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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the variation in inflow to East Asia on surface ozone over Japan during 1996–2005" *by* S. Chatani and K. Sudo

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I thank Anonymous Referee #2 for thorough review and variable comments. My replies are as follows.

(Anonymous Referee #2) 1) The authors use the difference between two sets of regional WRF-Chem simulations with inter-annual varying BC and fixed BC for the year 2000 from the global CHASER model, to estimate the variability in pollution inflow to East Asia. Since Asian emissions are included in the global model simulations, the variability in ozone BCs can be affected by the export of Asian pollutants. These Asian-influenced pollutants at the regional domain boundaries can also be recirculated into East Asia when driving WRF-Chem with CHASER BCs. Therefore, the inter-annual variability in CHASER BCs can NOT be considered as "inflow" to East Asia. Results

from this experiment set-up can NOT be used to draw any conclusions that the authors stated in the manuscript about the influence of inflow to East Asia on the variability or long-term trends of surface ozone in Japan. The variability can be simply caused by the amount of ozone produced in East Asia and its recirculation at the regional model domain boundaries.

(Reply) The ozone BCs were updated frequently (daily) by using CHASER results to avoid the recirculation. The meteorological fields in CHASER and WRF/chem are expected to be consistent because both models used the same NCEP/NCAR global reanalysis data. As pointed out, the variability in the ozone BCs of WRF/chem can be affected by the export of Asian pollutants in the CHASER simulation. However, the wind direction should be also outward in WRF/chem in the days when the ozone BCs are affected by the export. Therefore, the influences of the recirculation should be minimal at least in the BXX case. The recirculation could cause problems in the B00 case because it used the ozone BCs created from the meteorological fields of 2000 even in other years than 2000. However, if the recirculation could much influence surface ozone over Japan, the major wind direction should be opposite in other years than 2000. It is not likely that such large variations exist in seasonal and annual meteorological fields.

(Anonymous Referee #2) 2) The simulated ozone are biased high as much as 30 ppbv (Figs.4-6). Nevertheless, the authors are talking about less than 4 ppbv of inter-annual variability in Figs.7-11. Wouldn't the variability in the model be simply driven by the variability of model biases? Is there a positive relationship between the model biases and the inter-annual variability of ozone? A scatter plot may help to address this issue.

(Reply) The simulated surface ozone follows the interannual variability of surface ozone well as shown in Fig. 7 of the original manuscript. Therefore, it would not be likely that the variability of simulated surface ozone was driven only by model biases. The correlation coefficients and the p values between observed and simulated surface ozone were calculated. The correlation coefficients were 0.73, 0.74, 0.81, 0.68 and 0.32, and the p values were 0.017, 0.013, 0.004, 0.032, and 0.404 for all months, MAM, JJA, SON and DJF, respectively. These values will be added in the figures in the revised manuscript. The high correlation coefficients and low p values between observed and simulated surface ozone also support the good performance of the simulation for the interannual variability of surface ozone except for DJF. Descriptions of the correlation coefficients and the p values will be added in the first paragraph of 4.

(Anonymous Referee #2) 3) The ozone anomaly shown in Figures 7 and 8 are very scattered, and looks more like inter-annual variability to me instead of a robust trend. The p value and the range of slopes in the 95% confidence limit should be calculated to explore if the trends are statistically significant. The model also fails to capture much of the variability in the fall and winter seasons.

(Reply) The p values and the range of slopes in the 95% confidence limit were calculated. Unfortunately, most of them were statistically insignificant. Only ranges and values which are statistically significant will be shown in Fig. 10 of the original manuscript (Please see Fig. 1 of this comment). At least, the increasing trends caused by the inflow for all months, JJA and DJF were statistically confident. These brief descriptions will be added to the third and fourth paragraph of 5. The model fails to capture the variability in the winter seasons, but has relatively better performance in the fall months only except for 2002.

(Anonymous Referee #2) 1) The abstract needs to be a concise and complete summary of the manuscript. Please describe briefly what models you are using and be quantitative about the model biases. The inadequate intrusion of low-ozone marine air masses and errors in monsoonal clouds/rainfall during the East Asia monsoon season are probably dominating factors causing the ozone overestimate in the model (Lin et al., 2009). The last few sentences in the abstract do not stand (see the major comment 1).

(Reply) Models and quantitative model biases will be added to the abstract in the re-

vised manuscript. One possible reason proposed by Lin et al., (2009) will be added to the second paragraph of 3.3. As described in my reply to the first comment, I believe that the last few sentences in the abstract are still valid.

(Anonymous Referee #2) 2) P30826, L4-25: Some other studies employing a global to regional coupled model system to study ozone inflow to Asia also need be referenced and discussed here, e.g. Lin et al., Atmos. Chem. Phys., 10, 4221-4239, 2010

(Reply) Brief descriptions of this paper will be added to the fourth paragraph of 1.

