Atmos. Chem. Phys. Discuss., 12, C8896–C8899, 2012
www.atmos-chem-phys-discuss.net/12/C8896/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of regional changes in anthropogenic aerosols on rainfall of the East Asian Summer Monsoon" *by* L. Guo et al.

Anonymous Referee #2

Received and published: 1 November 2012

Review of the Manuscript: The effect of regional changes in anthropogenic aerosols on rainfall of the East Asian summer monsoon

Recommendation: major revision.

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.

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 $\sim$10 mm/day over southeastern China, compared to $\sim$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.

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?

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.

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 to carry out this experiment as well.

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?

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

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

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

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

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?

Technical Comments:

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

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

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).

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23007, 2012.