

Interactive comment on “PTR-MS measurements of non-methane volatile organic compounds during an intensive field campaign at the summit of Mount Tai, China, in June 2006” by S. Inomata et al.

Anonymous Referee #2

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General comments

This manuscript reports the VOC measurements by PTR-MS during a field campaign at Mountain Tai. Several excessive ratios of VOCs to CO influenced by a biomass burning source are also reported. The paper presents a valuable VOC dataset for understanding the regional VOC levels and the photochemistry in the central east China. However, it is not well organized or structured, lacks clarity and lacks in-depth analysis and discussions. Two areas in the analysis and discussions need to be improved. First, as stated in the paper, the objectives of this measurement study are to improve

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the understanding of the photochemical and transport processes of ozone and aerosol production, and help identify emissions sources and improve the regional emission inventories, but there are not enough analyses and discussions on the implications of these measurements to the objectives. Presenting data is important, but more important is to present what do the data tell us about. Second, due to the fragmentation interference, the current data form may find limited applications, especially in emission evaluation and model constraint where exact speciated VOC data are needed.

A major revision is needed before it can be considered for publish in ACP.

Specific comments

1. Abstract. Abstract is the concise summary of the important findings of a study. To me this one is not inclusive and not well written. The authors emphasize on the biomass burning affected one-day event, but the regional representation of most measurements that they collected should be equally, if not more, important.

2. Introduction. (a) I wonder why the authors put relatively a lot of efforts on the overview of the VOC measurements in China nation-wide (2nd paragraph), but almost none in the region (CEC) they investigate. Although the former is informative, more overviews should be focused on VOC studies in CEC, which is closely relevant to this study. (b) The MTX2006 campaign should be described, so that the reader can understand what the “we continuously measured” means, which appears several times in the text without preceding.

3. Section 2.2. Need to describe the environment of the sampling site, particularly the local emission sources, which is important to interpret the diurnal variation of measured species presented in section 3.3.

4. PTR-MS calibration. A fragment of the calibration appears in the end of P26702, where only 7 species are calibrated. Another fragment appears in the beginning of P26705, where 11 species are calibrated. This is hard to follow and not consistent. In

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addition, how about the calibration and detection limits for the rest ~20 species? Does the paper only present the 11 species/clusters? It is not clearly stated.

5. Section 2.4. It is not clear whether the GC-FID measurement/analysis was conducted by the same group or not. If it is, the title (and abstract, conclusion, etc) should be modified; if not, clarification and references are needed.

6. Section 3. PTR-MS correction. Concentrations of four species are corrected by inter-comparison with GC-FID data. What about the rest species? The overwhelming impression I get when reading through sections 3.2 and 3.4.2 is that the PTR-MS data need to be used with caution, especially in applications to emission estimation and model constraining. More discussions and explanations on this matter would help.

7. P26706 Ls 6-10: “The peaks at m/z 129, 143, and 157 were assigned to naphthalene and methyl-substituted naphthalenes. . . . however, The peaks at m/z 129, 143, and 157 were assigned to saturated C8–C10 aldehydes/ketones in this study”. I am confused.

8. P26707 Ls9-10 and Fig 2e. There is hardly a linear relation between M136 and monoterpenes.

9. Section 3.3. (1) Episode selection. The reader would not understand why the period of June 24-28 is chosen until he/she reads through the middle of P26714. (2) It seems that the PBL evaluation is critical to understand the diurnal variation of most species. For example, NO_x concentrations jump up between 6-8 am, which is more likely due to local emissions instead of development of PBL (At this time the observation site is located in the free troposphere according to the many VOC data). Add a brief discussion of the in situ PBL evolution.

10. Section 3.4. (1) It may be better to change the title to something like “Inter-diurnal variations of NMVOCs” (to differentiate from Section 3.3). (2) Add a table to summarize the PTR-MS VOC concentrations and statistics. (3) P26709 Ls 23-25. Unless benzene and toluene share the same emission sources with equal emission rates, this statement

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is not true.

11. OVOC dominance. The authors conclude that OVOCs are the predominant VOCs, but I don't find how this conclusion is reached. If it is based on their contribution to measured VOC concentrations, it would be problematic, since as the authors admit (P26710), the majority of NMHCs are not measured by the PTR-MS.

12. P26710 L23. Describe how the PTR-MS HCHO concentrations are corrected, and include references if necessary.

13. P26713 Ls 13-14: "Model simulation showed that the episode was related to open burning of biomass (crop residues)". That the BB event was identified by a model study, instead of by measurements, detections, or first-hand reports, sounds weird to me. The measurements presented in this paper, especially Fig 6a, are sufficient for the identification of the BB event. In addition, if available, describe how far the BB sources are from the observation site, which would help to discuss the BB-affected excessive VOC/CO ratios in Section 3.5, and how representative is the agricultural BB source in the region.

14. Table 2. The side-by-side comparisons of the excessive VOC/CO ratios may be misleading, because the referenced numbers are the BB emission ratios, while the values in this study are probably contaminated by other pollution sources (P26714-26715). In addition, adding more species in Table 2 would be valuable.

15. Table 3 and relevance, The table caption indicate the ratios are for the non-BB episodes, but the description in the text (in Section 3.5 and abstract) sounds as if they are for the BB event.

16. P26715 Ls 7-8 (and in the Summary): "which suggests that the photochemical production of OVOCs or the emission ratios were higher in China than in the United States". This is overly interpreted—the measurements at Mount Tai do not represent the nation-wide conditions.

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