Page 23017, lines 11-13: The authors need to evaluate the vertical distribution of BC and sulfate aerosols.

This comment has been addressed in our answer to specific comment 6 above.

17) Page 23018, lines 12-13: Could the authors add a figure reference to this statement.

We have made this statement quantitative as given below, and provided the figure in supplemental information.

Modified: "*The small change in precipitation (0.2 mm/day) is mainly due to changes in local evaporation (0.13 mm/day, shown as Figure S9).* "

18) Page 23018, Line 25-28: The definition of radiative effect should be moved to the methodology section. Is the radiative effect simulated in the SW spectrum?

The aerosol radiative effects are simulated using an off-line radiative transfer model. It is defined as the difference of net radiation including both the SW and LW spectrums. To clarify this in the paper, the text on P23018, L25-28 has been removed, and a paragraph has been added in Section 2.1.2. as follows:

"Three different aerosol radiative effects have been calculated in this study using the E-S code. The "direct radiative effect", is calculated as the difference of net radiation at both TOA and surface due to the change in sulphate or BC concentration, but with the atmospheric state variables (temperature, pressure and relative humidity) and other

radiative agents unchanged. The "combined radiative effect" of direct and first indirect effect for sulphate aerosol, is calculated from the difference of the net radiation at both TOA and surface by considering changes in both sulphate concentration and cloud droplet size predicted by the model. The "combined radiative effect" of direct and semi-direct effect for BC aerosol, is calculate the difference of the net radiation at both TOA and surface by considering changes in both BC concentration and in cloud cover change. Note that, since the changes in cloud cover and cloud liquid water content are not significant in sulphate experiment, only changes in cloud droplet size are considered in the off-line model. And since BC aerosol cannot act as CCN in this model, there is no indirect effect considered in BC experiment. "

19) Page 23020, lines 5-9: Could the authors add some observational precipitation trends to corroborate the findings (Figure 7c)?

Zhai, et al. (2005) using daily precipitation dataset of 740 rain gauges shown the precipitation over China during autumn has generally decreased throughout eastern China (Fig. 3e in their paper). However it is important to note that observed changes would not necessarily be expected to match those seen in the experiment since the observed changes will include components due to greenhouse gas forcing etc. We have modified the description of the text to discuss this issue.

" Figure 7c shows the eighteen-year mean changes in precipitation in September (Control minus SO2 1950). The precipitation significantly decreases over Central China, South-Western China and parts of the Indochina Peninsula. Since the change in precipitation over East Asia can be contributed to different factors rather than aerosol, e.g. ENSO,

GHGs, it is unfair to compare the this result with observational trend. However, Zhai et al. (2005) shows a generally decrease in precipitation throughout eastern China using daily precipitation data of 740 rain gauge."

20) Page 23022, lines 1-8: Could the authors specify whether the forcing values from clear-sky or all-sky conditions. Also add some discussion about the cloudy-sky forcing to support the indirect radiative effect findings.

The forcing values shown here are calculated under the all-sky condition. This aerosol radiative effect under the all-sky is different from many calculations shown in previous modeling and observation that calculate radiative effect due to changes in aerosol loading with fixed atmospheric states and cloud properties. Both changes in aerosol burden and in cloud properties are considered in the combined radiative forcing under the all-sky condition. To explicitly evaluate contributions from indirect effects, a set of radiation calculations considers changes only in cloud properties have been carried out using E-S code. For sulphate, the 1st indirect effect is dominant (account for 66% of combined effect). For BC, the BC direct effect is dominant but BC direct effect has been offset by semi-direct effect by 30%. These aerosol indirect effects are shown as Figure S14 in the supplement. Accordingly, the original text also has been modified as follows:

P23019, L28-30: "*Figure 7a shows the geographical distribution of the eighteen-year sulphate combined effect (all-sky) at the surface in September. It is negative over East Asia with the center over the lee side of the Tibetan Plateau where low cloud fraction is large. This suggests an important role for indirect effect and additional off-line*

simulations changing only cloud properties show that the sulphate 1st indirect effect accounts for 66% of combined effect (all-sky). See also Figure S14."

P23022, L1-6: "*Figure 8a shows the BC combined radiative effect at the surface in September. The BC direct radiative effect is compensated by a decrease in low cloud cover over the lee side of the Tibetan Plateau. This is because the increased BC concentration exerts a negative radiative effect at the surface, but as low cloud cover is reduced due to BC heating, more solar radiation reaches the surface that partially compensates for the BC direct radiative effect. In the additional off-line simulation changing only cloud properties (see Figure S14), the changes in low-cloud offset 1.2 Wm^{-2} radiative flux at the surface and this accounts for 30% of the BC direct radiative effect.*"

21) The conclusion section uses the terms 'slightly compensated, and small decrease'. Could the authors be more quantitative in their presentation?

We have added quantification to the conclusions as below:

"- The direct radiative effect is regionally compensated by decreased low cloud over Sichuan basin as more solar radiation reaches the surface (From -3.2Wm^{-2} to 2.9Wm^{-2}.).

- The decrease in land surface temperature is mainly over southeastern China, and compared to the sulphate experiment, the weakening of moisture transport exists only from Pacific Ocean."

22) Check the Figure 7 caption and correct it.

Done.

Reply to Reviewer #2:

General comments:

The authors investigate the impact of regional anthropogenic aerosol emissions (specifically, SO2 and BC) on the East Asian summer monsoon. The study makes use of a series of experiments with an atmospheric general circulation model in which either SO2 or BC emissions over East Asia are kept at 1950 levels, while they are at 2000 levels elsewhere in the world. The authors find no significant precipitation changes in June-August, while precipitation significantly decreases in September due to a reduced land-sea thermal contrast, which weakens the circulation. The topic is important and interesting; few studies have investigated the effect of local emissions on the monsoon. However, the authors need to address a number of issues related to the experimental setting and the results before the paper could be acceptable for publication. I therefore recommend a major revision.

We thank the reviewer for confirming the importance of studies carried out in this topic and address their specific comments below

Specific Comments:

1) I am concerned by the poor skill of the model in simulating the climatological precipitation amount and distribution (Fig. 2). For example, the model gives on average ~10 mm/day over southeastern China, compared to ~5 in observations. The distribution is also poor. I suspect that this large bias might affect the results described in the following sections. I recommend the authors to address this issue. I also wonder what is the model bias at monthly scale.

