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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of ozone and particles in the near-surface atmosphere in urban area of the Yangtze River Delta, China" *by* Huimin Chen et al.

Anonymous Referee #2

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A major revision of the MS must be made. Reconsideration of the MS is only possible pending the responses from the authors to the points listed below. The MS reports the observational data but barely digs enough into it, let alone a sufficient and reasonable discussion without conceptual mistakes. Moreover, the MS is not comfortably readable and lacks brevity. There are quite a few grammatical errors to be corrected. It would also be better if the language could be polished in the revision.

Main points: 1. The structure of the introduction apparently lacks logic organization. Even more, major scientific issues the MS to be addressed are not clearly stated. 2. A detailed description of the environment where all instruments are installed should be given in section 2.1. How about the drying system upstream AE-31 and Aurora-3000? The instruments used to measure trace gases should be at least briefly described, instead of having not even a single word on that. 3. The SC2006 is adopted in this study to correct the systematic biases inherent in the principle of AE-31. What are the parameters used in your procedure? How about the values of your correction factors? The description needs to be more specific. 4. The truncation correction of Aurora-3000 is based on Mie calculations. If I understand it correctly, Mie calculation is not performed in this study, since obviously there is no measurement of particle number size distribution. Instead, correction parameters are directly taken from the literature in this study. How much uncertainty might be introduced to scattering coefficients due to the choice of the correction factors? 5. HYSPLIT model is driven by NCEP data with a temporal resolution of 6 hours and a spatial resolution of 2.5 degrees in this study. I doubt that the resolution is adequate for carrying a simulation of near surface transport process. 6. Page 14, Line 283-284, particles especially sub-micron particles could hardly be removed from the atmosphere by rain droplets. It is strong wind before the rain that sweeps them out. 7. Page 18, Line 385-386, the author states that the diurnal pattern of NOx is mainly governed by photochemical processes and meteorology. However, emission is a key factor that should not be ignored. 8. Page 22, Line 454-460, the concept about fog is completely wrong. Fog only occurs above 100% RH, though droplets can exist below 100% due to the hygroscopic growth of particles. 9. Page 22, Line 467-471, the existence of aerosols might affect solar radiation to some extent and thus ozone photochemical production (not always to a measurable amount). However, the main reason for the observed variation of ozone should not be attributed to aerosols. The author tempts to build a relationship between aerosols and ozone, but I find the analysis of data and deduction not robust and even incorrect, just like here and discussions elsewhere in the MS, e.g, Page 24, Line 503-504. 10. Page 23, Line 491-499, the author draws a conclusion that ozone photochemical production is VOC-limited by using CO/particle-O3-NOx relationship. I find it very unconvincing.

Minor points: 1. The full form should be given for the abbreviation of BTH in the ab-

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stract. 2. The abbreviation of several aerosol optical parameters such as AAC and SC is not common. It would be better to follow the convention in the community. 3. Page 13, Line 270-274, the sentence beginning with ‘For example’ is supposed to illustrate the point brought forward by the sentence before it. However, I find their connection rather confusing.

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