

***Interactive comment on* “Measurements of volatile organic compounds in the middle of Central East China during Mount Tai Experiment 2006 (MTX2006): observation of regional background and impact of biomass burning” by J. Suthawaree et al.**

**Anonymous Referee #2**

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GENERAL COMMENTS

The manuscript “Measurements of volatile organic compounds in the middle of Central East China during Mount Tai Experiment 2006 (MTX2006): observation of regional background and impact of biomass burning” by Suthawaree et al., presents a data set potentially very interesting. The literature on the distribution and characterization of VOC in China is greatly increasing (especially within the past 10 years), but new mea-

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surements can improve the present knowledge of Chinese emissions and can document the change in emission pattern from China. The “Introduction” is very well organized and comprehensive. However, in the reviewer opinion the manuscript (as it was submitted) is not ready to be published in a high profile journal such as ACP for the reasons listed in the comment section.

#### SPECIFIC COMMENTS.

1- The experimental part lacks important information about the quantification of the reported VOCs. For instance, there is no discussion on the use of the standards used to quantify the VOC, the limit of detection, accuracy, and precision of the measurements.

2- The discussion about the halocarbon fraction is worrisome. The minimum levels reported for CFC-11 and CFC-12 (215 and 483 pptv) can be justified only if representative of stratospheric intrusion (which most likely can be excluded for this set of samples). An average value of 511 pptv for CFC-12 and 232 pptv for CFC-11 in 2006 is hard to believe. The NOAA global monitoring program is reporting for the month of June 2006 an average of  $532 \pm 1$  pptv for CFC-12 and  $249 \pm 0.4$  pptv for CFC-11. The average values reported in this manuscript for CFCs have not yet been reached despite the declining in CFCs recently observed as result of the implementation of the Montreal Protocol. Also, in the text it is stated that the average concentration of CFCs reported in Table 1 were comparable to the background levels reported by Barletta et al (2006) for China’s background. In the Barletta et al. paper, the reported background was calculated from TRACE-P, a project carried out in 2001. It is not meaningful to compare regulated halocarbons, such as CFCs, among data sets that are 5 years apart (TRACE-P 2001 vs Mount Tai, 2006). Moreover, in Barletta et al (2005) the CFC-12 and CFC-11 background levels were reported as 535 and 259 pptv respectively. These values are not at all comparable to the 511 and 232 pptv reported in this manuscript for CFC-12 and CFC-11.

3- The authors keep discussing about “concentrations” (see also axis labels in Figures

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1 and 2) while “mixing ratios” are reported. This oversight is not acceptable for a publication in ACP.

4- Section 3.1.1 line 23 (p. 16722), the weak enhancement of n-hexane could be the result of lack of n-hexane sources in the region. The age of air masses is quite a complex issue and the overall levels of n-hexane are not generally used as indicator of air mass aging.

5- Table 1 does not indicate the units (which are clearly pptv, but still it needs to be indicated). The average level of CH<sub>3</sub>Cl is incredibly high for a remote location (990 pptv) with a maximum of 2 ppbv. These levels are often hard to be reached in a urban center. The last sentence of page 16722 (“This also supports the belief that the air masses were aged and well mixed before reaching to the site”) is definitely not supported by the average CH<sub>3</sub>Cl measured at this site and it is not consistent with the last sentence of section 3.1.1 where the region is found to be affected by emissions from biomass/biofuel burning. Is Mt Tai representative of the regional background? Or is it directly impacted by emission sources such as biomass burning? Also, bearing in mind the change in the global background of many VOCs (halocarbons in particular), the authors should indicate when a background level was measured. For instance, the reader should know when the background of 520-560 pptv and 8-9 pptv reported for CH<sub>3</sub>Cl and CH<sub>3</sub>Br was observed. Finally, CH<sub>3</sub>Br is emitted by biomass/biofuel burning but it has other important anthropogenic sources such as fumigation and vehicle emission (for cars using leaded gasoline).

6- Section 3.4. It would be useful to have an error associated with the slopes calculated for the different correlation plots (Figure 4,5,6,8).

7- Figure 7. The distribution profile of the alkenes looks quite similar too, with 1,3-butadiene being the only alkenes with noticeably different.

8- It is not clear to the reviewer why the profile illustrated in figure 7 indicates impact of biomass burning at Mt Tai. The profile indicated higher normalized levels in Beijing

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with respect to Mt Tai. This is consistent with Mt Tai being a remote sampling site and Beijing being an urban center characterized by the presence of many VOC sources.

#### TECHNICAL COMMENTS.

1. Page 16722 line 8, the sentence starting with “In brief, ...” needs to be rephrased, it is not clear what the subject is.
2. P. 16722, line 20 the verbs are not consistent. “Chan et al. (2006) found. ...was the most abundant VOC. ...” (rather than “...is the most abundant VOC. ...”).
3. Section 3.4. Please indicate how many samples were collected in Beijing and the sampling time (daytime or nighttime samples).
4. Figure 1. It would be useful to indicate which are the nighttime samples and try to find an explanation about the considerably lower levels measured between June 22 and June 25.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16715, 2009.

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