The monthly precipitation in HiGAM and GPCP and mean bias are now given in Supplement S8. In response to this comment, and a similar one from reviewer 1, section 2.3 has been rephrased to include more

discussion about model biases. However, we would point out that this model bias is common to many CMIP3 and CMIP5 models. Since our study concerns the processes influenced by changes in aerosols in the model, rather than trying to recreate the actual historical trend (which would also have been influenced by greenhouse gas forcing etc not considered in this simulation), the existence of a bias does not necessarily imply a problem for our conclusions. The text has been modified and can be found in specific comment 1 of the reviewer 1.

2) The experimental set-up is quite unusual. Compared to the cited studies, and others surprisingly not cited despite their relevance (e.g., Cowan and Cai GRL 2011; Ganguly et al. JGR 2012), I am not convinced by the way the authors designed the experiments, that is restarting the simulation from April every year from the same control run which has 2000 emissions. Most of the cited studies used a long continuous simulation, and examined the last 40-50 years when the model is at equilibrium. There could be some issues related to the present set-up. For example, how can the authors neglect the memory effect of the land which has a long time scale? Even if the atmosphere adjusts quickly to the emissions, it might take some time to reach equilibrium over a large domain considering all the feedbacks at play. It would be appropriate to compare the results with those from a continuous run. Additionally, to avoid interannual variability, wouldn't it be better to run a model with climatological-mean SSTs?

In this comment, the reviewer suggests two different experiment set-ups. One is the long-term (coupled) equilibrium experiments (more realistic), the other suggested experiment set-up is using climatological SST (more simplified). These two set-ups would be appropriate for answering quite different questions. Our experiment set-up stands in

the middle of two suggested by the reviewer. And this type of experiment is also used by other studies (e.g. Keeley et al., 2012).Our experiments exclude the feedback of ocean as a result of aerosol by prescribing SST, however, our experiments also consider the interannual variability of SST as well as impact of ENSO on EASM, and compare these impacts with aerosol impact. In fact we do estimate the likely aerosol impact on the ocean adjacent to East Asia (in section 5, P23023, L24-25 to P23024, L1-8), the estimated SST change as a result of aerosol is 0.6K/month, which is not significantly greater than internal variation of SST over this region. This suggests that the result in a coupled run would be similar to that presented here.

The soil moisture has an obvious seasonal cycle with maximum soil moisture at August (See Figure R6). The magnitude of the variation of soil moisture is about 27 kgm^{-2} from January to August. Compared to this seasonal cycle, the change in soil moisture due to sulphate and BC is small (less than ±3 kg/m^2)(see Figure R7). Therefore, the effect of change in soil moisture is not a major factor considering in this study.

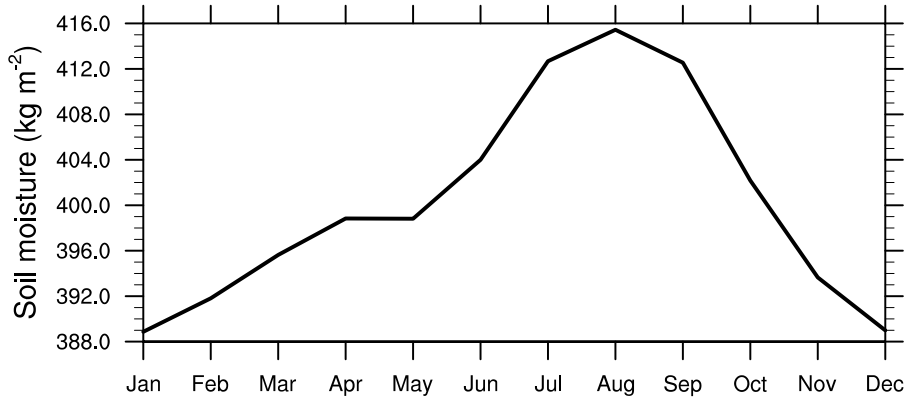

Figure R6. The seasonal cycle of soil moisture in Control experiment. Average over East Asia (20-45N, 100-122E). Units: kgm^{-2}.

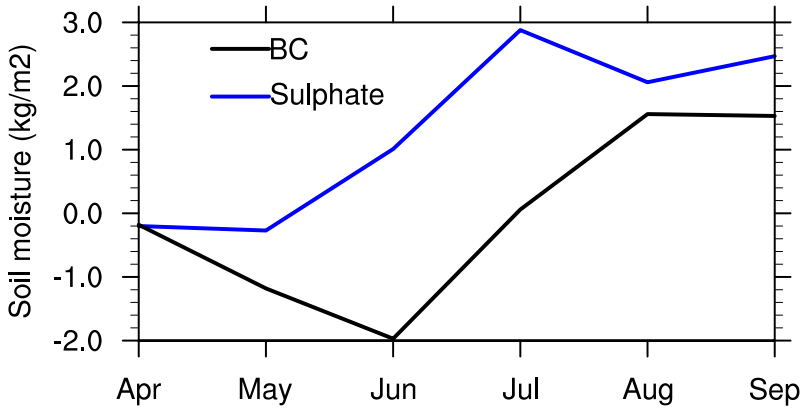

Figure R7. Change in soil moisture in sulphate experiment and BC experiment. Units: kg/m^2.

3) On the use of the same prescribed SSTs in all the experiments: this is another major point, since part of the difference between the various experiments is masked by the use of the same boundary conditions. I do not agree that the aerosol signal is comparable to internal variations, since it also affects SST gradients, which are perhaps even more important than absolute values. Precipitation-SST feedbacks might also be relevant.

I agree with the reviewer that the precipitation-SST feedback is a missing part in the study. However, the possible precipitation-SST feedback is detectable from our results. As shown in Figure 7c and Figure 8c, the precipitation changes in both sulphate experiment and black carbon experiment show a similar pattern over land and ocean, with a decrease over the land and an increase over the ocean. Therefore, a possible consequence as the precipitation-SST feedback is included would be that the extra precipitation over the ocean can cool the SST over the adjacent ocean, and the contrast of surface temperature between ocean and land also decreases. As a result, the response of EASM will be smaller as the adjustment of SST. Figure 7d and 8a can be used to explain the increase of precipitation over the

adjacent ocean. Due to the weakening of the EASM, the low level circulation changes as a moisture convergence center appears over the South China Sea and the Philippine Sea.

