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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Process analysis and sensitivity study of regional ozone formation over the Pearl River Delta, China, during the PRIDE-PRD2004 campaign using the CMAQ model" *by* X. Wang et al.

Anonymous Referee #3

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General comments

In this paper the authors performed a modeling study using the CMAQ modeling system with attempts to characterize ozone formation, quantify the contribution of different physical and chemical processes the in situ O3 formation through a process analysis, and investigate the O3 response to emission reduction through the Brute Force method in the PRD region in China. The paper adds values in understanding the ozone chemistry in the specific region. The paper stands in a good structure, but revisions are needed as suggested below before it is accepted for publish in ACP. A major concern

is on the emission estimates. No measurements of NO are presented, which is a better indicator of the NOx emissions (since NO2 is heavily affected by chemistry and the model-observation emission evaluation through the model-measurement comparison normally places emphasis on morning hours). It is hard to tell from Fig 5 whether VOCs are reasonably simulated or not. More comparisons on speciated VOCs (if measurements are available) would help to identify possible needs for further adjustments of the VOC emissions. In addition, a more detailed and clearer classification of the O3 pollution pattern is needed.

Specific comments

The comments are listed in the order of appearance in the paper. Several comments may converge to a same issue (such as emissions and PA analysis).

1. P26839, regarding the emission inventory (EI). Since the emission data is a major input to and a major uncertainty in the CTM modeling, and modeling results and conclusions can be altered by the emissions, it is necessary to briefly describe how the inner-domain high-resolution EI is constructed, and discuss its uncertainty. Also specify the resolution of the TRACE-P EI.

2. P26839, L8: specify the "some results". L18: specify what databases are input to BEIS (landuse, vegetation leafmass distribution…), and it's time and spatial resolution. What meteorological input is used to drive BEIS?

3. P26839, Ls19-25 and Table 1: The VOC/NOx ratio in the anthropogenic emissions is very low ($\sim$1.5) compared to many urban areas around the world, which alone suggests the O3 formation is VOC-limited according to the rule of thumb (even though it is dependent on the VOC composition). This ratio is lower than that in Los Angeles in 80s and in Mexico City in 90s. Any idea why the ratio is so low? How about it compared to other cities in China, say Beijing and Shanghai? It would be helpful to add the mean biogenic emissions of NOx and VOCs during the model period in Table 1, which gives the reader an idea the relative contribution of biogenic emissions.

4. P26840 Ls3-20: There are not descriptions of the measurement techniques for O3 and NOx (and maybe CO too, see Comment #7). In addition, explain why C2-VOCs are not included when compared with the on-line observation.

5. P26841, PA analysis and relevance in Section 3.3: The PA analysis is made on a single grid cell for each of the three sites. The three sites are selected to represent the areas, by my understanding, where O3 formation is mainly affected by local emissions (GDEMC), mainly affected by transport (Xinken), and comparably affected by both local emissions and transport (Donghu). The problem is that the single cell results may not be representative to these three area's conditions, in particular if a grid cell is heavily influenced by point or uncharacteristic highly localized emission sources. It would be more representative by including more surrounding cells in each of the grids.

6. P26843, Table 3, add performance statistics for NO2 (and NO) and NMHCs. Fig. 3, replace the time series at an unmentioned site, such as Huijingcheng, with Foshun. The Foshun data may help to illustrate the Category #3 pollution pattern. 7. P26843 Ls9-17 and Figs 4-5. First, from the way presented as in Fig. 5, it is difficult to tell if NMHCs are reasonably predicted or not, particularly when the canister samples have coarser time resolutions (3-hour span or whatever) than that of the model output. It would be more appropriate to use scatter plots. Second, it would be desirable to provide or discuss the comparisons of speciated VOCs. These comparisons may direct to a need for the adjustment of the emissions. This is important because the conclusion of the sensitivity study could be changed. Third, as mentioned in the general comment, it would be necessary to include NO comparison if observations are available. If the NO comparison is included, since the NOx emission evaluation would be focused on morning hours when vertical mixing also play a critical role in determining a sepcies's concentrations, CO (or other chemically relatively inert species) data may be needed to examine the vertical diffusion.

8. P26845-26846 on classification of the simulated O3 pollution patterns. Couple issues here. First, the description presented lacks clarity, particularly for Categories

#2 and #3, both formed due to the interaction between a weak synoptic circulation and the sea-land breeze, but hard to tell how they differ, which may lead to confusions. An example of the resulting confusion is indicated in Fig 11, where both Day 16 and 22 seem to belong to Category #2, but at Table 4 Day 22 is put in Category #3. I understand that a comprehensive classification would need a separate paper, but the authors should present a bigger and clearer picture about the synoptic flows and local circulations (and not just the surface flows), how they interact and evolve that lead to the different patterns. Second, are the simulated patterns consistent with the real world? The latter is more important in the context of air quality and health effect. In another word, upgrading from the "simulated pollution pattern" to the "pollution pattern" would be more relevant. Third, name the three categories that characterize the pollution, such as O3-South (O3S), O3-Southwest (O3SW) and O3-Nothwest (O3N), including at Table 4.

9. P26846-26847 on model layers and time used in the PBL process analysis. First, it would be more suitable to use simulated time-varying PBL height for the PA analysis of O3 production in the boundary layer. The 7 layers used do not represent well the PBL height most time of the day. Second, in different pollution patterns that reflect the interaction of synoptic and local circulations, the contributions to O3 formation of each process may vary significantly, particularly for the transport process. It would be more appropriate and insightful by conducting the PA analysis under different categories and summarizing the results. Averaging over the three patterns might smooth out and mask some important results.

10. P26848 Ls13-14 "the chemical process dominated the O3 enhancement from morning to mid-afternoon" is contradictive to P26846 Ls17-18 "the chemical process exhibited a significant consumption of O3 during the whole day"; positive CHEM term at Table 5 and in Fig 9 is also contradictive to Fig 8 where CHEM is negative throughout the day at GDEMC.

11. Fig 16. The data points are so intense that they mask each other. Consider diluting

the data points. Describe the sites Kaiping and Duanfen in Page 26852 (not just in the figure caption).

12. P26852 Ls13-16. This sentence seems not right. According to Fig 16, delta-P(Ox) decreases with increasing NOx/NOy in the VOC-reduction case, but opposite for the NOx-reduction case.

Technical

1. P26839 L8, personal communication, misses the contact person's name(s).
2. P26839, L18: add a reference for the Chinese plantation survey dataset.
3. P26851 L12, change fewer to less.
4. Fig 14, explain the ellipses in the figure caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26833, 2009.