Replies to Anonymous Referee #1

Fadnavis et al. studies the regional impact of carbonaceous aerosol in ASM region by doubling the Asian carbonaceous emissions. In general the topic is interesting and important. However I think the paper may overstate some of the significance and some information are missing/incomplete. General comments:

Reply: We thank reviewer for careful reading and valuable suggestions. We have incorporated all the suggestions given by the reviewer. These changes are marked in red color and corresponding line numbers are indicated.

1. I am a little bit confused here. The title suggested the paper is going to focus on UTLS region. Reading through the paper, I found little evidence from ECHAM6-HAM model supports a sounding impact of carbonaceous aerosols on UTLS.

Reply(1): We have revised the manuscript and incorporated discussion of impact of carbonaceous aerosols on monsoon processes. Therefore the title of the paper is now changedas "Potential impact of Asian carbonaceous aerosol emission on the Upper Troposphere and Lower Stratosphere (UTLS) <u>and precipitation</u> during Asian summer monsoon in a global model simulation".

2. This study shows a heating rate of +0.003-0.005K/day (Line 434) due to carbonaceous aerosols near the tropopause, and it is only 1 to 2% of total atmospheric heating rate (~0.2 to 0.5 K/day). What is the uncertainness of the atmospheric heating rates at the UTLS? Gettelman et al. (2004, Figure 4) suggests that the uncertainties from different radiative transfer models is on the order of +-0.1 k/day, and spatial/temporal distribution of water, ozone, aerosol will add more uncertainties. Is the +0.003-0.005 K/day due to carbonaceous near the UTLS significant enough given the relative large uncertainties.

Reply(2): Heating rate figures are revised. We have now added 95% confidence level and they show that heating rate anomalies are significant in the UTLS (Fig. 4 c-d; page 44).

3. Paper shows one profile BC comparison with balloon sounding. It is hard to tell the concentration from the Figure in linear scale. Maybe a log scale is better for UTLS. In addition, it is necessary to show more model's validations of BC's vertical profile especially in UT since the conclusion relies heavily on modeled BC vertical profile. I know there is a SP2 campaign over Japan up to 9/10 km. You mentioned CARIBIC aerosols, how does your model simulation compared with CARIBIC data?

Reply(3): Thank you for the suggestion. Figure is now modified on log scale. We have tried to obtain the above mentioned data for model's validation. We had contacted Dr. Hang Su, the investigator of CARIBIC and he informed that data is not ready to release (an email correspondence attached). However, additionally, we have compared model simulations with aircraft measurements in the lower-mid troposphere over Guwahati, India since we could not get BC measurements in the UTLS (Fig. 2a-d and discussion on pages 10-11, lines 204-228).

4. How you treat OC and BC? How much contribution comes from OC and BC separately?

Reply(4): Thank you for the suggestion. We have incorporated a few figures showing contribution of BC and OC separately (Table1; Fig. 7; Fig.S1-Fig.S4) and related discussions are incorporated (Page 15-16, lines 316-321, 325-337, 349-352, page 17, lines 363-369; page 19; lines 419-422).

5. Paper shows a warming core above TP, and a large temperature anomaly of 3K (Figure 4f) due to carbonaceous aerosol increase. I wonder is the 3K due to aerosol directly or water (through change in dynamics) or just model noise? The feature (spatial pattern) of 3K temperature anomaly in Figure 4f is different/inconsistent with heating rate/aerosol anomaly in Figure 4(b, d). In addition, your calculated heating rate due to aerosol (Figure 4d) shows some value less than 0.003 K/day, (very difficult to tell from the color scale) above 500 mb, while your 3K feature in Figure 4f is located at regions between 500 and 200 mb. Seems the heating rate (0.003 K/day) is too small to achieve a temperature difference of 3K. Paper mentioned 2.6 W/m2 for the total forcing in Line Some other suggestions:

Reply(5): Thank you for the suggestion. We have improved the color scale in revised manuscript. These figures show heating rates are ~0.03-0.05 K/day near 500 hPa and temperature ~1K over the Tibetan Plateau (TP). We have mentioned that temperature anomalies of 1K in the upper troposphere over the TP may be due to heating by aerosol and water vapour together. The increase in water vapour in the mid-upper troposphere in response to dynamical changes (as seen in Fig 8a and b) induced by doubling of carbonaceous aerosols contributes additionally to this warming (page 17, lines 363-369 also page 21, lines 459-460).

Figure color scale is hard to tell

a. Reply: We have re-plotted the figures and color scale is improved.

b. Line 302, explain why your forcing (+2.7 W/m2) is quite different from other studies from Babu (2002) for +28 W/m2, and Badarinatha and Latha (2006) +42 W/m2. Is that because of different spatial sampling? Please justify.