The purpose of this study is to evaluate the mechanisms between local aerosol change and the monsoon response. To this extent, our experiment set-up is valid to draw a conclusion on this topic. And results from our experiment offer a idea to separate aerosol direct impact from aerosol impacts through precipitation change to SST over the adjacent ocean.

4) I am not convinced by the lack of a clear signal in summer (JJA). This is in stark contrast with the findings of most of the previous studies. Is it because SSTS are the same? Is it because regional forcing is not important? Some more clues would be obtained by performing a complementary experiment in which only regional aerosols are at 2000 levels, while elsewhere are at 1950 levels. I ask the authors to consider carrying out this experiment as well.

As the reviewer notes, most of the previous studies changed aerosol emissions globally, but emissions in our experiment have been changed regionally. It is possible that this may weaken the response of EASM (e.g. Cowan 2011) to total aerosol changes and explain the lack of a clear signal in JJA comparing to most previous studies. In fact our results are consistent with previous studies considering only LOCAL aerosol influences. In Figure 14 d and e of Ganguly, et al. (2012), the responses of precipitation during the summer monsoon to local and non-local aerosol forcing have opposite signs over Eastern ASia. When considering local aerosol forcing, their result is consistent with ours,with a decrease in precipitation over eastern China. However,

when considering non-local aerosol forcing, the response of monsoon rainfall shows a increase of precipitation over eastern China, but a decrease in the north. Additionally, there remains significant uncertainty in the impact of remote aerosol on the monsoon. Cowan et al. (2011) attribute the suppression of Asian Monsoon to non-Asian aerosol sources, whilst Ganguly et al. (2012) emphasize the importance of local anthropogenic aerosol changes. Also considering non-local aerosol influences, Lau et al. (2006) show that absorbing aerosol over India can shift the rain belt over East Asia northwestward and suppressing rainfall over eastern China. However these results are different from the non-local aerosol experiments of Ganguly et al. (2012). Our study is deliberately targeted at considering possible mechanisms by which local aerosol can influence the EASM, and does not attempt to tackle the impact of remote aerosol, since the uncertainties in the processes by which this influence might happen are clearly large.

5) I am surprised by the fact that the combined SO+BC response in not presented. This should be the first brick of the study, just to see if all local emissions are important or not. Furthermore, what about OC? Why is it not considered?

We have replied a similar comment of the reviewer 1. Therefore, we combined the replies of this comment with the specific comment 4 of the reviewer 1.

6) Regarding comment 4, how can these results be interpreted in the context of the previous studies?

We thank the reviewer for the opportunity to emphasize the novelty of our study. As the reviewer 1 gave a similar comment (specific comment 8), we rewrote the first paragraph of the conclusion. Please refer we reply to reviewer 1's specific comment 8.

7) Could you show geographical maps of the changes in precipitation and temperature (surprisingly not shown) in addition to the area-average changes?

We did consider showing these in the paper, and now show them in the supplemental information The monthly change of precipitation and surface temperature for both sulphate and black carbon are shown as Figure S10-S13 in the supplement.

8) How do these findings relate to observed precipitation changes?

We have addressed the comment in reviewer 1's technical comment 19.

9) I do not find a clear explanation of why the aerosol impact is evident only in September and not in the previous months.

The interannual variability of EASM is strong compared to aerosol forcing simulated in our experiments. Therefore, the signal of both sulphate and black carbon is insignificant during JJA. As the aerosol-low cloud interaction plays an important role in changing the EASM, the increased amount of low level stratiform cloud during September allows a stronger aerosol effect which makes the response of EASM in September strong enough to be significant compared to interannual variability. This is a key result of the paper and therefore, Figure 6 has been added to illustrate change of low level cloud in the experiment.

10) The BC emissions have no seasonality in this study. Isn't it a major limitation as actual emissions have been shown to have a strong seasonality?

We have addressed this comment in reviewer 1's technical comment 15.

Technical Comments:

1) Page 23010, line 3: what do you mean by "some aerosols"?

This should have referred explicitly to carbonaceous aerosol and the text has been modified accordingly. The modified text can be found in reviewer 1's technical comment 3.

2) Page 23010, line 16: this conclusion is not correct.

We gave an explanation of this conclusion in reviewer 1's technical comment 4.

3) Page 23018, line 12: May has already substantial precipitation over the region (based on GPCP, about 3.5 mm/day, increasing to 5.1 mm/day in June).

According to Lau et al. (1996), the onset of summer monsoon over Southern China is during the end of the May and the beginning of the June. As shown in lately added Figure 3 and discussion by Tian et al. 1992, during Sprint over south China, there is a persistent precipitation due to the interaction between low level flow and the orography over eastern China. Therefore, the aerosol impact during May is mainly changing surface temperature and change local evaporation. To clarify this point, the text over P23018, L12 has been changed as:

"Original: 'The small change in precipitation is mainly due to changes in local evaporation.'

Modified: "*The small change in precipitation (0.2 mm/day) is mainly due to changes in local evaporation (0.13 mm/day, shown as Figure S9).* "

4) Fig. 2: please compare the data over the same period of time. Change the scale for plotting the arrows, it is difficult to see the flow over China.

Thank for the comment. The Figure has been modified to show the same perod (1983-2000).

Added reference:

R. Adler, G. Huffman, A. Chang, R. Ferraro, P. Xie, J. Janowiak, B. Rudolf, U. Schneider, S. Curtis, D. Bolvin, A. Gruber, J. Susskind, P. Arkin, and E. Nelkin. The Version-2 Global Precipitation Climatol- ogy Project(GPCP) Monthly Precipitation Analysis(1979-Present). J. Hydrometeorol., 4(6):1147–1167, 2003.

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Keeley, SPE ; Sutton, RT; Shaffrey, LC Source: QUARTERLY JOURNAL OF THE ROYAL METEOROLOGICAL SOCIETY Volume: 138 Issue: 668 Pages: 1774-1783, 2004.

