Atmos. Chem. Phys. Discuss., 8, S334–S338, 2008
www.atmos-chem-phys-discuss.net/8/S334/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mixing ratios and eddy covariance flux measurements of volatileorganic compounds from an urban canopy (Manchester, UK)" *by* B. Langford et al.

Anonymous Referee #2

Received and published: 23 February 2008

This manuscripts describes part of a measurement campaign in Manchester, UK. Flux measurements of VOCs from above the urban environment are presented. While this is a welcome and important addition to ongoing field measurements of VOCs (and other trace gases), the conducted research is surprisingly poorly described with many important pieces of information omitted. I have serious doubts about the data validity, and I suggest giving the study another, more detailed look. The authors might want to resubmit after the listed talking points have been successfully addressed.

My fellow reviewers have done an amazing job in pointing to the many shortcomings of the manuscript. Therefore, I will not reiterate in detail what has already been listed, but focus on some other details not previously mentioned.

The most important shortcomings from my perspective that have already been partially addressed by the other reviewers are

- lack of detail on PTR-MS and misinterpretation of what the "transmission curve" is and means;

- very short dwell time (20 ms) in the vDEC method and resulting potentially (very) high noise level. Even for commonly high fluxes measured under high parent ion abundance and dwell times (2E6 cps, 0.2 s) the reviewer has found that the noise level affects the high frequency end of the cospectrum (see also comment by referee #1);

- should it be m/z 37 on page 251 (not m/z 39), aka the first water cluster?

- measurement setup relative to roof environment (see also below); the rule of thumb is that the vertical distance from the rooftop should be at least two times the horizontal extension of the roof to avoid the worst;

- m/z 69 is likely going to have other interferences in a city besides just furan - benzene comparison with emission inventory (see below)

In addition and extension to these shortcomings:

1. One of my main criticisms is the lack of detail on the site description. An urban site is by nature heterogeneous, and the urban fluxnet community (http://www.indiana.edu/˜muhd/, or Oke, 2006) has developed criteria to describe the environment, and to locate an appropriate measurement site (the building used in this study is far from ideal). Very important is the characterization of the surrounding urban morphology, which determines air flow and turbulence parameters, such as displacement height and roughness length, z0. I see no efforts the authors made in that respect although at least one of them has previously published such. A quick look at the internet (e.g. http://www.aidan.co.uk/photo4688.htm) shows that the Portland tower has an extensive (rectangular!) footprint and is obviously in the neighborhood of at least

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one taller building (the Beetham Hilton Tower, to the N). Aside from that, using Google Earth for viewing, it appears to me that the tower height extends significantly past the surrounding buildings (~2 times?). These are important observations: The former suggest that there is at least one extensive wind direction sector that is influenced by a wake from a neighboring building. The latter suggests that the wake produced from the Portland tower itself is going to be large and varying with wind direction due to its rectangular shape. Even though the authors made an effort to escape the building wake effect, an up to 15 degree effect was observed (page 255). I think the stated dependence should be shown and explained together with an analysis of the urban morphology of the surrounding areas. Otherwise, no confidence can be created in the flux data presented. In that respect the statement that "although the mean airflow at the anemometer is affected by the building, the influence can be compensated by standard rotational corrections" (page 255) is misleading, because it suggests that the measured flux under these conditions is representative of the urban areas surrounding the tower (as calculated with a footprint model), which is not the case. Rather it may be representative of the immediate surroundings of the Portland tower, or not at all (see below).

2. As there is no detailed description of the surface and activities from which the measured fluxes are expected to come from, and there are no vertical measurements of wind speed and turbulent structure, one cannot assess what depth and therefore influence the roughness sublayer has on the measured fluxes. The review of Roth (2000) suggests that the roughness sublayer depth over cities may be at least 2, likely closer to 4 times the average roughness element height. Without at least some estimate of the latter, the given measurement height of 95 m and its effective reduction by the building's wake effect tells little about the flux measurement validity.

3. Meteorological data is given and I assume it was coming from met instrument installations on the Portland tower. If so, the authors need to realize that low wind speeds at such height probably occurred alongside a decoupling from the surface emissions,

and high wind speeds may be biased due to the wake effect, as are the wind speeds statistics as a whole. Similarly, measured temperatures should be viewed with caution.

4. Related to the above are the footprint calculations presented on page 264 and in Figure 9. It is unclear whether the input values were taken from typical onsite measurements. The assumed roughness length is most likely an underestimate in these conditions (Roth, 2000). If the authors want to analyze their data properly, they ought to make a better z0 estimate, possibly as a function of direction, and also present the traffic counts collected, assuming traffic is the major contributor to the emissions. It is unclear to me how the footprint analysis was used to compare to the emissions inventory, as the latter does not normally come in the shape of a footprint. Hence, some spatial extrapolation must have occurred aside from the temporal one discussed in the manuscript.

5. VOC data

a) One thing about the data that confuses me is the lack of a morning rush hour peak, both in the concentration data and the flux data. Unfortunately, Figure 3 does not have enough detail to evaluate this properly. What it does show is a buildup of concentrations under low wind speed conditions (e.g. Thu 8), a small weekend effect for toluene, and lower OVOC abundance after the frontal passage. But this is trivial, and the low wind speed conditions are generally conducive of low u* values, making flux calculations invalid. What this may show though is that the measurement location was far from ideal for the purposes of this study (see above). If the morning rush hour was not picked up due to too low wind speeds, and emitted VOCs are vented quickly instead (Wed 14?), and/or bypass the sensor due to the building's wake effect (most of the time?), measured fluxes are invalid for comparison to emission inventories. Even if emission rates are low due to a presumably "clean" carfleet (page 264/265) a morning rush-hour peak ought to be observable in a city. Unless the measured toluene (and benzene) fluxes correspond to the collected traffic counts (showing a rush-hour at 11-12 UTC (10-11 local time?)), that is, in my opinion, a clear sign of a fundamental flaw

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in the measurement design (see above), or simply of an insufficient length of data collection (more time needed to observe situations of sufficient turbulence during rush-hour times). Alas, I suggest the authors show the traffic counts and plot u* alongside the measured VOC fluxes to remove the guesswork (note that the afternoon rush hour appears to be observed for toluene.

b) Much of the spread in correlations shown in Figure 4 is trivial which has been pointed to by my fellow referees. I suggest the authors compare these data to existing urban measurements (e.g. Munich), particularly with respect to the range observed, which appears quite small to me, and may be the result of the duration of this study and/or the instrument sensitivity.

c) The parts about isoprene and the B/T ratio are superfluous. They contribute little to the manuscript and contain speculative discussion. I suggest referring to Holzinger et al (1999) on the "isoprene" data. There is probably an interference at m/z 69, and isoprene is a tailpipe emission (cited in Table 2; not evaporative because not a significant component of gasoline).

References:

Roth, M., 2000, Review of atmospheric turbulence over cities. Quarterly Journal Of The Royal Meteorological Society. 126(564): p. 941-990.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 245, 2008.