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## ***Interactive comment on “The impacts of precursor reduction and meteorology on ground-level ozone in the Greater Toronto Area” by S. C. Pugliese et al.***

### **Anonymous Referee #1**

Received and published: 29 May 2014

This paper provides a measurements-based investigation of trends in ozone and its precursors in the greater Toronto area and the sensitivity of ozone to emissions. Dramatic reductions are documented for VOC reactivity and NO<sub>2</sub>, and the paper explores why ozone reductions have been far more muted. Though the paper is not particularly ground-breaking, it does apply thoughtful methods that can provide a good example of how similar analyses could be conducted in other regions. I recommend publication after moderate revisions to address the following comments.

Major comments: 1. It would be more appropriate to analyze trends in terms of %/year changes. This is likely to better fit the data and better represent the underlying rela-

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tionship, and would allow for easier comparison across sites and pollutants. 2. Results are presented primarily in terms of summertime averages, which is appropriate. However, two other temporal metrics are of interest as well: (1) fourth-highest daily max 8-h ozone, to better align with the form of the Canadian regulatory standard, and (2) looking beyond summer, since some studies indicate health impacts of ozone extend to low concentrations and sensitivity relationships are likely to be very different in other seasons. In addition, it would be informative to comment on differences between conditions on high and low ozone days within the summer. 3. Despite its usefulness in some other contexts, it's unclear that  $O_x$  is an appropriate metric for characterizing long-term trends in ozone, especially given the ozone regulatory context. Here,  $NO_2$  declined far more steeply than  $O_3$ , so most of the  $O_x$  trend reflects reduction in  $NO_2$ . 4. In Section 3.4, is there evidence to indicate where the transition lies between  $NO_x$ - and VOC-limited ozone formation regimes, in terms of the ratios of OH reactivity to  $NO_2$  and to VOCs that are reported here. 5. The text and Figure 2c have helpful delineation between types of VOCs, but then it becomes unclear which VOCs are being included in Table 2, Figure 7, and some portions of the text. This should be clarified. 6. The authors adopt a thoughtful technique to use VOC ratios to gauge trends in OH, and find evidence for increasing OH. Was this expected? Has modeling been conducted to examine whether it predicted a rise in OH under large-scale  $NO_x$  and VOC reductions as have occurred in Toronto?

Minor comments: 1. p. 10211, line 3: Pointing to a \$9.6 billion impact in Ontario is somewhat misleading in a paper focused on ozone, since most of that impact presumably arose from particulate matter. 2. p. 10222, line 1: change “was” to “were”

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10209, 2014.

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