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Supplement for:

The effect of regional changes in anthropogenic aerosols on rainfall of the East Asian Summer Monsoon

L. Guo, E. J. Highwood, L. C. Shaffrey, and A. G. Tuner

1. The optical properties of sulphate and black carbon aerosols used in HiGAM and off-line E-S radiative transfer model.

For sulphate aerosols, the single scattering albedo (Figure S1) is close to 1 over shorter wavelength in shortwave regime. The mass extinction coefficient (Figure S2) increases exponentially with relative humidity. As sulphate aerosol is hydrophilic, the mass extinction coefficients increase exponentially, especially as the relative humidity higher than 90%. The asymmetric function (Figure S3) shows a similar result. As the relative humidity grows, more shortwave radiation is scattering forward.

For black carbon aerosols, these optical properties are not varied with relative humidity. In Figure S4, the mass extinction coefficient, single scattering albedo and asymmetric function of BC decrease with the increase of wavelength in shortwave regime.

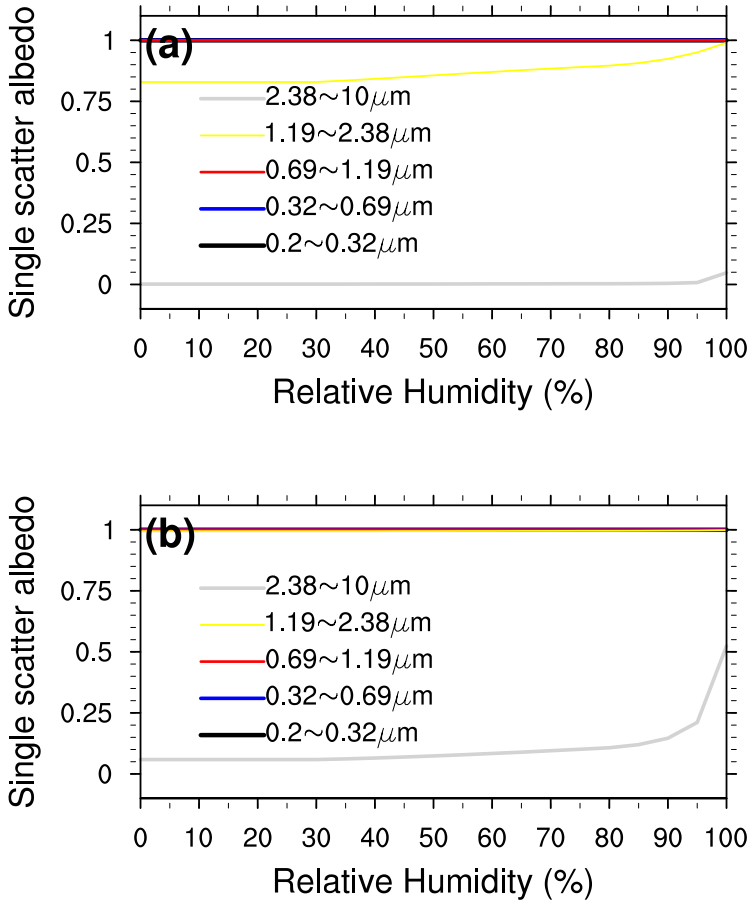

Figure S1. Single scattering albedo of sulphate aerosol as the function of relative humidity used in HiGAM and off-line E-S radiative transfer model. (a) Aitken mode and (b) accumulation mode.

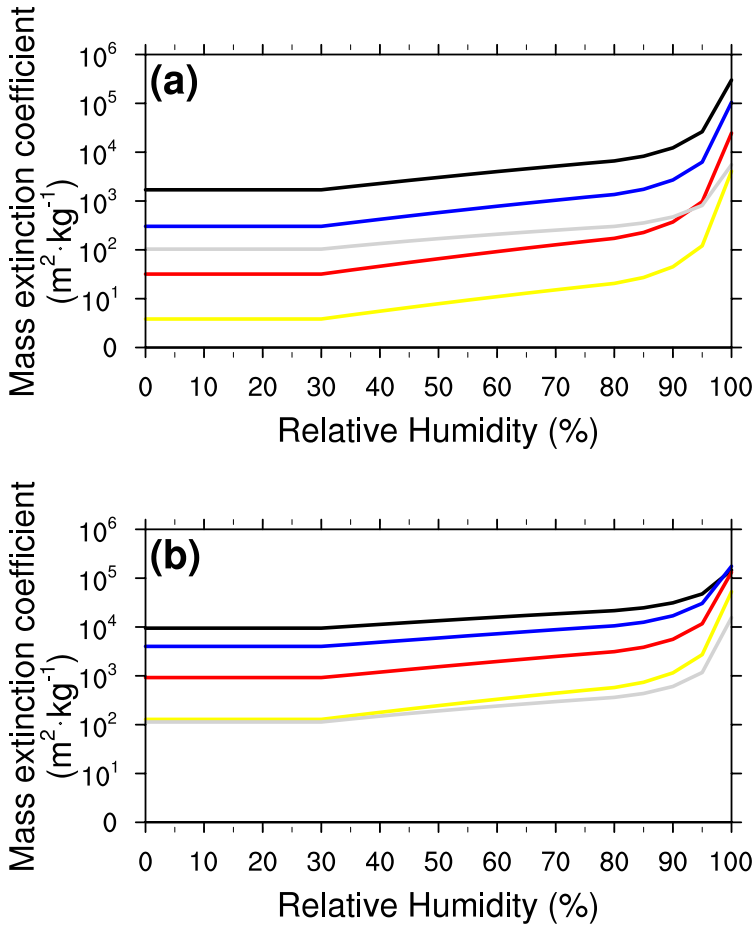

Figure S2. Mass extinction coefficient of sulphate aerosol as the function of relative humidity used in the HiGAM and the off-line E-S radiative transfer model. (a) Aitken mode and (b) accumulation mode.

