Authors' response to anonymous referee's comments on "Unraveling the complex local-scale flows influencing ozone patterns in the southern Great Lakes of North America" by Levy et al

The authors thank the anonymous referee for his\her compliments on the original manuscript and for his valuable comments and suggestions. The main points raised by the referee have been addressed in the revised manuscript: a paragraph was added to the Introduction stating the objectives of the work and another paragraph discusses the use of the model results has been added to Section 2. Model predictions have now also been identified more clearly in both the text and figure captions. Below are the authors' replies to all of the referee's comments, marked by [AR] and by blue text.
A revised version of the manuscript is attached as supplement, along with a version showing the changes done in track mode. Additional changes were made to the manuscript, mainly regarding minor corrections of style and grammar, but also for better clarity of the text and figures (e.g., Figure 10).

Introduction

The manuscript describes an analysis and interpretation of data from the BAQS Met field study. The data are rich and varied, and provide ample opportunity to develop an understanding of the extremely complex relationships between mesoscale meteorology and ozone air pollution in the far southwestern corner of Ontario, surrounding lakes Erie, Huron and St. Clair, and adjoining portions of the USA. The data are supplemented by output from a grid based, regional scale air quality model. The most important meteorological features represented in the data are interacting lake breeze fronts, land/lake breezes and strong diurnal boundary layer development. The data are exceptionally well presented, well analyzed and interpreted in useful and interesting ways.

General Comments

1) The figures in the manuscript are uniformly excellent. They are well drafted, clear, informative and complete. A minor exception to this is the plotting of wind direction (see specific comment 16)

[AR] We thank the referee for these compliments. Following a comment by another referee, some of the figures were improved by enlarging the text size of some labels. As for the wind direction, after further deliberation we believe that the current way it is presented in Figures 6 and 12 remains the best compromise. A more detailed reply is given for specific comment 16.

2) The work would benefit greatly from a clearly stated set of objectives.

[AR] This is a good suggestion. The following paragraph was added to the Introduction section to provide a clear statement of objectives:

"The goal of this paper is to elucidate the complexity of various processes influencing summertime ozone levels in the southern Great Lakes region of North America. More specifically, it examines the spatio-temporal variability in the levels of surface ozone and related air pollutants, with respect to precursors

emission sources, regional- (synoptic) and local- (land-lake breeze) scale meteorology, differing processes over land and water, and the vertical structure of ozone."

3) The authors are far to cavalier in their unquestioning acceptance of the correctness of the AURAMS model output.
[AR] The reviewer is correct that the model results should not be stated as facts and the reader should be reminded that these results are only predictions from a numerical model. This is now discussed in a new paragraph added to Section 2 (given below) and emphasized in Section 3.2 (see response to specific comment 10).
Nevertheless, as the GEM/AURAMS model outputs are presented in many of the BAQS-Met papers in this special issue, it is not appropriate to evaluate the model performance for the BAQS-Met period in each of these papers. A thorough evaluation of the model performance in predicting ozone is presented by Makar et al. (2010a,b) in this special issue. In the current paper we specifically aimed to examine the model output with respect to the general features related to ozone formation and destruction in four dimensions (i.e., time and space) over the lakes and land. For this reason, only the 23-day averaged fields of the models outputs are examined in sections 3.1 and 3.2. For the case study in section 3.3 the model is not used because of the ample information available from the measurements, with the exception of section 3.3.4.
A reference to Makar et al. (2010a) was added to the manuscript.
Below is the paragraph added to Section 2 of the manuscript:
"Although the model has limitations, it also has unique value in providing a complete, physically-consistent, four-dimensional description of the atmosphere during BAQS-Met that is not possible with measurements. Recognizing the model is imperfect, an attempt is made in this paper to use the model predictions in conjunction with available measurements so as to provide broader insight in a manner that accentuates the model strengths (e.g., long term average fields as opposed to specific measurement periods) and provides complementary information that helps to interpret the measurements. Our focus here has not been to evaluate the model (cf. Makar et al., 2010a,b) but to use it to help understand the conditions in the region, in light of available measurements. Nonetheless, the reader needs to be aware that what is displayed in several cases is model output and we have tried to make this distinction clear in the text."

Specific Comments
1) Throughout the paper, EDT is used. The phenomena under consideration are almost solely driven by solar heating, which argues that local solar time should be used. Since EST is easier to access, and very close to local solar time, this would be a more appropriate time frame.
[AR] We agree that EST would be a natural choice to present the data related to ozone photochemical formation and destruction. Nevertheless, there are arguments for and against using a range of different time zones. The main reason that we selected EDT in this paper is that human activity in the region, which is responsible for many of the main emission sources, is directly related to EDT. More specifically, the morning and

afternoon weekday commutes are a major consideration for air pollution that shows up in the paper (e.g., Figures 2, 3 and 4), and is more easily understood when presented using the same time frame as that used by emitters, i.e., EDT. Furthermore, during the BAQS-Met campaign, the convention for the chemical measurements performed by all participants was for them to be in a uniform time zone, so as to avoid confusion, and EDT was selected for this purpose.
To help the reader, the following sentence was added at the end of Section 2:
"Throughout the paper, Eastern Daylight Time (EDT) is used to present the data, which is one hour ahead of Eastern Standard Time (EST) and four hours behind Coordinated Universal Time (UTC)."

