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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 #1

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The Waliguan Observatory (WLG) is an WMO global atmosphere baseline station, located in the northwestern part of the Tibetan Plateau. Observed levels and variations of O3, CO and relatively long-lived VOCs at WLG generally reflect the chemical and transport processes of these species on a large (Eurasian or Northern Hemisphere) scale. This paper by Xue et al. reports VOCs measurements made in the late spring and summer of 2003 at WLG. The authors also investigate the chemical budget of O3 and radicals with a chemical (Master Chemical Mechanism) box model, constrained by measured VOCs and other species including NO and O3. This kind of study is

very import for understanding the chemistry of the remote atmosphere as well as the perturbations from human activities.

The VOC data reported in this study is valuable, and the modeling method they applied appears to be fine, although some parameters and simulation results need to be introduced in detail. In contrast to previous modeling work, the authors show net production of ozone at WLG in summer. This finding is very interesting and needs investigating further. On the other hand, I do have a doubt about their method used to access the pollution inflow and outflow of the China sub-continent. I am also too conservative to accept their conclusion in that transport of anthropogenic pollution from central and eastern China can SIGNIFICANTLY perturb the chemistry of the background atmosphere over the Tibetan Plateau. I think that VOCs measurements and modeling work presented in this study may still have a big gap to attain this conclusion. In my opinion, the paper can be considered as accepted after major revisions having been made in response to my comments and questions below.

1. The method used to access the inflow and outflow of the sub-continent of China does not sound. What geographic domain does "the sub-continent of China" refer to in this study, the eastern China or the entire China area? In either case, measurements at a ground station like WLG cannot be used directly to infer an inflow, especially considering that the air masses tend to undergo substantial aging and mixing processes while being transported. Moreover, the authors did not provide a strong evidence to demonstrate that the air masses encountered during TRACE-P had originally come from (or passed by) WLG, partially mixed with emissions from eastern China, and then transported to the west Pacific. A simple arithmetic operation as performed in this study seems not sufficient to obtain such a conclusion as "the surface emission in China may not have significant influence on the free tropospheric outflow". Therefore, I would recommend Sect. 3.2.2 and Table 4 (as well as related conclusions in the abstract and Sect. 5) being skipped.

2. In contrast to previous work, this study has derived a net ozone production from

in-situ photochemistry at WLG. In addition to the NO level, which appears to be the most important factor in determining the net ozone production at WLG, the authors may want to have a discussion on the effect of the differences in the concentrations of other chemical species. While NO being constrained by measurements, how was NO2 treaded in their box model simulations, and how about its mixing ratio value(s)? What are the levels of intermediate products such as HCHO and H2O2 predicted by the model? Are the mixing ratios of NOy simulated by the model comparable to its measured values?

3. One of the important conclusions of this study is "the transport of anthropogenic pollution from central and eastern China can significantly perturb the chemistry of the background atmosphere over the Plateau". The authors might want to consider making this argument weaker and narrower according to the following aspects. First, being located in the northwest corner of the Tibet Plateau, WLG cannot represent the main body of the plateau, which is rarely influenced by air masses from central and eastern China during summertime. Second, the authors do not provide any evidence to show increasing NMHCs at WLG with increasing pollution in central and eastern China. They should make a comparison of the levels of NMHCs at WLG measured in this study with those made in previous years, instead of listing the results from Mt. Tai experiment in Table 2. Nevertheless, while this study focuses on photochemistry of volatile organic compounds, the authors have shown by their model tests that the ozone production at WLG is insensitive to CH4 and NMHCs. Third, the authors observed much higher levels of NO in this study than from previous work. However, they could not make it clear whether such discrepancy was due to an increasing influence of pollution or the difference in the experimental method. Even if an increasing trend of NO was found, the cause from local emission sources (within the plateau) should be excluded before reaching such a conclusion.

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