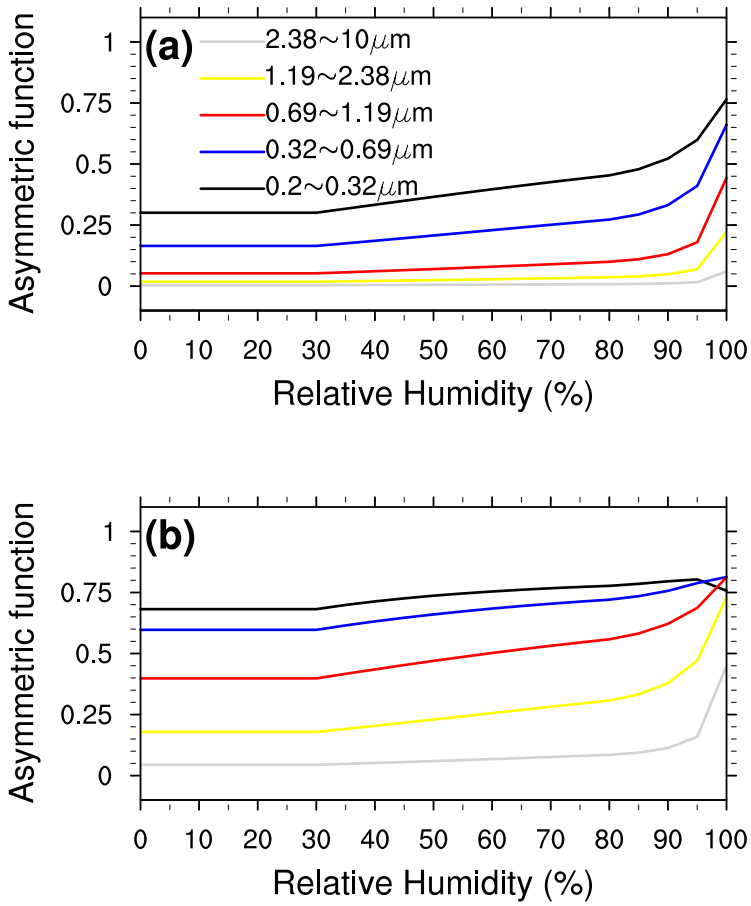

Figure S3. Asymmetric function of sulphate aerosol as the function of relative humidity used in the HiGAM and the off-line E-S radiative transfer model. (a) Aitken mode and (b) accumulation mode.

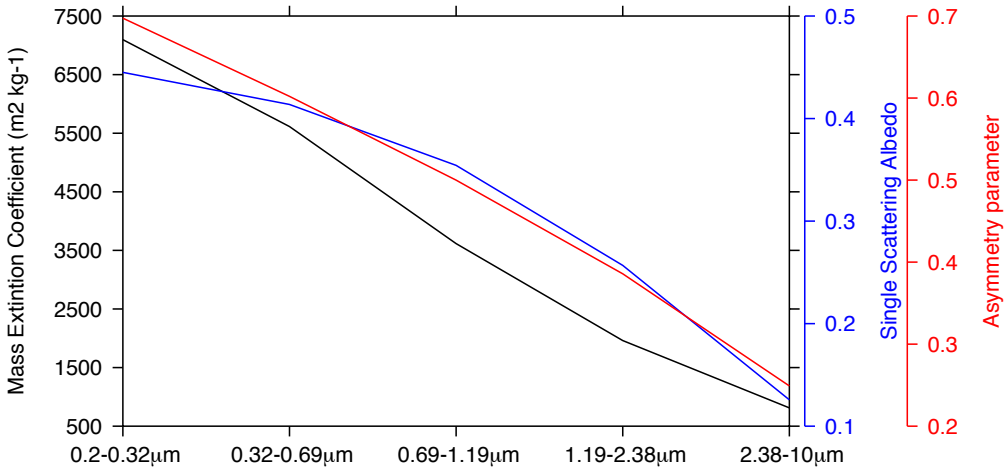

Figure S4. The mass extinction coefficient (black), single scattering albedo (blue) and asymmetry parameter (red) used in HiGAM to represent the optical properties of black carbon.

2. Vertical distributions of sulphate and black carbon aerosols.

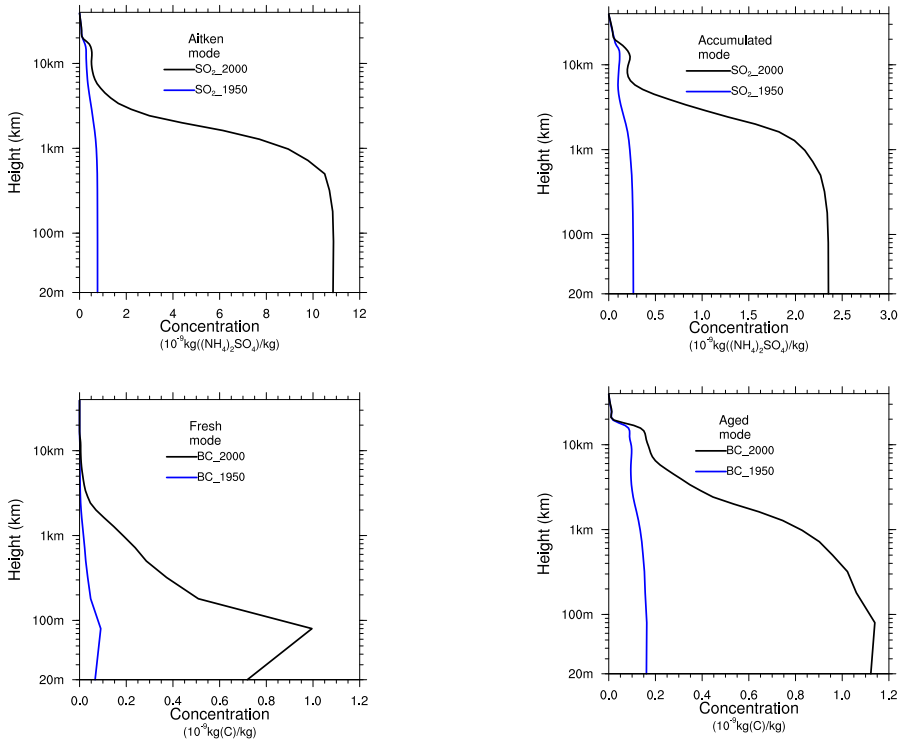

Figure S5. The vertical distributions of sulphate aerosols (upper panels) and black carbon aerosols (lower panels) averaged over 20-45N, 100-122E.