Reply: The above stated forcing (+2.7 W/m2) is anomalies obtained from Demiss-CTRL simulations. Studies pertaining to BC/OC radiative forcing are sparse over the Indian region. Radiative forcing given by Babu (2002) for +28 W/m2, and Badarinatha and Latha (2006) +42 W/m2 are at a point location and during different season. This discussion is now moved in the introduction section (page 5, lines 92-97).

We have compared radiative forcing with Sreekanth et al., (2007) during the monsoon season. The reasons for differences are also explained (pages15-16, lines 328-333).

c. When you show how much vertical velocity or water vapor etc change with your experiment, please also provide % change.

Reply: Thank you for the suggestion.The changes in water vapor are provided in % (Fig.8a-b) (related discussion on page 21, lines 451-453). Values of vertical velocity are small and to avoid division by small values, we show differences.

Replies to Anonymous Referee #2

This paper considers the impact of doubled BC and OC aerosol emissions in the South Asia monsoon region using the ECHAM6-HAM model. This model has coupling between aerosol and convective processes but lacks important indirect effects and an interactive ocean surface. The authors find a systematic positive impact of doubled BC and OC emissions on the rainfall over India and eastern China. Since it does not account for the sea surface temperature (SST) feedback (e.g. Ganguly et al., 2012) and aerosol indirect impacts, this study should provide additional analysis that helps improve understanding of the impact of aerosols on the atmospheric state. I recommend this paper for publication after a major revision.

Reply: We thank reviewer for careful reading and valuable suggestions. Aerosol indirect impacts were already included in our model simulations. We have clarified it via email communication with model developer (email correspondence attached). There was misunderstanding related to model settings and we are sorry for this confusion. We have added few figures showing indirect impacts of aerosols from model simulations.

We have now incorporated additional analysis to show the impact of aerosols on monsoon processes. The changes are marked in red color and corresponding line numbers indicated.

1) In particular, the dynamical response to the aerosol forcing should receive a more thorough analysis and focused discussion. Given the lack of leading order feedback effects in these simulations, aspects that are likely to persist when these feedbacks are included should be given more attention since they have more value. In contrast, the impact on the rainfall is not such an interesting result since it is not likely to be robust given the disagreement with Ganguly et al. (2012).

Reply(1): Thank you for the suggestion. We have now incorporated discussion on the dynamical response to the aerosol forcing and corresponding figures (Carbonaceous aerosol induced changes in cross-equatorial jet, clouds, static stability from Brunt Vaisala frequency) (Fig. 5a, 5e, Fig. 6; pages 17-18 lines 376-380; page 18-19, lines 395-405) in the revised version.

The objectives of current study and Ganguly et al. (2012) are different. Both the studies give important results of impact black carbon aerosols on precipitation. Study by Ganguly et al. (2012) gives impact on precipitation on climate scale (present day and pre-industrial emissions) while the present study gives impact of atmospheric Asian carbonaceous aerosols on precipitation on the seasonal scale. Since purposes of these two studies are different the model set-ups used are different. Ganguly et al. (2012) used a general circulation model coupled to the surface layer of the ocean (slab ocean setup) to understand slow response from SSTs since they want to study impact on climatic scale. In contrast, the current study uses atmospheric-aerosol-climate model. Previous model studies using prescribed SSTs (Chung et al., 2002; Menon et al., 2002; Lau et al., 2006; Randles and Ramaswamy, 2008) also show increase in precipitation over India due to black carbon aerosols. These results are in agreement with present study. This is clarified in the revised manuscript (page 20, lines 427-441).

(2) Major comments: 1) Using fixed SSTs is a major limitation of this model study. The Ganguly et al. (2012) study indicates that surface heating feedbacks have an impact on the rainfall over India. Thus they cannot be ignored. The authors acknowledge this limitation but the value of their study diminishes if atmospheric processes that a exert a leading order impact are neglected. This study would have been of more interest if the process differences between the Ganguly et al. (2012) simulations and the prescribed SST simulations with ECAHM6-HAM had been evaluated.

Reply(2): As mentioned above objective of the present study and Ganguly et al. (2012) are different and therefore model set-up are different. In the past a number of studies (Chung et al., 2002; Menon et al., 2002; Lau et al., 2006; Randles and Ramaswamy, 2008) has analyzed impact of black carbon from model simulations using fixed SSTs. Their results are consistent with the present study. While in the present study we have studied impact of Asian carbonaceous aerosols unlike global BC emissions documented in the previous studies (page 20, lines 427-441).

3) On page 19, lines 405-408, the authors state that "Positive anomalies in cloud ice and ICNC (in the upper troposphere) may be due to enhancement in ASM deep convection...". The model makes it possible to discern such process details in contrast to observational studies.