2) Line 45: Higher than what?
[AR] Changed to "high"

3) Line 74: Surely you mean spatial, rather than temporal?
[AR] The sentence refers to the change in the location of the ozone maximum within a one-hour interval between measurements, emphasizing the temporal change. However, the reviewer is correct that the change is also in the location of the ozone maximum. The sentence was rephrased to:
"…suggesting that both spatial and temporal changes, perhaps related to advection patterns, play an important part."

4) Lines 90-94: This really is not an adequate statement of study objectives.
[AR] See answer to General comment 2 above.

5) Section 2: Surely there is a BAQS-MET overview paper that describes all available data from the study?
[AR] Not for now. There is an overview paper in preparation for the special issue that will describe the data and integrate the main scientific findings (Brook et al., 201x), but its title and author list are not yet known so it is difficult to give reference to it at this time.

6) A number of references in the paper (Halla et al, 2010; Hayden et al, 2010; Sills et al, 2010; Stroud et al 2010) are to works that are either under review, or in preparation. I assume those will all be part of the BAQS-MET special issue. If those papers are not accepted for publication, all references to them must be removed.
[AR] Agreed.

7) Lines 152 & 153: This seems to be a statement of objectives. It must be presented much earlier in the work.
[AR] This sentence refers to Section 3.1 only. See answer to General comment 2 above.

8) Line 209: This seems to be the wrong figure reference. 3c seems more appropriate.

[AR] The reference should be to Figure 1. The sentence was rephrased to connect it better to this reference:
"These differences might be due, at least in part, to the six urban sites being located at or close to the city center, which are thus more influenced by traffic NO_x emissions compared to the part of the model domain that is assigned to the "urban" group (cf. Fig. 1)."

9) Lines 255 to 260: The authors must make some attempt to evaluate the veracity of the model runs before discussing them. Figure 2 can form the beginning of this evaluation.
[AR] See our answer to General comment 3 above.

10) Lines 280 to 320: The authors write about model output as if it is reality. Much more subtle wording is needed.
[AR] The section was modified and adjectives like "modeled" and "predicted" were inserted to emphasize the fact that model predictions are being discussed.

11) Line 517 - 519: The authors must explain how "these suggest a local to regional transport".
[AR] The sentence was rephrased to make this point clearer:
"These suggest more freshly emitted pollutants that, with the southwesterly flow, point to local to regional transport of polluted air crossing over Lake Erie, potentially related to a large power plant near the western shore of the lake."

12) Line 541: The authors describe a "great complexity", but make no attempt to show that the AURAMS model is able to capture this complexity.
[AR] The vertical structure of the ozone over the study region described in section 3.3.3 is discussed based on measurements taken by the aircraft and the Ridgetown tethersonde only, without relying on the AURAMS model. Regarding the model performance in general, see our answer to General comment 3 above.

13) Figure 3 caption: The caption must state that these are model results. The use of the work "predicted" is inappropriate here.
[AR] The caption was changed to:
"Figure 3: Mean AURAMS model output showing time-height cross-sections for the sub-domain shown in Fig. 2,…"

14) Figure 4 caption: The caption must state that these are model results.
[AR] The caption was changed to:
"Figure 4: Mean AURAMS model output showing (a) mean ground-level ozone, (b) mean ozone cross sections (ppbv), and (c) mean vertical velocity (m s^{-1}) for all 24 hours (a1, b1 & c1) and selected times (00:00, 06:00, 14:00 and 21:00 EDT, a2-5, b2-5 and c2-5, respectively), calculated for the…"

15) Figure 12 caption: I believe red and black have been exchanged here.
[AR] Thank you. The caption has been corrected.

16) Figures 6 and 12: Linear plots of wind direction are always a problem. The vertical axes should be chosen so that plotted points do not “switch” across the plot as in Figure 6, 0800-1300, 06/07/2007. There exist a number of solutions to this problem.

[AR] We examined several alternatives to present the wind direction in these figures in another way, such as presenting the direction as different colors, showing wind vectors (“feather” plot), and changing the vertical scale limits to better fit the measurements. However, each of these methods had its own disadvantages. Given the complexity and wealth of information presented in Figures 6 and 12, we believe that the current presentation provides a compact description and remains the best compromise. Note that there are only two time periods shown in an “alternating” direction: 0800-1300 in Figure 6 and 2130-0100 in Figure 12d.

References:

Makar, P. A., Gong, W., Mooney, C., Zhang, J., Davignon, D., Samaali, M., Moran, M. D., He, H., Tarasick, D. W., Sills, D., and Chen, J.: Dynamic adjustment of climatological ozone boundary conditions for air-quality forecasts, Atmos. Chem. Phys., 10, 8997-9015, doi:10.5194/acp-10-8997-2010, 2010a.

Makar, P., Zhang, J., Gong, W. M., Stroud, C., Sills, D. M. L., Hayden, K., Brook, J. R., Levy, I., Mihele, C., Moran, M. and Tarasick, D.: Mass tracking for chemical analysis: the causes of ozone formation in southern Ontario during BAQS-Met 2007, Atmos. Chem. Phys. (under review), 2010b.