

Interactive comment on “Observations of the summertime atmospheric pollutants vertical distributions and the corresponding ozone production in Shanghai, China” by Chengzhi Xing et al.

Anonymous Referee #2

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General comments

This paper presents a comprehensive measured dataset of vertical profiles of aerosol extinction, NO₂, O₃ and HCHO at a rural site in the area of Yangtze River Delta (YRD) with well established MaxDOAS and lidar techniques in their research groups, respectively. The successful deployment of these new measurement techniques in Chinese megacity areas will potentially open up a broader view (especially over the vertical dimension) on the exploration of the air pollution formation mechanism. The retrieved NO₂ concentrations from the MaxDOAS instrument is assisted by the lidar observed

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aerosol extinctions and further validated by the balloon measured NO₂ concentrations. The well established MaxDOAS NO₂ concentrations were then used to validate the satellite measurement results over YRD. The lidar measured O₃ is compared with the chemical transport model results and showed reasonable good agreement. Overall, the authors presented a number of high quality vertical profiles of aerosol extinction, NO₂, O₃ and HCHO in the rural area of YRD for the first time and is an important and valuable contribution to the community of atmospheric chemistry which definitely worth publication in the journal of ACP. Nevertheless, some parts of the paper still need some further modification or polishment. For that purpose, I had the following comments for the consideration of the authors.

Specific comments

1. Figure 1 and related text, is the choice of Gaussian a priori really the best one? The selected lidar measurement were performed at 08:30 when the Planetary boundary layer was not fully developed. So the high aerosol concentrations appeared at around 500 m. Nevertheless, I assume the aerosol profile will be different for 12:00. And even for the lidar results of 08:30, the Gaussian a priori profile seems to be significantly underestimated the observed aerosol concentrations above 1 km. In this case, may be a Lorentz a priori would be better.
2. Figure 2 and related text, what was the time of the balloon measurement performed? The balloon measurement may take some time from lower to higher altitude so that the observed NO₂ profile is composed by both the vertical and temporal change of the probed air masses. Which instrument is used for the measurement of NO₂? As we know, the NO₂ measured by the Mo-CLD instrument will be strongly influenced by the NO_x in the atmosphere. Likewise, I suggest the authors to have more detailed description of the experimental part of the Vehicle-based tethered-balloon observations of PM and O₃.
3. Figure 5, if the NASA OMI NO₂ products is biased in such a way over the Chinese

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megacities, it is an important message to the community which needs more highlight in discussion and conclusion. Is the NASA OMI NO₂ products also strongly biased on the retrieval of the spatial pattern of NO₂ compared to that of the USTC OMI NO₂ product (shown in Figure 4)?

4. Figure 6-9 and related text (mainly Sect. 3.3 and 3.4), the authors have used WRF-chem simulated results to assist the observed O₃ vertical profiles for the diagnosis of the ozone sources. And it is argued the O₃ high values observed is locally formed which is probably related to the high VOCs and relatively strong solar irradiance. The currently analysis is strongly depending on the observational deduction. What does the WRF-Chem model tell? I encourage the authors to abstract more quantitative results from the used WRF-Chem simulations.

5. Line 308 -309, “High HCHO levels during the time are mainly contributed by the oxidation of biogenic emissions of VOCs from plants, i.e., isoprene.” Do the authors have evidences for such statement? At least a reference is needed for this statement.

6. Line 317 – 318, “Major sources of atmospheric aerosols in FengXian area could be the sea salt aerosols because the measurement site is close to the coast.” This is probably not true. In China, even for the area close to the coast, the aerosols were normally dominated by sulfate, nitrate, ammonia and organics. I suggest the authors to look up the available published results of aerosol chemical composition in Shanghai and surrounding areas.

7. The judgement of the secondary aerosol formation from a correlation of NO₂ and aerosol extinction coefficient seems to be quite arbitrary. And a $R = 0.63$ can not be well recognized as a significant correlation. More discussions are required to support this deduction. And the information by Figure 11 is very limited, I suggest to move this figure into supporting materials.

Technical comments:

Line 168, change 'Dependency' to be 'Dependence'

Line 227, USTC shall be explained when appeared for the first time.

Line 580, Figure caption of Figure 2, (a) ... as well as the correlation coefficients shown in the brackets. (b) ... shows the mean differences and standard deviations in the brackets ...

Line 626, Spatial distribution of USTC OMI tropospheric NO₂ ...

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