

Interactive comment on “Ozone production in the megacities of Tianjin and Shanghai, China: a comparative study” by L. Ran et al.

Anonymous Referee #2

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The paper deals with the ozone production in two densely populated and highly industrialized regions in VR China also presenting speciated hydrocarbon measurements of the two regions. The paper contains interesting material from a part of the world from which information regarding air pollution is still sparse. I only recommend publication if the following points are modified or considered in a careful way. Main points: 1. The authors point out that “In general, the ozone problem in Shanghai is on urban scale” (see page (p) 9163, line 17-18) ... Whereas “By contrast, the ozone problem in Tianjin is a regional problem” (see p. 9163, line 18-20): I don't believe, that this general conclusion can be deduced from the presented ground-based measurements because they only refer to very few ground-based receptor sites covering only limited sampling times. However, (only) when additionally including the NO₂ satellite measurements of OMI presented Figure 1 one might argue, that the related photooxidant problem is

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expected to extend over a much larger area in the Tianjin region (Beijing and surroundings) than the area of Shanghai. 2. I would prefer to describe the field measurements of the organic compounds as Non-Methane hydrocarbon measurements (NM-HC) instead of VOC-measurements because the term VOC (Volatile Organic Compounds) is much broader than hydrocarbons also including carbonyls, chlorofluorocarbons, hydrogenated chlorofluorocarbons, etc. which are not measured nor discussed in this paper. 3. It might be valuable to check the English by a native English speaker. 4. Do what extend differs the submitted paper from the papers of Ran et al., 2009 and Ran et al., 2011 ? Are the measurements discussed in this paper the same as presented in the earlier papers ? If yes which aspects are new in the submitted paper ? I expect clarification in this question in the last paragraph of the introduction (not only learning in the middle of the paper that several aspects were already discussed the earlier papers, e.g. p. 9173, line 17; p. 9174, line 9; p. 9174, line 17; p. 9178, line 13-15). Specific points: Abstract: 5. Page (p) 9163, line 9: I suggest to write: Two intensive ... at an urban and a suburban site of Tianjin and in Shanghai, in addition to ... Introduction: 6. p. 9163, line 26: The references of Streets and Waldhoff, 2000 and Klimont et al., 2001) seem rather old to me and I suggest to use more recent references, possibly referring to Van der A, R.J., et al. (2008), Trends, seasonal variability and dominant NO_x source derived for a ten year record of NO₂ measured from space, J. Geophys. Res. 113, D04302, doi:10.1029/2007JD009021 ? Experiments and methodology: 7. p. 9165, line 16: How large is the spatial extension of NCP ? Is the region 115oE to 114oE/37oN to 38oN part of NCP ? 8. p. 9165, line 16: I cannot see the Bahai Bay in Fig. 1a. 9. p. 9167, second paragraph: hydrocarbon sampling: The period of sampling is rather short (about one week, not simultaneously at the sites) so that the representativity of the measurements is restricted because of the variability due to meteorology. 10. p. 9167, second paragraph: You might refer to regarding analytical technique to Ran et al., 2009 (if applicable). 11. p. 9168, second paragraph, starting at line 6: Is this selection of measurements based on meteorological conditions only applied to O₃, NO_x and CO measurements or also to NM-HC measurements ? Results and dis-

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cussion: 12. p. 9172, Characterization of hydrocarbon measurements: only averages of NM-HC are presented in the paper: I suggest also to summarize the distribution of the individual hydrocarbon concentrations NM-HC, possibly as tables in an annex – e.g. presenting together with mean values the largest and smallest concentrations measured for the individual compounds at the 4 sites. 13. p. 9172, Tab. 1: For me it is quite remarkable (and could/should be explicitly mentioned in the manuscript), that the total amount of hydrocarbons in ppb is substantially larger at Wuqing than at Tieta despite the fact that Tieta is the urban site and Wuqing is the suburban site in the Beijing area (see e.g. Table 1) which is explained by the authors by the (large) evaporation of alkanes at Wuqing. 14. p. 9174, line 27: do you mean “. . . . Jinshan is a sparsely populated (instead of located) residential area”. 15. Table 3: How is “background” for NO_x defined ? Conclusions: 16. Last paragraph: I strongly recommend to include the NO₂ satellite measurements shown in Figure 1 to underline the argument that the photooxidant problem extends over a larger area in the Tianjin region than in the Shanghai region (comp. point 1).

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