

Replies to review's Comments

Review of Paper: Potential impact of carbonaceous aerosols on the Upper Troposphere and Lower Stratosphere (UTLS) and precipitation during Asian summer monsoon in a global model simulation by Fadnavis et al. 2017, submitted to Atmospheric Chemistry and Physics

In general the paper is improved. However there are some issues that needed to be addressed before publication, especially Major concern #1.

Reply: We thank reviewer for valuable suggestions and careful reading. All the suggestions given by the reviewer are incorporated in the revised version. These changes are indicated at line numbers mentioned below and also marked in red color.

Major concerns:

1. In the previous paper version, the increased heating rate due to BC/OC near the tropopause in the ASM region was 0.003-0.005 K/day; however in the revised the version, the heating rate is 0.02-0.03 K/day, which is a factor of 6 larger. The heating rate of CTRL run remains the same between two versions of the paper. The BC/OC amount in UTLS remains similar (about 2 ng/m³). I wonder what happened? What have you changed? Also I noticed your TOA radiative forcing changed from strong negative to positive...Please explain.

Reply (1): In the previous version heating rates were estimated from model runs of a few years. Since the parameters, 'heating rates' and 'radiative forcing' were not extracted in output stream for every year to save space on the system with the thought that there may be minor changes in their values. We thank the reviewer for pointing out uncertainty in heating rate during last revision. To show significance level we have extracted these parameters for all the years which have improved the values and they are significant at 99% confidence level. Since Tibetan Plateau (TP) and Indo-Gangetic Plains (IGP) play important role in driving monsoon Hadley circulations we show radiative forcing over this region while in the first version of the manuscript it was over the ASM region.

We have seen that there is year to year variability in BC and OC concentration at different levels over the region of interest TP and IGP. This may be related to transport pattern due to monsoon convection in response to varying SST. These yearly varying concentrations of BC/OC aerosols will change RF and heating rates. Therefore RF and heating rates estimated from 20 years of simulations will show improved values. These values show better agreement with other studies (Myhre et al., 2013; Sreekanth et al., 2007). To show variability in RF we have incorporated standard deviation values in table-1.

2. The authors added more model experiments: add "double OC only run" and double "BC only run". Line 366-369: the radiative forcing for each case do not add up (compared with "double OC and BC run"). I can imagine the system is non-linear, but please provide more discussions on what cause the non-linearity.

Reply(2): Thank you for the suggestion. We have incorporated discussions on non-linearity in the radiative forcing due to BC/OC (pages 16-17, line nos 354-356).

3. Line 363-369: I am not convinced by the discussion on the temperature anomaly core in figure 4f. It is good to see in FigureS3a, the temp anomaly core is in the “double BC only run”. I am still wondering which leads to the warming core, note the core in the figure extends to mid-high latitudes. The discussions in the paper are not enough, and seems the authors are somehow guessing. Please justify.

Reply(3): BC aerosols play a major role in creating a warm core. An Extension of warm core to mid-high latitudes is due to BC emission from mid latitudes regions of China, Mongolia, Russia within to 80-110E. We have now incorporated discussion on this (page 17, line nos 375-377).

(4) Line 348: ASM region, not ASM

Reply(4): It is now corrected in the revised manuscript (page 16, line no 349).

(5) Line 216-217: re-write the sentence. I assume you meant the values range from surface to 10 km in model, while 6 km in observations.

Reply(5) : It is re-written in the revised manuscript (page 10-11, line nos 216-218).

(6)Line 366: Figure 8 instead of Figure 7?

Reply(6): Yes, it is figure 8. It is now corrected in the revised manuscript (page 17, line no 372).

(7) Please clarify the confidence level used for multiple figures. Are they 99% or 95%? In the response, you mentioned 95% while 99% in the figure captions.

Reply(7): In the revised version significance is plotted at 99% confidence level.