3. Aerosol emissions in HiGAM at the level of year 2000 for black carbon and biomass burning material.

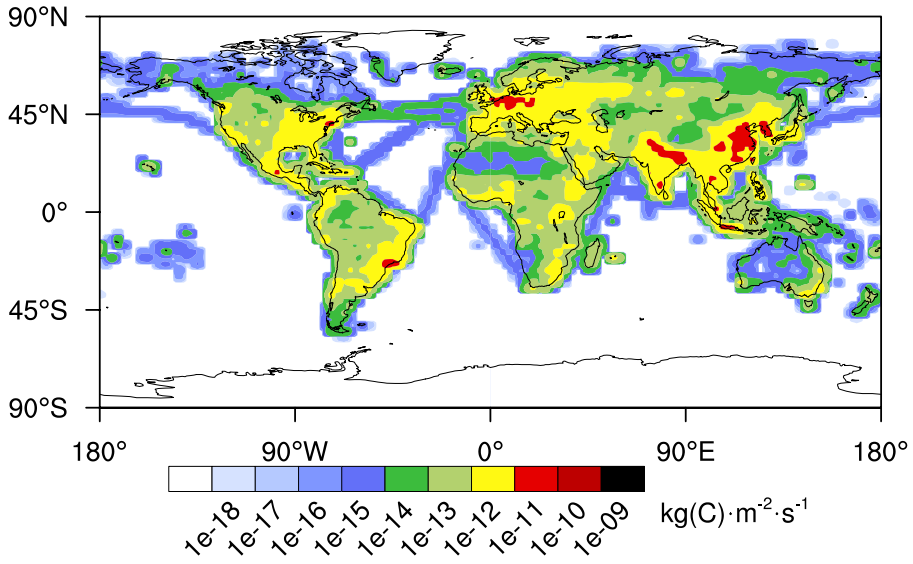

Figure S6. The annual mean emission of black carbon aerosol used in HiGAM. Units: kg(C)$m^{-2}s^{-1}$.

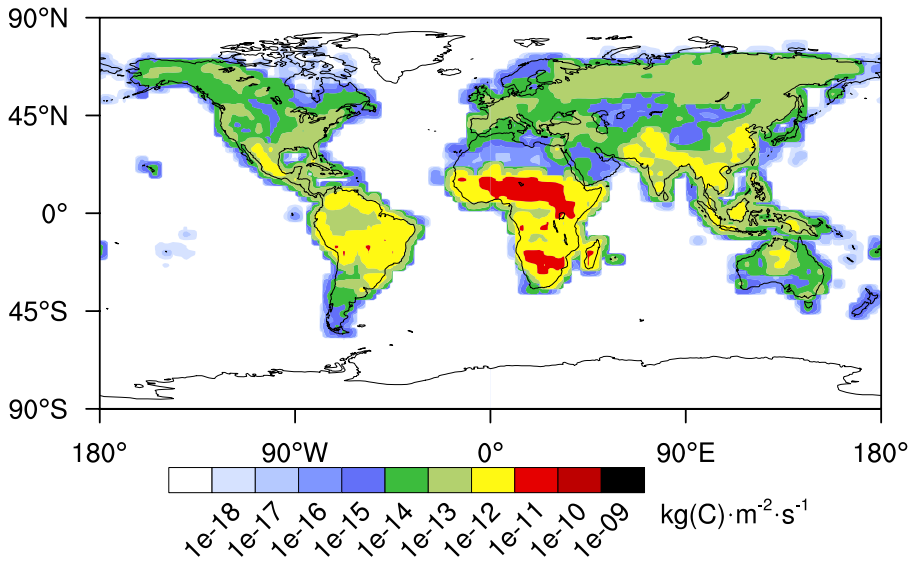

Figure S7. The annual mean emission of biomass burning aerosol used in HiGAM. Units: kg(C)$m^{-2}s^{-1}$.

4. Monthly precipitation

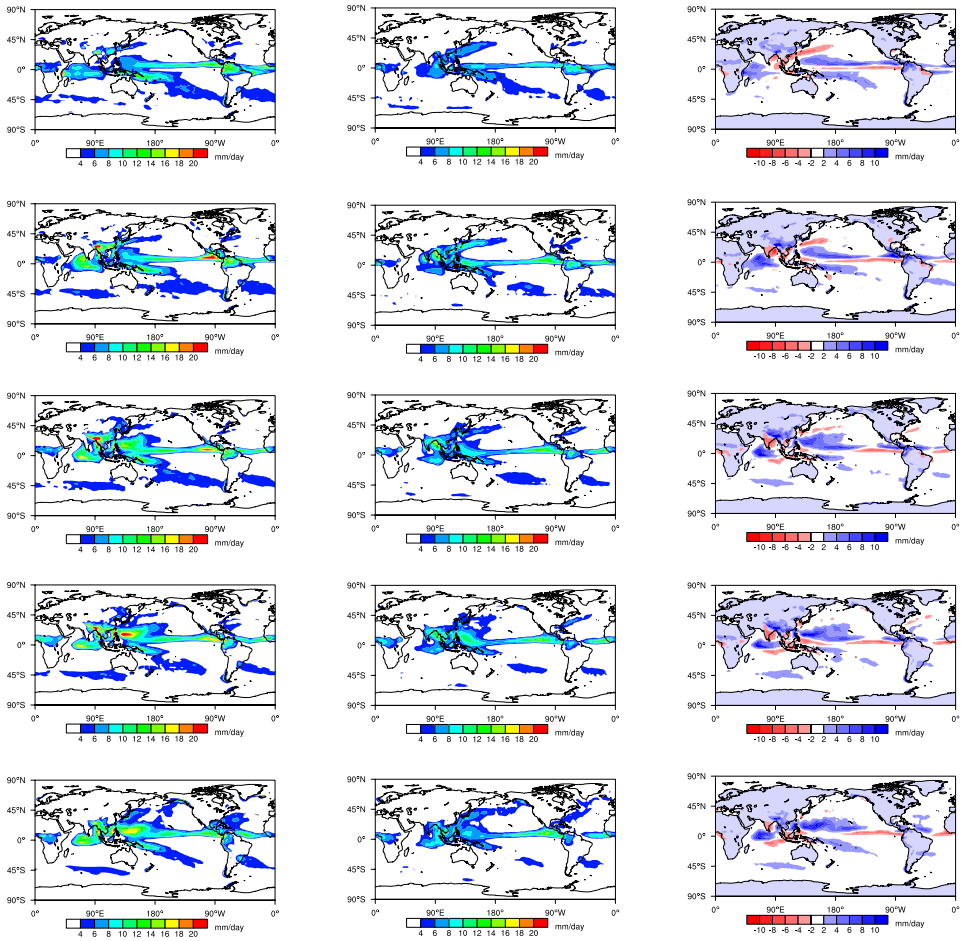

Figure S8. Monthly mean precipitation from May to September. The first column is HiGAM, second column is GPCP, and the third column is the difference. Units: mm/day.

