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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-year (2004–2008) record of nonmethane hydrocarbons and halocarbons in New England: seasonal variations and regional sources" *by* R. S. Russo et al.

Anonymous Referee #1

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Russo et al. present several years of hydrocarbon and halocarbon measurements from New Hampshire. The dataset will be a valuable one for testing models and emission inventories, and is one of a very few which includes high quality VOC measurements sustained over such a long period. The subject is appropriate to ACP and is clearly written. I recommend publication once the following points have been addressed:

My biggest complaint is that Section 5.1 (Emission rates of NMHCs) lacks any comprehensive error analysis. For instance, Table 3 presents no uncertainty estimates for the canister-derived emission estimates. On p.1104, L10 it is stated that "a boundary layer height of 125 m was used", but no assessment of uncertainty or variability in

that parameter is given. In the abstract it is stated that "toluene emissions were overestimated (20-35%) in both versions of the NEI". I'd be hard-pressed to believe that the overall uncertainty on the emission rate calculation presented here is better than 20-35%, to allow such a claim. For example, the two methods employed to estimate xylene emissions give very different results of 1.8 versus 0.45!

A related comment on Section 5: the nighttime calculation (wind speed < 1m/s) seems like it will provide very local emission estimates representative of the immediate area, as opposed to the daily canister approach. Please comment on this discrepancy.

p.1105, L29: suggest "do not appear to be varying detectably" in place of "do not appear to be varying significantly"

p.1106, L5 – agreement to within a factor of 4 (compared to White 2008) and 80% (between the two methods) does not necessarily "indicate the robustness of the emission rate estimates" ... please provide additional reasoning or detail to buttress this argument.

Section 3. You should discuss whether there is a regular seasonal change in wind direction that could influence the observed seasonality.

p.1100, L11-14. These statements don't seem to hold water. Presumably both LPG and FF use correlate to some extent with population, and any fine-scale differences between the two have been integrated/mixed during transport to your site. I don't see any basis for the statement "suggests that non-vehicular exhaust emissions, such as residential use of natural gas or LPG, were important sources of ethyne and benzene".

You may want to consider moving parts of the methods sections to Supplemental. In particular, the second half of section 2.1 through section 2.4 is of interest mainly to a small subset of your readership; moving this (along with Figures 2 and 11) to Supplemental would improve flow and readability of your paper.

Section 3.1. Previous VOC surface measurements in this region from other

campaigns would also provide useful context for this section: NARE (e.g., Jobson et al., JGR 103(D11), 13557-13567, 1998) NEAQS-2k2 (e.g., Goldan et al., JGR 109, D21309, doi:10.1029/2003JD004455, 2004; Warneke et al., JGR 109, D10309, doi:10.1029/2003JD004424, 2004) ICARTT (e.g., Millet et al., JGR, 111, D23S53, doi:10.1029/2006JD007287, 2006; Warneke et al., JGR, 112, D10S47, doi:10.1029/2006JD007930, 2007)

Section 3.3. Is there a high-NOx assumption here?

p.1087, L1. 10:00-15:00 – are these integrated samples over this entire time frame, or merely filled at some point during this window? Please clarify.

p.1093, L4. Based on Table 1 and Fig 3d, I would characterize this as a summer minimum, not late spring, for these 3 compounds.

p.1094, L19, suggest "(total range 2.4-5.7 ppbv)"

p.1097, 15-16, also VOC oxidation in the case of CO, cf. Hudman et al., GRL, "Biogenic vs. anthropogenic sources of CO over the United States", 2008.

p.1099, L2, "strong enough to PARTIALLY counteract OH chemistry"

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1083, 2010.