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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of volatile organic compounds in the polluted urban atmosphere of Bristol, UK" *by* A. C. Rivett et al.

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All in all, I was pleased to see this research carried out and the clever analyses reported in a clear fashion.

The authors dealt with the technical and pedagogic suggestions I made in the "technical" review part of the process. However, the paper would be more complete if it listed or referenced the numerical value of rate constants used in computing daytime OH concentrations. It might also be good to explicitly state the assumption of the model used to derive Eq.1, which I take to be a well-mixed, "box" model. At least that was the way I derived the equation (as a time average from zero to t), but maybe it has a more general derivation. Also, it would be interesting to see the value for Kdispersion computed for alkenes, so that readers might make use of the number in their research.

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Perhaps, this number will be included in a subsequent paper.

There are many interesting results in the paper, including the relative contribution of different types of VOCs to ozone production. In addition, the paper generated a number of interesting questions.

1) The halocarbon spikes made me wonder if they were caused by changing wind directions or changing emissions in time. Apparently, the authors are planning to address this issue in subsequent work.

2) The authors argue that Cl radicals produced by halocarbon degradation may change the oxidizing capacity of the troposphere, but they do not consider the potential impact Cl radicals may have on the validity of the equations they use in estimating OH radicals. Presumably, the contribution is small. But if so, why do we care about halocarbons changing the oxidation capacity in urban atmospheres?

3) I wondered if a dispersion model coupled to an area wide emission model or traffic model would do any better than a box model? This is a important question because a lot of effort has been put into complex models. Maybe, it is not necessary.

4) Can the authors speculate, using their equations, as to how global warming would effect OH and ozone levels in urban areas?

5) Because hydrocarbons have different lifetimes, calculations with them in effect are sampling different effective box sizes. Thus, I wondered if there were any trends in the results that varied with hydrocarbon decay rates.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 769, 2003.