(Anonymous Referee #2) 3) P30827, L20-25: Need to describe the model vertical resolution in the lowest 2 km and near tropopause, which can partly contribute to the model overestimate.

(Reply) The first six layers are within 2 km above the surface. The lowest layer height is about 70 m, and the layer height around the tropopause is about 1 km. This explanation will be added in the second paragraph of 2.1.

(Anonymous Referee #2) 4) P30829, L1-5: Please clarify temporal resolution of BCs? Daily? Monthly mean?

(Reply) It is described in P30828, L25 in the original manuscript. Simulated daily concentration of ozone and monthly concentrations of other chemical species in CHASER grids were interpolated to boundary grids of the WRF/chem domain.

(Anonymous Referee #2) 5) P30829, 1.045 monitoring stations –> 1045 monitoring stations

(Reply) This part will be corrected in the revised manuscript.

(Anonymous Referee #2) 6) P30828, L8-9, Why not include biomass burning emissions? Biomass burning emissions modulated by ENSO should be an important factor for driving the inter-annual variability of tropospheric ozone and other tracers.

(Reply) I agree that biomass burning emissions should be an important factor for the inter-annual variability of surface ozone. If biomass burning emissions are included in the simulation, a large inter-annual variation may appear in simulated surface ozone. However, the inter-annual variation of biomass burning emissions has large uncertainties. I just feared that influences of the inflow and anthropogenic emissions within the domain on surface ozone would be masked by large uncertainties of biomass burning emissions. The first priority of this study is to investigate influences of the inflow and anthropogenic emissions within the domain. Therefore, biomass burning emissions were not considered in this study. The inter-annual variations caused by biomass burning emissions are remaining issues to be solved in the next step. A short explanation on biomass burning emissions will be inserted in the third paragraph of 2.1 in the revised manuscript.

(Anonymous Referee #2) 7) Table 1: Gunther –> Guenther. Why not use MEGAN to calculate biogenic emissions? Some studies have suggested that the simple biogenic emission scheme (bio_emiss_opt = 1) in WRF-Chem tends to underestimate isoprene emissions.

(Reply) The misspelling will be corrected in the revised manuscript. I thought that it is not likely that the difference in the biogenic emission schemes significantly influence the inter-annual variability and the long-term trend of surface ozone.

(Anonymous Referee #2) 8) Does CHACER have similar biases (up to 30 ppbv) at mid-latitudes sites in the other continents? If so, wouldn't the model overestimate the contribution of ozone produced in other continents to East Asian ozone?

(Reply) Actually, it is not appropriate to compare results of CHASER with observation data at Japanese populated areas because it is a global CTM which is intended to represent the large-scale air quality. Therefore, biases have not been evaluated in the other continents. I just put them in figures to indicate effectiveness of the coupled models used in this study. Basically CHASER has the good performance over the regional scale.

(Anonymous Referee #2) 9) Section 4-5 and associated figures: all discussions and conclusions about inflow need to be rephrased. The current experiment set-up in this study does not support the conclusions (see major comment 1)

(Reply) As described in my reply to the first comment, I believe that discussions in Section 4-5 are still valid.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30823, 2010.

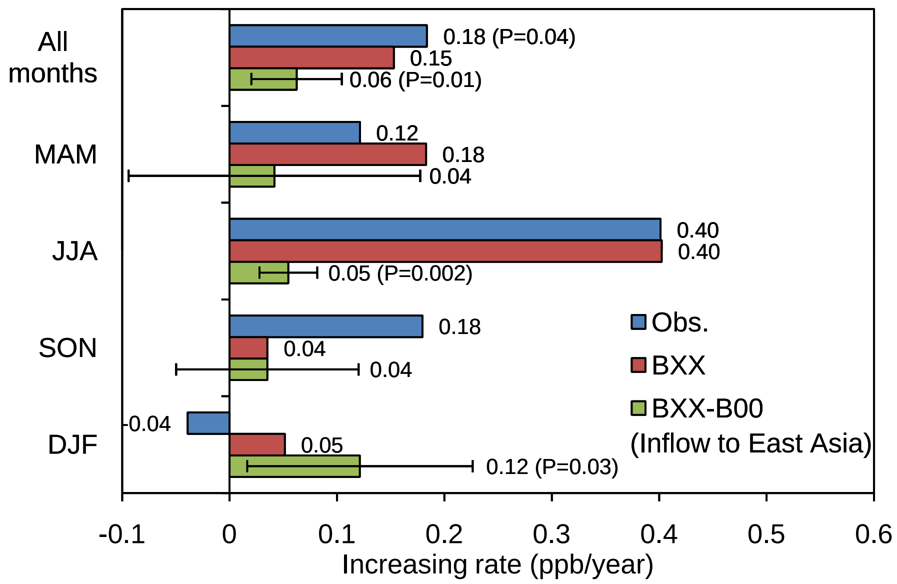

Fig. 1. (to be replace with Fig. 10 in the original manuscript)