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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volatile organic compounds and ozone air pollution in an oil production region in northern China" *by* Tianshu Chen et al.

Anonymous Referee #1

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General comments

This paper presents observations of NMHCs, OVOCs, O3, and other reactive trace gases made at a site in the Yellow River Delta during a winter-spring period and a summer period in 2017. The study area is very interesting because it is one of the largest oil and natural gas (O&NG) exploration areas in China. And it is a part of the North China Plain; a region suffers from severe air pollution. Some of the NMHCs and OVOCs samples were taken near oil wells and petrochemical industrial areas. The authors derived emission profiles of VOCs from oil fields. Such emission profiles were not available in China before. The concentrations of VOCs in ambient air over the

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Discussion paper

site were very high due to large emissions from O&NG exploration in the Yellow River Delta. The authors also studied the atmospheric oxidative capacity, radical budget, and ozone formation mechanism at the observation site using a box model constrained by measurements. The results show that O3 formation was mainly NOx-controlled due to high-VOCs and low-NOx conditions. OVOCs played a dominant role in OH reactivity and were the major source of ROx radicals. The low NOx level in summer limited the radical recycling.

The observational data reported in this paper are valuable. The emission profiles of VOCs from O&NG exploration are highly needed in air quality studies and emission control. The results from the box model study are interesting and important for such a special area. This paper is well written and within the scope of ACP. I recommend publication of this paper in ACP after the following minor points are appropriately addressed.

Specific comments and suggestions:

1. After reading this paper the readers would like to know the impacts of large VOCs emission in the Yellow River Delta on the air quality over the surrounding area. The summer concentration of NOx at the study site was low. However, the NOx levels over many other parts of the North China Plain are much higher. Photochemical O3 production may be very different when transported high-VOCs plume mixed with high-NOx air. To address this 3D simulation is needed, which may be out of the scope of this paper. But I think some discussions in this aspect are necessary.

2. Section 2.2 should include details about the calibrations and data quality.

3. Although HONO and OVOCs were observed, no observational data of these are presented. Since these species are very imported in your studies of atmospheric oxidative capacity and radical chemistry, I do not think these observational data can be omitted. I think HONO data should be included in Figures 2 and 3, and OVOCs data should be either presented in Figure 4 or in an extra figure.

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4. P13, L1-4 and Figure 11: why is the “NO+HO2 = NO2 + OH” reaction not included as a source of OH? If you have any reason not to include this reaction in OH production, you should not state “Photolysis of OVOCs is identified as the dominant radical source” (L2-3) because your Figure 11a shows OVOCs photolysis has only a minor contribution to OH production.

5. Section 6, first paragraph and P9, L18-20: your measurements show a rapid morning increase of O3 concentration. This increase may not only be resulted from photochemistry but also from vertical mixing. I suggest that you integrate the net O3 production rate in Figure 13 to get a diurnal profile of O3 based on box model simulations and compare it with the observed diurnal profile shown in Figure 3.

6. Some of the box model results can be compared with those from Chinese megacities reported by Tan et al. (2019). [Tan et al., Daytime atmospheric oxidation capacity in four Chinese megacities during the photochemically polluted season: a case study based on box model simulation, Atmos. Chem. Phys., 19, 3493–3513, 2019.]

7. P3, L15-18: when it comes to long-term trends of surface ozone in China, “Ma et al., Significant increase of surface ozone at a rural site, north of eastern China, Atmos. Chem. Phys., 16, 3969-3977, 2016” is one of the few important papers and should be cited.

8. P4, L23-26: “In view of the regional scale, the observation site is constrained by both aged continental air masses transported from the Beijing-Tianjin-Hebei region and clean marine air from the Bohai Sea, making it an excellent platform to study the interaction between anthropogenic pollution and the natural background air in the North China Plain (NCP)”. I do not think you can really sample “clean marine air from the Bohai Sea” or “natural background air” because you are so close to the oil wells.

9. P4, L27-P5, L2.: these sentences belong to section 2.2.

10. P5, L14: what is “SHARP”? This abbreviation should be explained.

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11. P5, L21-30: did you use O3 scrubbers when taking NMHCs samples?

12. P7, L20-21: “Photolysis frequencies within the model were adjusted by the solar zenith angle and the measured J(NO2) (Saunders et al., 2003).” Why did you adjust photolysis frequencies within the model when you had the measured values?

13. P9, L14-17: this statement applies only to summer.

14. P9, L26-28: can you estimate errors in the so obtained emission profiles?

15. Figure 11b: Since you single out HCHO photolysis, “OVOCs Photolysis” here should be changed to “Photolysis of OVOCs other than HCHO” or similar.

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