

Review of ACP 2013-184

Introduction:

The manuscript is a retrospective summary of results reported in a series of papers already published in the BAQS-Met special issue of *Atmospheric Chemistry and Physics* in 2010 and 2011. This is rather unusual in that it is not a review paper in the conventional sense, but has some aspects of a review. Neither is it a summary paper of the kind that introduces papers in a special issue, and is part of that issue. Despite the unusual nature of the submission, I believe the paper should be published because it provides an excellent overview of closely linked results in what is a large number of closely linked works, and will be invaluable to readers interested in developing an overview understanding of the BAQS-Met study. I must add that while the results summarized in this paper are clearly at the leading edge of our understanding of regional scale tropospheric pollution photochemistry, there are no analogous locations worldwide, so the results have mostly regional importance.

General comments:

- 1) The paper is an masterful summary of the target special issue papers, and the authors should be congratulated on the succinct and well-structured paper. In addition, the writing is of a very high standard.
- 2) In my specific comments, I make a few suggestions for changes that will remedy a number of low-level problems in the text.

Specific comments:

- 1) Line 83: The term “model observations” has no useful meaning. Elsewhere the authors use “model output”, which does.
- 2) Line 88: “highly” is awkward. “strongly” would be better.
- 3) Line 155: “Great Lakes region” is misleading, since the study really only covers southwestern Ontario.
- 4) Line 252: “AOD” must be defined. It finally is on line 546.
- 5) Line 524: “Large differences” between what and what?
- 6) Line 530: OM/ Δ CO must be defined.
- 7) Lines 548-549: The time zone must be specified. In the present case, since all chemical and physical processes under consideration are local, it should be local solar time, though local standard time would suffice.
- 8) Line 578: The “downward” in “downward subsidence” is redundant.
- 9) Line 639: See specific comment 7).
- 10) Line 689: It is not clear which the “first flight” is.
- 11) Line 742: Zhang et al 2012 is not in the list of references.
- 12) Line 742: Gordon et al 2012 is not in the list of references.
- 13) Line 835: “Similar to similar” is awkward.
- 14) Figure 5: “AWS” in the vertical axis label should be defined in the caption.