

Interactive
Comment

Interactive comment on “Impacts of urban land-surface forcing on air quality in the Seoul metropolitan area” by Y.-H. Ryu et al.

Anonymous Referee #2

Received and published: 13 November 2012

This paper uses an offline meteorological and air quality modeling system to investigate how the urban heat island effect alters the chemical evolution of ozone. This study builds upon similar findings from previous studies; however, the complex variations of topography and coastal environment could also alter the impact of the urban head island on the evolution of ozone and its precursors. There have been few such studies, so content of this paper should be useful to the air quality modeling community. There are still a number of issues, described next, that need to be addressed by the paper.

Major Comments:

1) While the results of the paper are interesting and show that adequately including a treatment of the urban boundary layer is important for air quality purposes, the air quality simulations depend upon the meteorological simulations that are presented in

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Interactive Discussion

Discussion Paper



another paper currently under review (Ryu and Baik, 2012). It is not known how well the model represents key factors, such as the magnitude of the UHI, differences in boundary layer height between the city and outside of the city, and winds in the city core that will affect the conclusions drawn in this study. Either this paper needs to be put on hold until the first paper is accepted and in press (and available for reviewers to access) or the authors need to include key results from that study that demonstrate the model performs reasonably well in representing observed temperature and winds in the urban area. Readers need to know if there are sufficient observations to verify the meteorological model predictions, otherwise, the present study is simply more of a modeling exercise that may or may not represent the true impact of the UHI on air quality predictions.

2) Some additional discussion is needed at the end of the paper to stress that the authors have examined one case and put this study into the proper context. They do mention that this case is for fair weather conditions when one might expect a larger impact of the UHI on the local meteorology. However, it would also be interesting to know how strong synoptic forcing needs to be that would overwhelm the UHI and the effects on air quality presented in this study.

3) The authors clearly show that representing the urban impacts on meteorology can affect ozone chemistry in a noticeable way. However, it is not clear how this effect compares to other uncertainties in air quality predictions. For example, choice of PBL parameterization could also introduce differences in ozone concentrations and uncertainties in emission inventories will impact predictions of ozone. How does the impact of the UHI compare to other commonly known uncertainties in air quality predictions? Some context is needed. It would be interesting to see Figure 2b repeated but using the simulation replacing the urban area with cropland. Presumably the results would be worse if the modeling system was adequately representing the overall ozone chemistry, transport, and mixing in the region. It is possible that the statistics would not look much different which would stress the difficulty in identifying the impact of the UHI

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using standard metrics by themselves.

4) The title refers to “air quality”, but really only deals with ozone chemistry. Other factors of interest to air quality, such as particulate matter, are not investigated. So I recommend changing the title from “air quality” to “ozone and its precursors” to better represent the contents of the paper.

5) While the paper is well organized, there are numerous grammatical errors, some of which are pointed out in the specific comments. I likely did not catch all of them and suggest that the authors find an editor to help them with the final manuscript.

Specific Comments:

Page 25794, lines 23-24: That may be true for some studies, but the impact of the UHI on simulated near-surface winds has been hard to verify in observations.

Page 25795, line 19: “Advanced Research” is not needed.

Page 25795, line 26: Refer to Figure 1a. I assume the WRF grids area the same as the CMAQ grids?

Page 25796, lines 4-5: Change sentence to “The National Center for Environmental Prediction (NCEP) final analyses are used for the initial and boundary conditions.”

Page 25796, line 12, Change “model” to “represent”.

Page 25796, lines15-16: Perhaps just say the domains are the same as those used for WRF.

Page 25796, lines 17-18: What are the default profiles for CMAQ? Perhaps include this information in a table for the most relevant species. Is long-range transport significant for this case and how might that affect the model results?

Page 25797, lines 1-9: It would be useful to include a table summarizing the total emissions of NO_x, VOCs, and other relevant species for the innermost grid for 2007

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Interactive Discussion

Discussion Paper



and 2008.

Page 25797, line 15: Change the phrase to “is the rate at 1500 LT on a weekday (Thursday).”

Page 25799, line 7: Change “observed ones” to “observations”.

Page 25800, line 21-22: I disagree with the terminology of UHI here, and subsequently in the text. UHI usually refers to a horizontal temperature gradient between the urban area and the surroundings. The author should use another term or simply call this a temperature difference between the simulations; otherwise, use of UHI introduces confusion in the description of the model results.

Page 25800, line 23: Change “appears in the nighttime” to “was simulated at night”.

Page 25801, lines 5 - 17: What is missing in this description is whether building wake effects slow the winds down. Since the urban canopy model is not described it is hard to tell whether those effects are included.

Page 25802, line 2: Change “influences significantly” to “significantly influences”.

Page 25802, line 2-3: Change “hence urban-modified” to “hence the urban-modified”.

Page 25802, line 7: Change “concentration is” to “concentrations are”.

Page 25802, line 14: Change “less” to “reduced”.

Page 25802, lines 26-28: This sentence needed to be reworded. I think the authors mean to say that vertical mixing dominates in both simulations, so that the reduced destruction of O₃ by NO in the URBAN simulation is not that significant.

Page 25804, line 2: What is “the cloud process”. Be more specific.

Page 25804, line 10-11: This is a poorly worded sentence. Please fix.

Page 25804, line 22: change “the more O₃ production in the surface layer, and the more” to “the enhanced O₃ production in the surface layer, and the enhanced”.

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Interactive Discussion

Discussion Paper



Interactive
Comment

Page 25804, lines 23-25: Change sentence to “In addition to the urban-modified boundary layer, the urban-induced local circulation contributes . . .”

Page 25804, lines 24: I don’t know what the term urban-induced/-modified means. Induced and modified seem redundant.

Page 25804, lines 28-29: Change to “In the NO-URBAN simulation, the reduction in O₃ by dry deposition and chemical processes in the surface layer is compensated by the downward vertical mixing of O₃ from the upper layers. The near-surface removal of O₃ by dry deposition is less in the URBAN simulation than the NO-URBAN simulation.”

Page 25812, line 7: Change “Under the fair weather condition” to “Under fair weather conditions”.

Page 25813, lines 13-15 This sentence seems to be redundant with the phrase in the previous sentence on fair weather conditions..

References: I noticed at least one reference cited in the text that was not included in the reference list. The authors should check all references to make sure they are properly cited.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25791, 2012.

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