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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Potential impact of carbonaceous aerosols on the Upper Troposphere and Lower Stratosphere (UTLS) during Asian summer monsoon in a global model simulation" *by* Suvarna Fadnavis et al.

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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 changed as "Potential impact of Asian carbonaceous aerosol emission on the Upper Troposphere and Lower Stratosphere (UTLS) and precipitation 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 (âĹij0.2 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 the 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 $\sim$0.03-0.05 K/day near 500 hPa and temperature $\sim$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.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-197/acp-2017-197-AC1-supplement.zip

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