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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of speciated VOCs on regional ozone increment derived from measurements at the UK EMEP supersites between 1999 and 2012" *by* C. S. Malley et al.

Anonymous Referee #2

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The paper presented a method to determine the contribution of individual (anthropogenic) VOC to regional ozone increment from field measurements. It identified ethane and m+p-xylene emission reduction would be most effective in reducing the regional O3 increment, among the 27 measured VOCs. It also made recommendations for building future emission inventories to identify VOC source sectors for ozone mitigation. The conclusion and the method would be interesting for the air quality management and policy community. However, the paper should address the following concerns before being considered for publication.

1. My major concern is that the manuscript failed to consider the impact of biogenic

VOCs especially isoprene, given isoprene concentration was reported at the supersites. It is well known that biogenic VOCs (mostly isoprene) impact the O3 formation by shifting between NOx-limited and radical-limited chemical regimes (e.g., in U.S., Jacob, et al. 1995, Hu et al., 2015; in UK, Vieno et al., 2010; in China, Xie et al., 2008). The manuscript as currently written would mislead readers that biogenic VOCs seem not to be important for ozone formation, but actually the authors are only (or mostly) considering anthropogenic VOCs emission reductions. I'd suggest that authors change some of the language/text and add additional discussion in the paper to emphasize that BVOCs are not discussed. Also, authors may consider adding "anthropogenic" in the title to reflect the main content of the paper.

2. I am curious why some VOCs including isoprene showed negative diurnal photochemical reactivity. How to interpret these negative values? I understand that VOC concentrations (POCP-weighted) were scaled to ethane concentration (POCP scaled) to remove the effect of boundary layer mixing, but I don't see the reason to use the differences of day and night, in addition to weighting by ethane. Can authors reach the same conclusion if only looking at the ratio of POCP-weighted VOC concentrations/ POCP-weighted ethane? Without further discussion and justification of the method, readers would be very confused by the current version of the manuscript.

3. How do the authors actually calculate the model-derived Photochemical Ozone Creation Potential (POCP)? The POCP seems to be a very important concept in terms of describing which VOC is more important for O3 production, however there was only very limited discussion/description on this.

4. A more general comment: there are lots of acronyms as written in the manuscript, which really downgrade the readability of the paper. Also, authors seemed to describe the method to a great detail, but missed to interpret the results and provide thoughtful discussion. The paper would benefit from more discussion and interpretation of their results.

5. I suggest that authors include a table showing the 27 VOCs with their chemical formula, main sources, and recommended OH reaction rates. This would be helpful for guiding the readers especially for those who are not familiar with a menagerie of hydrocarbons.

Specific comments: P7269, lines 20-25: here and later, what are the SOMO35, PODY and EU27? Should readers care about them? The acronyms really limited the smoothness and readability of the paper.

P7283 line 25 and P7284 line 7: here and other places, these two sentences are repeating themselves.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 7267, 2015.