

Interactive comment on “Measuring OVOCs and VOCs by PTR-MS in an urban roadside microenvironment of Hong Kong: relative humidity and temperature dependence, and field inter-comparisons” by Long Cui et al.

Anonymous Referee #2

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Cui et al. show VOC measurements from PTR-MS in Hong Kong. They also discuss humidity dependence of formaldehyde in PTR-MS and inter-comparison with several other techniques, including DNPH, canister samples, online GC-FID. However, this manuscript generally does not provide much new information and new technique. Many valuable experiences from over 20 years of work in the PTR-MS community are not fully reflected in the data processing procedures in this manuscript. The inter-comparison results are not as good as those previously reported in the literature, but the authors do not provide good reasons about it. Thus, I do not think this manuscript is suitable to publish in AMT, unless this manuscript is totally re-written and provide more information

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that can support new idea/technique.

Specific comments

1. Hong Kong is just one city in Pearl River Delta region. Thus, the authors might want to introduce more previous work in PRD region, rather than just Hong Kong. Several researchers have reported PTR-MS results in PRD region [Wang et al., 2016], and more in other parts of China, but the authors do not acknowledge these references in the introduction.

2. Formaldehyde humidity dependence in PTR-MS. Several papers have discussed this issue previously [Inomata et al., 2008; Vlasenko et al., 2010; Warneke et al., 2011]. Especially, Valsenko et al. derived explicit equations to fit the sensitivity vs. absolute humidity, based on the equilibrium between forward and backward reactions. In this manuscript, the authors tried to fit the observed rate coefficient (k) and sensitivity with relative humidity, temperature and absolute humidity. All of these tests are just using one dataset with different parameters/equations to derive curves for the correction, but without knowing the physical meaning of parameters/equations. The best way to do it should use the equations shown in Valsenko et al. Thus, Figure 2-8 should be replaced with one Figure similar to Fig. 5 in Valsenko et al.

3. PTR-MS operations: how often the instrument is calibrated, how is background determined, how often do you do background, how often the humidity dependence curve for formaldehyde is conducted (just one in 2 years?).

4. PTR-MS data processing: how signal is normalized, do you see sensitivity drift with time, how background is interpolated (especially formaldehyde). The authors should apply the widely accepted data reduction methods shown in many previous papers (most important ones [de Gouw and Warneke, 2007; de Gouw et al., 2003]).

5. Inter-comparison: In addition to PTR-MS, the QA/QC procedures should be provided to evaluation the data quality. Previous studies also found many limita-

tions/interferences for DNPH method, such as formaldehyde [Wisthaler et al., 2008], aldehydes [Herrington and Hays, 2012] and ketones [Ho et al., 2014]. Without acknowledging these interferences, the inter-comparison is meaningless. From Figure 9 in the manuscript, the bad agreement for acetone might be a DNPH issue.

6. Eq. 7: If you use m107 to calibrate C2 benzenes, then C2 benzenes should not be corrected. Where is 0.2235 from? Note that only ethylbenzene fragments significantly. Thus, Eq. 6 should be $[\text{Benzene}] = [\text{m79}] - 0.2235 * [\text{m107}] * f$. f is the fraction of ethylbenzene in ethylbenzene+xylenes, which can be from GC-FID measurements. Then, PTR-MS measurements would somewhat rely on GC-FID measurements. How would this reliance affect inter-comparison?

7. Figure 1 should only contain the periods with only PTR-MS measurements. The time series of VOCs should be also shown.

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