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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sources and photochemistry of volatile organic compounds in the remote atmosphere of western China: results from the Mt. Waliguan Observatory" *by* L. K. Xue et al.

Anonymous Referee #2

Received and published: 19 June 2013

Review of Xue et al., 2013

General comments:

-Does the paper address relevant scientific questions within the scope of ACP? Yes.

-Does the paper present novel concepts, ideas, tools, or data? The measurements are valuable.

-Are substantial conclusions reached? Fairly true generally.

-Are the scientific methods and assumptions valid and clearly outlined? Overall good, but the authors still need to include details in method and results sections (see specific comments).

-Are the results sufficient to support the interpretations and conclusions? See specific comments.

-Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Fairly true.

-Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Many of the references cited are old.

-Does the title clearly reflect the contents of the paper? Yes

-Does the abstract provide a concise and complete summary? Overall good.

-Is the overall presentation well structured and clear? Overall good.

-Is the language fluent and precise? Overall good.

-Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Overall good.

-Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See specific comments.

-Are the number and quality of references appropriate? Need to cite newer studies.

-Is the amount and quality of supplementary material appropriate? NA.

This paper describes the VOCs seasonal (spring and summer) and diurnal variability (noon and midnight) at Mt Waliguan Observatory located in southwestern China in 2003, and discusses the VOC speciation in the airmasses with five origins indicated by back trajectories. The authors then used the measurements to constrain a chemical box model to understand the photochemistry there. The objective is clear and impor-

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tant, the discussed science questions are within ACP's scope, and the measurements are valuable at an important location in Asia. Therefore, I recommend the paper to be published after the following concerns are dealt with.

Specific comments:

1. Introduction

P11747, line 8-9: "In the lower troposphere, the photochemistry is more important than the stratospheric ïňĆux in controlling the ozone budget (Crutzen, 1995)." Recent studies have shown that the lower tropospheric ozone, especially at remote high-elevation regions may also be strongly affected by stratospheric intrusion during spring/summer. In page 11749, the authors cited a paper by Xue et al. 2011, showing the importance of stratospheric intrusion to ozone in the studied region in this paper.

P11747, list of reactions: I think they are somehow redundant for this paper. You did not describe all of them in the following text. Perhaps cite previous studies, and only the ones most relevant to WLG chemical regime should be kept.

P11748, the paragraph from line 7: "long-range transport" is a vague word in the context here. The authors should clearly define them using the terms of "hemispheric", "trans-Pacific", "transport from Europe to Asia", "import and export", etc. In addition, the references cited regarding the transport are relatively old. E.g., the sentence: "Moreover, there have been very few studies of the long-range transport of air pollution to East Asia from the upwind continents such as Europe (e.g., Wild et al., 2004)." is too strong. There are recent studies by the HTAP communities using multiple models regarding the impacts of transport of pollution from different source regions on east and south Asia pollution levels.

P11749, line 3: "Since then, ozone, greenhouse gases and aerosols have been measured continuously along with complete meteorological parameters (Tang et al., 1995; Ma et al., 2003; Zhou et al., 2003, 2004; Kivekas et al., 2009). These observations

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have yielded invaluable insight into the composition and chemistry of background atmosphere in the Asian continent" -Tropospheric ozone is an important greenhouse gas. -Why aren't they valuable? The authors described the findings based on ozone and meteorology measurements in the following paragraph. Maybe just remove this sentence.

P11750, line 3-9: maybe better to add the related section numbers when introducing the paper structure

Fig. 1: Maybe better to add latitude/longitude. You have compared your measurements with those at other mid-latitude remote sites in Section 3.1.2, and TRACE-P data in Section 3.2.2. I'd be useful to include Mt. Tai and Happo (which are also located within the big domain) in this figure. You should also denote the locations of TRACE-P samples in the figure.

2. Method

P11751, line 2: "aimed to study..."→aimed at studying the impacts of non-local sources in spring/summer. You are going to discuss in specific the airmass source regions in Section 3.

P11751, line 18-22: please clearly define the sampling frequency. E.g., What do "continuously" and "routinely" mean?

P11751-11752, Section 2.2: I think the author should include more details regarding the trajectory analysis in this section:

- Backtraj is not only useful for high-elevation sites. The authors could also add some references in which backtraj were used for source attribution analysis. There are many of them.

- How good are the FNL meteorological fields over these regions with complex topography? Citing some papers maybe helpful. What is the actual topography in the grid? Adding the spatial resolution and temporal frequency of FNL maybe useful.

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- What is the method/criteria you used to define the five regions for your trajectory grouping?