5. Evaporation in May.

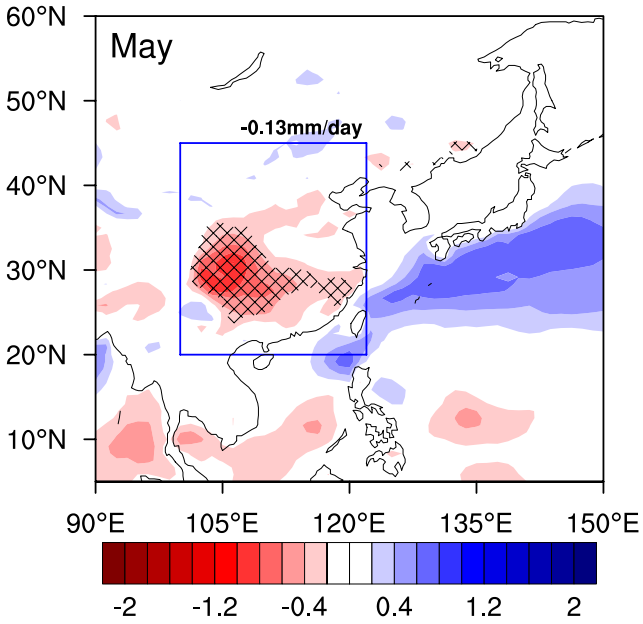

Figure S9. Monthly mean change in evaporation in May. Area mean value over the blue box is shown above the box. Significant change over 95% has been hatched.

6. Monthly change in surface temperature and precipitation.

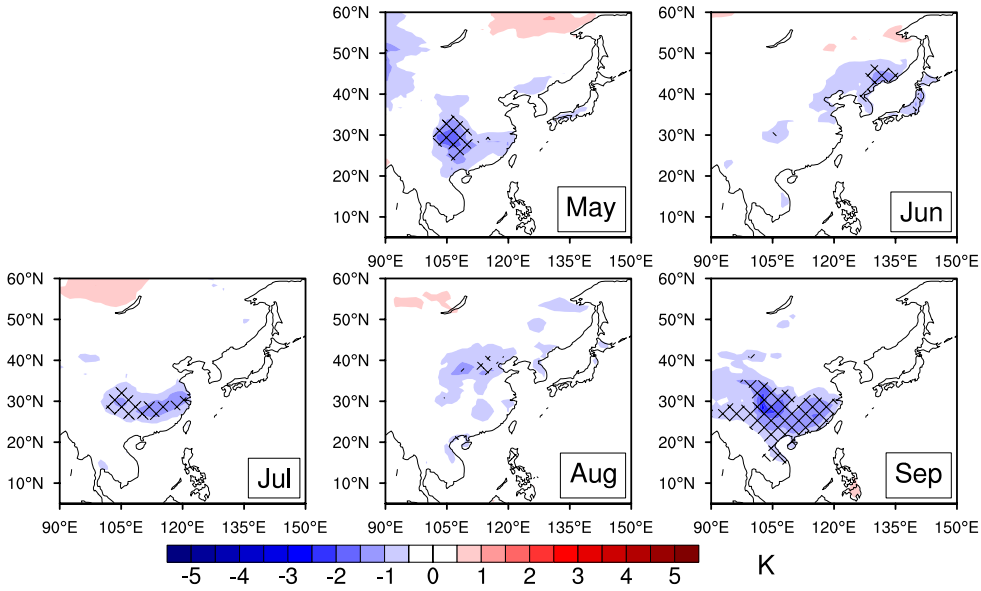

Figure S10. Monthly changes in surface temperature (SO2_1950 minus Control, Units: K). The significant changes over 95% level are hatched.

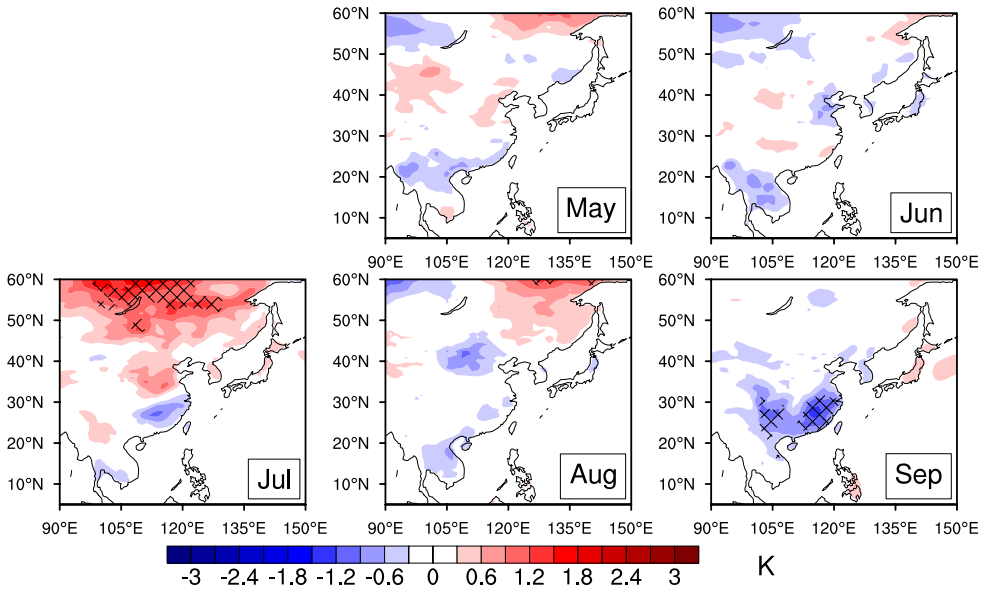

Figure S11. Monthly changes in surface temperature (BC_1950 minus Control, Units: K). The significant changes over 95% level are hatched.

