Atmos. Chem. Phys. Discuss., 14, C8416–C8422, 2014
www.atmos-chem-phys-discuss.net/14/C8416/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone distributions over southern Lake Michigan: comparisons between ferry-based observations, shoreline-based DOAS observations and air quality forecast models" *by* P. A. Cleary et al.

Anonymous Referee #1

Received and published: 22 October 2014

Cleary et al. provide an analysis of measured ozone concentrations over Lake Michigan and a comparison against model predictions. They show that the observed ozone is higher over the lake than onshore and that this has important implications of the fate and transport for ozone and other air pollution due to the meteorological dynamics unique to lake effect wind patterns. In addition, they show that the model over-predicts this onshore/offshore ozone gradient potentially leading to false positives when alerting the public about air quality exceedance days. The authors provide some suggestions on the sources of model bias but do not provide an in depth evaluation. The topic is of

scientific importance and has not received a lot of attention in the research community. Despite the fact that models have long predicted strong gradients of ozone over Lake Michigan, few efforts have been undertaken to verify this feature and its potential to impact air quality at shoreline locations. The authors provide some insight but could probably do more to probe both the ambient and the modeled data. I think the article is an appropriate topic and will be of interest to ACP readers. I recommend publication after comments below are addressed.

General comments:

1. The authors discuss many "trends" and "patterns" in their data but provide no statistical analysis despite the large sample size. For instance in figure 1, they have at least 75 observations going into each of 12 wind direction bins yet the authors never bother to determine whether the differences between bins are statistically significant. In addition, patterns in box plots shown in Figures 5, 6, 7, 10, 11, and 12 are discussed but the authors do not quantify the differences. Figures 5 and 6 show the difference in measured ozone between ferry measurements and shoreline DOAS which is used as a proxy for the onshore/offshore gradient. Although median values are sometimes above or below zero, the interquartile range crosses zero in almost all cases suggesting that the difference in ozone may not be statistically different from zero. Similarly In figures 11 and 12 the authors break down model bias spatially and temporally. There is not a single boxplot with a mean or median value that is outside the interquartile range of any other boxplot. Again, this suggests that the spatial and temporal differences are not likely to be statistically significant. Adding some simple statistical analysis would not be a large effort. The authors should do this to add rigor to the analysis. I do believe that the spatial/temporal patterns identified by the authors are of interest even if they are not statistically significant but authors should at least acknowledge which results are significant in this discussion.

2. The authors do not discuss uncertainties in measurements or limitations/uncertainties in the comparisons between instruments or between the observa-

tions and the model predictions. The authors should add a brief discussion of all major uncertainties and limitations.

3. The case study presented in Figure 4 should be better integrated into the overall analysis. The authors choose 6 days out of 6 months to analyze in detail. Why was this particular episode chosen? Does this episode demonstrate general trends that are seen throughout the entire measurement period? Is this episode interesting because it is unique within the period or because it represents typical patterns? Can any broader conclusions be drawn about pollutant fate and transport in this area from these 6 days? Some connection should be made between these six days and the study period as a whole.

4. A major part of this article is the comparison between modeled and measured ozone over Lake Michigan yet the authors do not describe the model or even give a reference for it. Some basic information about how the model is run is essential to understand what any biases mean.

Specific comments:

Page 23209, line 7: Was binning done for 30 minute averaged values? This is implied in the figure caption but never stated explicitly. Please state in the text. Page 23209, line 9: It might be of interest to also look at variability and max values by wind direction. As stated above, a statistical analysis could be performed to determine whether concentrations associated with different wind directions are statistically different.

Page 23210, line 11: Can any quantitative comparisons be made with older studies to show how pollution levels/processes have changed in the 10 years since the previous studies?

Page 23210, lines 26-28: Were any instrument inter-comparisons made? For instance, were the DOAS and the ferry O3 instrument ever co-located to determine instrument bias/variability between the two? Since this comparison is used to imply differences

in onshore and offshore concentrations, it is important to know how the instruments perform when measuring at the same location. The authors should at least discuss this as a limitation if no instrument inter-comparison was ever done.

Page 23210, lines 26-28: Are there any ground-based ozone observations from the ozone monitoring network in Kenosha and Milwaukee that could be compared to give a sense for how much shoreline ozone concentrations typically vary between these locations. This is also an important piece of information for interpreting differences between the DOAS and the ferry. Are the onshore/offshore differences greater that typical differences in ozone concentrations along the Wisconsin shoreline? If not, then these differences may be representing spatial variability of ozone but not necessarily typical lake versus onshore differences.