P11753, paragraph from line 16: Since you only analyzed one day for each period, how representative are they for the spring and summer periods? Therefore, the conclusions based on such results indeed need to further explored by extending the study period. Please also spell out the abbreviations before using them in this section.

3. Results

P11754-11756: I think this section could be better organized.

-Table 1: how big is the detection limit? Need to add in the notes. The VOC species in the table may be better ordered/grouped for us to read: e.g., based on the major sources lifetimes, or/and the order they are discussed in the text

-Emissions and concentrations of VOC species from biosphere such as isoprene are not only sensitive to temperature, but also others such as radiation, etc, which can cause the strong seasonal variability.

-Table 2: in the notes, Cimone. . .m.asl. messed up (typesetting issue)

P11757-11758: - Please consider rewording the title of “influence of long-range transport”

-The authors included the VOC emissions in Fig. 3 together with the trajectories but did not discuss them in detail. The emissions plotted did not reflect the biomass burning emissions. Also, the anthropogenic VOC emissions along the “North” trajectory are low, but the VOC measurements (Table 3) in these airmasses are higher than “NW” and “NW-MT”, close to the highly-polluted “NE” and “SW”, may be useful to add some explanations on this.

P11758-11759: I am not very convinced by the idea of direct comparing the TRACE-P data in 2001 with the WLG measurements in 2003 in this study, to represent the

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inflow/outflows of “China sub-Continent”. I am also not convinced by the conclusions in this section. Specifically,

1) The emissions (anthropogenic, biomass burning, etc) and the meteorological conditions may vary significantly for these two periods. You have mentioned that CO measurements during TRACE-P were higher than measurements in this study. However, previous study (e.g., Tang et al., 2003) has shown biomass burning could have a big impact during TRACE-P. At least the authors should discuss these differences between these two years.

2) TRACE-P data you used covered a larger area in E. Asia, rather than measurements at a single location (this study).

3) I doubt whether WLG measurements are representative of the inflows (transported from Europe). The Fig.7 (screenshot below) in a study by Lin et al. (2010) indicated that the impacts of European emissions on E. Asia air quality were spatial varying during TRACE-P period, and the strongest impact did not appear to be at WLG based on their conclusions.

4) The Lin et al. (2010) paper also showed that outflows of CO, O3, PAN can be extended to 6-9 km during TRACE-P, indicated by observations and models. It’d be useful to compare your conclusion of “Emissions in China may not have significant influence on the free tropospheric outflow” with theirs.

5) The authors should be careful about using the words of “airmass(es) from Europe” and “European pollution”.

References

Lin, M., Holloway, T., Carmichael, G. R., and Fiore, A. M.: Quantifying pollution inflow and outflow over East Asia in spring with regional and global models, Atmos. Chem. Phys., 10, 4221-4239, doi: 10.5194/acp-10-4221-2010, 2010.

Tang, Y., et al. (2003), Influences of biomass burning during the Transport and Chemi-

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cal Evolution Over the Pacific (TRACE-P) experiment identified by the regional chemical transport model, J. Geophys. Res., 108, 8824, doi: 10.1029/2002JD003110, D21.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11745, 2013.

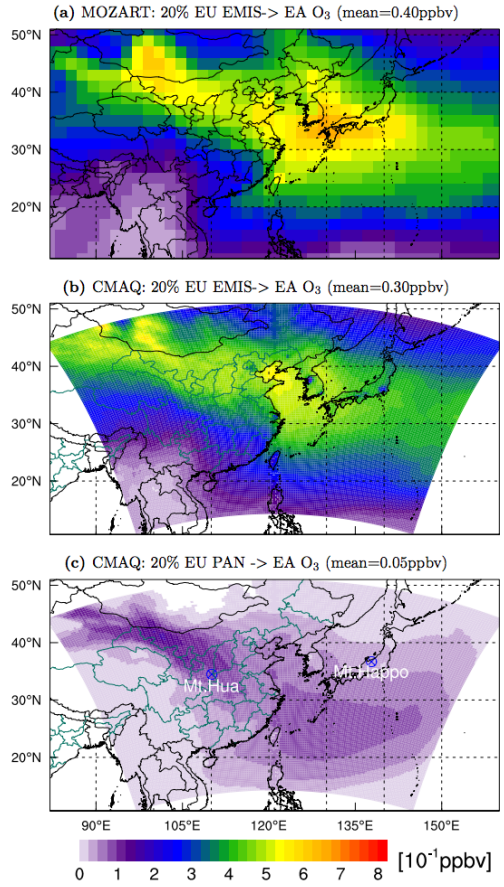

Fig. 1.

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