Reply(3): We have removed “may be”. This sentence is now changes as “Positive anomalies in cloud ice and ICNC (in the upper troposphere) are due to enhancement in ASM deep convection (increase in heating rates, mid/upper tropospheric temperature, vertical velocity, and monsoon Hadley circulation) induced by the doubling of carbonaceous aerosols emissions” (page 22, lines 471).

4) Similarly on page 15, lines 321-323, "may" is used to describe a process that can be diagnosed from the model. The use of "may" is routine in other instances where transport

impacts on aerosols and other tracers are considered in this paper. It makes it seem as if the authors are not sure of the transport effects of the doubled BC and OC emissions. I suggest the "may" qualifier be removed and if there is some uncertainty in the interpretation of the model processes, then this uncertainty should be explicitly noted and discussed.

Reply(4): As suggested “may” is removed from the discussions in the revised manuscript.

5) Expanding on the aerosol impact on the circulation state would add value to this study given its limitations. For example, is there an organized circulation structure (both diabatic and isentropic) that is characteristic of the South Asia monsoon region? This subject is covered to various degrees in other studies but lacks the emphasis it deserves in this study. A more focused discussion of the dynamical response to the aerosol forcing is needed and a comparison with the dynamical regime in the Ganguly et al. (2012) study may help to improve understanding of the aerosol sensitivity in this region.

Reply(5): As mentioned in reply (1), we have now incorporated discussion on the dynamical response to the aerosol forcing and corresponding figures (carbonaceous aerosol induced changes in cross equatorial jet, clouds, static stability) (Fig. 5a, 5e, Fig. 6; pages 17-18 lines 373-377; page 18-19, lines 392-402) in the revised version since changes in circulation and atmospheric heating and temperature are already presented in the previous version.

As mentioned in reply (1). The purpose of the present study is different than Ganguly et al (2012) and therefore model set-ups are different. We have included discussions on this (page 20, lines 427-442).

6) Evaluation of vertical transport in the model is worthwhile in the case of the "subgrid" convective transport since the convection parameterization contributes heating tendencies that impact the circulation. Figures 3, 4 and 5 have the circulation anomaly vectors presented. In the case of UTLS transport, it is next to impossible to see the transport pattern in many cases since the lower altitude vectors dominate the scaling. Figure 5 compensates for the poor readability of the other figures and justifies the link between enhanced penetration of aerosol into the lower stratosphere and the increased vertical circulation through the tropopause which has substantial statistically significant regions.

Reply (6): Figure 5 is now improved to show transport pattern.

7) Figure 5 indicates that it is not just the Hadley circulation which changes but the low altitude jet structure between 10 and 25 N. This has an impact on the rainfall pattern as well.

Reply(7): As suggested, discussion on the low level jet structure between 10°N and 25° N is now incorporated (page 18, lines 395-397).

8) Indirect effects of BC and OC are not included in these simulations. It would have been much more worthwhile to consider the impact of increased IN and the increased cloud

evaporation due to aged BC fractions in cloud liquid and ice phases. It is not at all clear that these indirect effects would not substantially change the results. Thus the lack of indirect aerosol forcing is a serious limitation of this study. The authors should include more discussion of this limitation than the cursory mention on page. The conclusions of this paper have substantially reduced relevance considering the lack of important feedback processes including SSTs and cloud indirect effects.

Reply(8): Aerosol indirect impacts were already included in our model simulations. We have clarified it via email communication with model developer (email correspondence attached). There was misunderstanding related to model settings and we are sorry for this confusion. We have added a figure (Fig 6) showing indirect impacts of aerosols from model simulations. Section 5 ‘Conclusion’ section is now changed as ‘Summary and conclusions’

Minor comments:

p3. l33: replace "being" by "is", "population and economies" by "regions in terms of population and economy"

Reply: The above suggestion is incorporated in the revise the manuscript at page 3 line 35-36

p3. l35: replace "major" by "main" or "primary"

Reply: The above suggestion is incorporated in the revise the manuscript at page 3 line 38

p3. l38: C3 ACPD Interactive comment Printer-friendly version Discussion paper replace "contribute largely" by "substantially contribute"

Reply: The above suggestion is incorporated in the revise the manuscript at page 3 line 39-40

p3. l41: replace "significantly large" by "large" or "significant"

Reply: The above suggestion is incorporated in the revise the manuscript at page 3 line 43

p21. l445: replace "evidences" by "evidence"

Reply: The above suggestion is incorporated in the revise the manuscript at page 24 line 526

p21. l446: replace "show" by "shows"

Reply: The above suggestion is incorporated in the revise the manuscript at page 24 line 527