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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nighttime observation and chemistry of HO_x in the Pearl River Delta and Beijing in summer 2006" *by* K. D. Lu et al.

Anonymous Referee #1

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This manuscript describes nighttime observations of OH and HO2* radicals during PRIDE-PRD 2006 and CAREBEIJING 2006 field campaigns and their comparisons to theoretical model simulation results. While HO2* was in fair agreement, observed OH levels were significantly larger than simulation. After detailed discussion on the possibility of artifact, the authors suggested possibility that the combination of additional ROx production and the recycle of OH from ROx through HO2 could bring the model simulation in fairly good agreement in OH. For processes, the authors implied that the additional ROx production would be explained by ozonolysis of unmeasured reactive terpenes at ample amount, or more possibly by the downward mixing of PANs as reservoir of radicals. The topic is well suitable to the scope of the journal. Although large uncertainty needs to be taken into account, the analysis is made on the best

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knowledge basis, with clear logical flow. Two major comments are that (1) the modeled (and observed, if available) PAN concentrations should be mentioned together with the estimated flux, to convince readers, and that (2) discussion on the resemblance of the night-to-night variations in the observed and modeled radical concentrations should be involved. Overall, I recommend publication after revisions with respect to the points raised above and the following minor comments.

1. Page 31316, line 8. It is better to mention that two separate cells are used for detection of each OH and HO2*. A single pump draws air from the two cells. Can the back diffusion of NO, added only to the HO2* cell, to OH cell be a problem, to measure OH and HO2* simultaneously in such a system, especially when the HO2*/OH ratio is large (>1000)?

2. Page 31318, line 11. How did the authors estimate concentrations of C2 species at BG site, as listed in Table 1?

3. Page 31319, Section 2.2. It is unclear if the model simulation was made for individual nights and then averaged to obtain the shown average time series (Figure 3 etc) or if only one model night with average concentrations of ancillary species was simulated. In the latter case, the titration relationship between O3 and NO will not be adequately represented in the model.

4. Page 31319, lines 16-17. List date of the seven and nine days specifically.

5. Page 31320, lines 16-17. Did the big differences in the CO and isoprene concentrations from night to night have correlation with radical concentrations, and indicate potentially important parent RO2 that have been recycled to OH?

6. Page 31321, line 16. What was the estimated average concentration level of HCHO? Was the 24-h lifetime regarding deposition important?

7. Page 31322, line 21. It is unclear what are meant by "large oxidation rates of the sum of reactive trace gases."

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8. Page 31323, line 3. Was the contribution of the O3+NO reaction included in the nighttime O3 turnover rate?

9. Section 4.1. Only average levels of the observed and modeled radical concentrations are compared. It is recommended to include correlation analysis between them, representing features of night-to-night variations.

10. Page 31326, line 9. Can the artefact from Criegee intermediate for example be common for LIF and CIMS instruments and thus be overlooked during the intercompaison studies?

11. Page 31328, line 18. comparison (not intercomparison)

12. Page 31330, lines 5-8. Do we expect large differences in the simulated NO3 concentration levels in the two model runs, as suspected from the large difference in OLNN and OLND? If yes, why was it?

13. Page 31332, line 25. 10**6 (not 10**-6)

14. Section 4.4.2. Can the authors evaluate simulated PAN concentrations, through comparison to observations if available, or to the levels typically present in the nighttime boundary layer? Can the PAN entrainment from the top boundary of the upper layer be also important, to increase the flux to the lower layer?

15. Section 4.4.2. Does Kz depend on z? The value for the 50-m altitude level was used?

16. Page 31334, line 8. How was the nocturnal temperature lapse rate used?

17. Page 31334. Can the dry deposition of the radicals on the ground be effective?

18. In Lu et al. (2012) and (2013) for daytime analysis for PRIDE-PRD and CARE-BEIJING, the authors discussed that the importance of the RO2-to-HO2 conversion was reduced, after considering RO2 artefact in the HO2* measurement. On the other hand, the manuscript indicated that the RO2-to-HO2 and HO2-to-OH conversion was

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required in the nighttime similarly to the daytime. Are they consistent to each other?

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