

Response to Referee #3

Thank you very much for your thoughtful and constructive comments on our manuscript. We have revised the manuscript accordingly. The detailed responses are given below point by point (in blue), and the revised manuscript is shown in red.

This paper describes one-year continuous monitoring of VOCs around an oil-gas region in northwest China in order to clarify atmospheric behavior of VOCs in such region. The authors revealed temporal variations such as seasonal and diurnal variations of VOCs around the oil-gas region and analyzed factors of such variations. In addition, they performed source analyses of VOCs and discussed source of VOCs in this region quantitatively.

General comments:

As the authors mentioned, VOCs are main precursors of tropospheric ozone and it is important to clarify atmospheric behavior of VOCs. Examples of VOC observations in oil-gas regions are low, especially; there are few continuous observations of VOCs with high time resolution. The authors supply valuable data and information. In addition, the authors conducted quantitative source analyses of VOCs. I recommend this paper to be published in Atmospheric Chemistry and Physics.

However, I found several dubious points in this paper. The authors should revise appropriately.

The authors would like to thank the reviewer for the detailed comments, which help to improve the manuscript. We have tried to clarify the points raised by the reviewer and to answer all remarks.

Specific comments:

The authors performed several discussions using NO_2 . Why do the authors use NO_2 instead of NO_x ? I think it is preferable to use NO_x instead of NO_2 (or both NO_2 and NO_x) for many of such discussions. The authors would observe NO and NO_2 because they used a TEI NO_x analyzer based on a chemiluminescence method.

Thanks for your suggestion that using NO_x instead of NO_2 to discuss, however, the NO_2 and other air pollutants data were from the Qingyue Open Environmental Data Center, which only NO_2 is available. So, we had to use the NO_2 to discuss and we will notice this point in our further study.

NO_2 and NO_x concentrations measured by a TEI NO_x analyzer are not accurate because of interferences of descendant species of NO_x such as HNO_3 and PANs. The authors should evaluate such interferences. Especially, organic nitrates could interfere the values of NO_2 concentrations obtained by a TEI NO_x analyzer under high concentrations of large hydrocarbons.

Thanks for your comments. There is a technic error in the manuscript that the NO_2 was actually measured using the automated monitors (TH-2000 series, Wuhan-Tianhong Instrument Co., Ltd, China), which also determine the NO_x using the chemiluminescence technic. Based on the chemiluminescence method, the measured NO concentrations is accurate. A molybdenum converter was used for NO_2 measurement, and as a result, part of the NO_y (e.g., peroxyacetylnitrate (PAN), HNO_3 , and alkyl nitrates) may have been transformed to NO_2 during the sampling. Therefore, a method developed by Lamsal et al. (2008) was used to correct our NO_2 data according to the following formula:

$$CF = \frac{NO_2}{NO_2 + \sum AN + (0.95PAN) + (0.35HNO_3)}$$

where CF is the correct factor, $\sum AN$ is the alkyl nitrates. However, in this study, we do not measure the PAN and HNO_3 and simulate the alkyl nitrates concentrations. Therefore, the NO_2 concentrations discussed in this study were considered greater than the actual values (Dunlea et al., 2007; Zou et al., 2015) and we used the average value of NO_2 concentration to discuss. We will notice this issue in further study.

Reference

Dunlea, E. J., Herndon, S. C., Nelson, D. D., Volkamer, R. M., San Martini, F., Sheehy, P. M., Zahniser, M. S., Shorter, J. H., Wormhoudt, J. C., Lamb, B. K., Allwine, E. J., Gaffney, J. S., Marley, N. A., Grutter, M., Marquez,

C., Blanco, S., Cardenas, B., Retama, A., Ramos Villegas, C. R., Kolb, C. E., Molina, L. T. and Molina, M. J.: Evaluation of nitrogen dioxide chemiluminescence monitors in a polluted urban environment, *Atmos. Chem. Phys.*, 7(10), 2691–2704, doi:10.5194/acp-7-2691-2007, 2007.

Lamsal, L. N., Martin, R. V., van Donkelaar, A., Steinbacher, M., Celarier, E. A., Bucsela, E., Dunlea, E. J. and Pinto, J. P.: Ground-level nitrogen dioxide concentrations inferred from the satellite-borne Ozone Monitoring Instrument, *Journal of Geophysical Research*, 113(D16), doi:10.1029/2007JD009235, 2008.

On page 9, lines 6-7, “It should be noted that VOCs: : : as well as BLH.”: I think NO₂ concentrations are controlled solar UV and concentrations of NO and O₃ as well as BLH, but are VOCs controlled concentrations of NO and O₃? (I don’t think so.) The authors should discuss this matter separating VOCs and NO₂.

Thanks for your comment. We have revised this part.

The VOCs had a reverse trend with O₃ ($r = -0.82$, $p < 0.01$). The lower BLH and less photochemical activities resulted in peak values for VOCs and low O₃ concentrations before sunrise (6:00 local time). After sunrise, with the initiation of photochemical oxidation and the increasing of BLH, the concentrations of VOCs decreased while the O₃ increased rapidly. The minimum of VOCs and occurred at about 12:00–14:00 LT was resulted from both dispersion or dilution conditions and photochemical reactions (with highest O₃ concentrations at 14:00 LT) in the afternoon. The diurnal variation of NO₂ was controlled by BLH, O₃ and photochemical reactions (i.e., OH radical) and showed a double peak.

Table 1: The authors should explain r^2 .

Done.