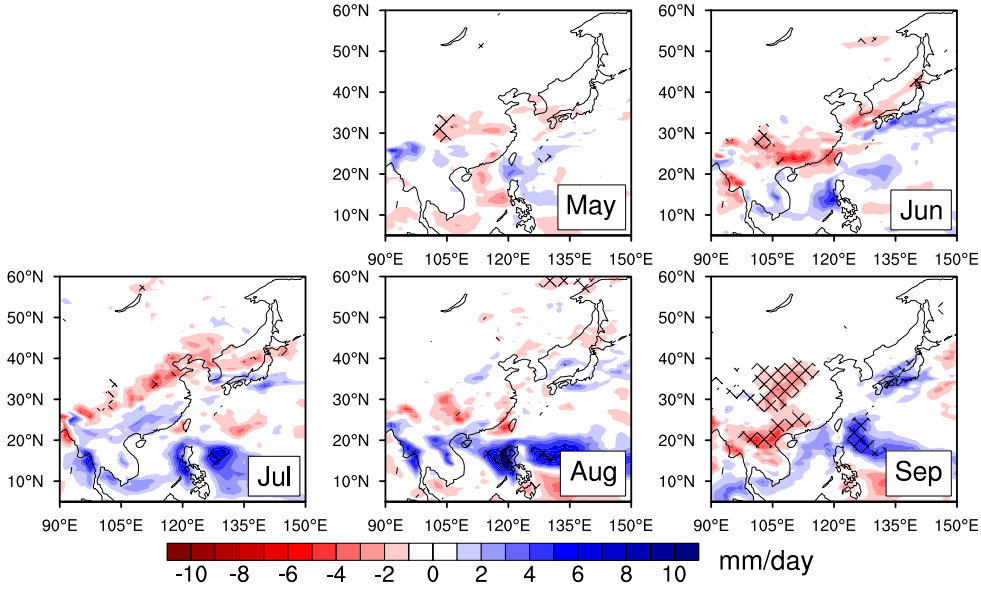

Figure S12. Monthly changes in precipitation (SO2 minus Control, units: mm/day). The significant changes over 95% level are hatched.

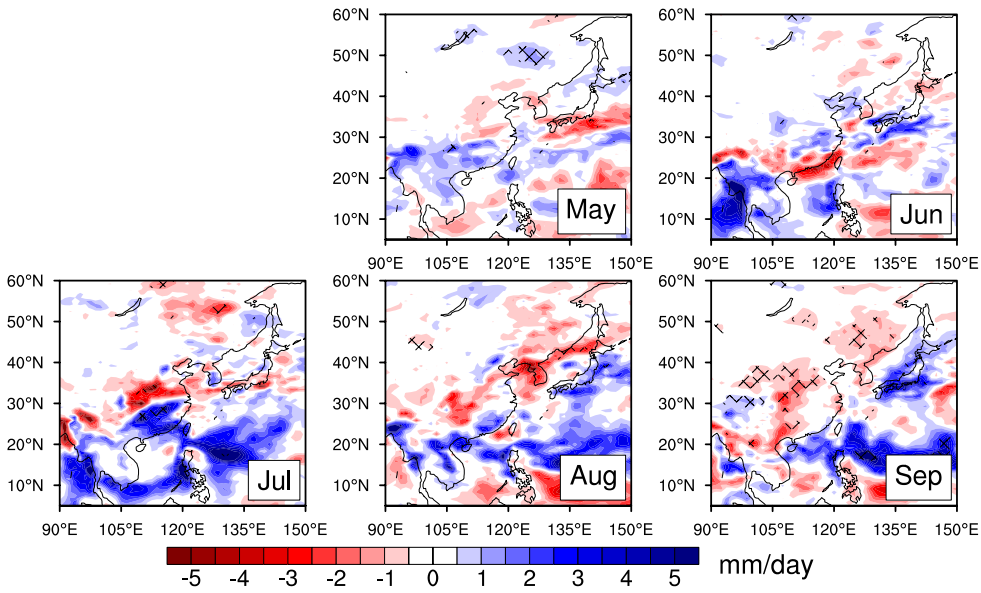

Figure S13. Monthly changes in precipitation (BC minus Control, units: mm/day). The significant changes over 95% level are hatched.

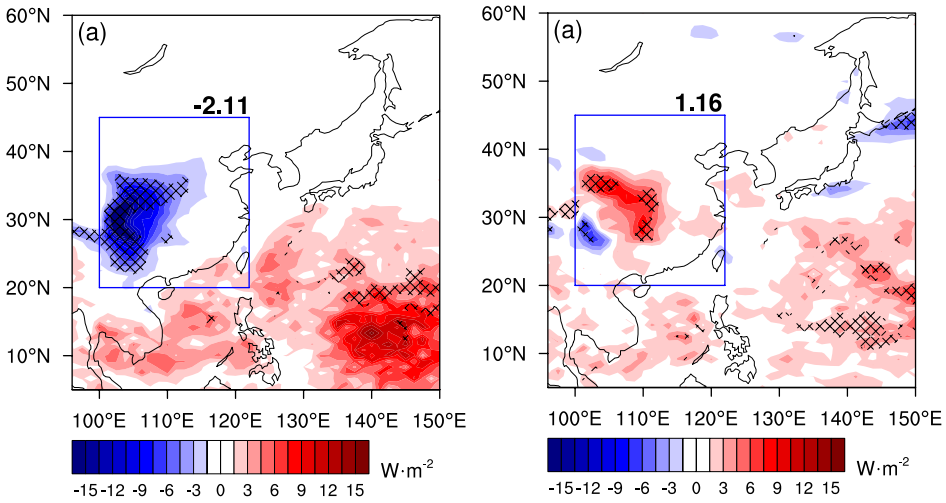

Figure S14. (Left panel) 18-yr mean net radiation flux (downward is positive) in September due to cloud droplet size change in sulphate experiment. (Right panel) 18-yr mean net radiation flux (downward is positive) in September due to cloud fraction

change in BC experiment. Units: Wm^{-2}. Numbers in the plots are the area mean value within the blue box.