Page 23210, line 18: When the authors say “daily maximum” are they referring to 1-hr daily maximum, 30 minute daily maximum, or 8-hr daily maximum values?

Page 23211: In figure 4, it appears that there is a pattern in the second half of the episode where the peak ozone occurs later in the day for the ferry measurements than for the DOAS. Is this pattern seen at other times in the study period? Does this temporal difference in the timing of peak ozone tell us anything about physical/chemical processes leading to high ozone? Is it a result of offshore flow during the day?

Page 23212, line 1: add “shoreline” before “NO2”

Page 23212, lines 14-15: Are wind direction and temperature correlated? Again, a quick statistical test could be used to answer this question quantitatively.

Page 23212, line 16: Should 18:00-02:00 actually be 18:00-22:00? Page 23208 line 8 states that the last Ferry trip of the day occurs at 22:00.

Page 23214: The authors should consider also looking to see if there was a seasonal dependence in the DOAS/ferry differences. They could recreate figure 5-7 except split them by season (spring, summer, fall) rather than time of day to investigate.

Page 23215: The authors need to provide a reference for the National Air Quality Forecast model and also provide a few key details about this model. The authors should discuss some of the largest uncertainties in providing this type of comparison but before they do so they need to provide some information about how the model was run.

* Is this the forecast that is provided by NOAA? What photochemical model is it based on? CMAQ? Please provide a reference.

* What is the model resolution? 12km? If the resolution was much coarser than 12km (36km or larger) then comparisons between coarse model grids and point measurements might not be appropriate.

* What emissions inventory year was used to project the emissions? I think it is likely that the 2009 model runs were performed using a 2005 base emissions inventory year. When the emissions were projected from 2005 to 2009 was any adjustment made for the economic recession or for new national and state emissions control programs that had been implemented in this time period? If not, it is likely that the emissions for the 2009 forecast were substantially higher than reality and this may explain some of the model bias.

* What meteorological model was used to drive the forecast model? How often was the met model re-initialized?

Page 23215, lines 12-13: I may be misunderstanding, but it sounds like the authors took image files and attempted to translate georeferenced ozone concentrations from the color scale shown in the image. This seems like an odd way to obtain the model data as it potentially adds an unnecessary source of error and a lot of work! Why didn't the authors simply ask NOAA for the raw model output so that they could use the actual model data? How precise is the translation of colors to ppb? For instance the authors would be limited by the resolution of the color scale used in the images. What resolution was used for the color scale? 1ppb? 5ppb? The raw data would give exact values down

to several decimal places. How accurate was the image program at distinguishing between adjacent colors on the color scale for translating colors to concentrations? Would this be impacted by how the computer monitor interprets colors? In addition, the gridded model output comes with information about the geographic projection used to create the model domain. It seems like goereferencing the images would add error if the authors did not use information about how the model grid was projected. Please further clarify if this is indeed what was done to obtain model data and add a discussion about the added uncertainty/error introduced by this process.

Page 23215, lines 22-23: Were model grid cells averaged when the ferry traversed multiple cells?

Page 23216, lines 5-7: Was the model data for the bias calculation in daily maximum ozone matched in time and space with the observations? I.E. was the model data the maximum daily ozone in the model or was is the model ozone at the time and location of the observed daily maximum ozone. If it was the latter, the authors should consider looking at the former as well to see if perhaps the model had a similar maximum value that was displaced in time or space.

Page 23216, lines 16-17: Although the authors report no ozone exceedances in 2008-2010 of the federal standard in their data, this is not consistent with ozone values reported by Wisconsin's ground-based monitoring network. For instance, the Kenosha monitor (#55-059-0019) reported one day in late July 2008, two days in late June 2009 and 7 days throughout the summer of 2010 that had 8-hr daily maximum ozone values above 75 ppb. There are four monitors located in Milwaukee County (55-079-0010, 55-079-0026, 55-079-0041 and 55-079-0085). Each of these shows one or more exceedance in the 2008-2010 time period. So the authors should qualify their statement that the model is predicted too many exceedances since independent nearby measurements do show multiple exceedances during the study period. Data from the ground-based monitoring networks is available online at: www.epa.gov/airdata

Page 23216: The authors should consider looking at bias split out by observed concentration bin. Past studies (for instance Simon et al. referenced in this paper) have found that model performance on high ozone days differers in magnitude and directions from model performance on lower ozone days.

Page 23217, line 21: October trends don't look substantially more distinct than other months

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23201, 2014.