

Interactive comment on “Evaluation of urban surface parameterizations in the WRF model using measurements during the Texas Air Quality Study 2006 field campaign” by S.-H. Lee et al.

Anonymous Referee #2

Received and published: 23 November 2010

This paper describes the performance of an urban canopy parameterization using measurements collected during a recent air quality field campaign in Houston. Overall, the paper is well written and the results are clearly presented. However, there are several points that need to be clarified to improve the manuscript.

Major Comments:

1) The abstract could be improved by including some specific numbers on the performance of the urban surface parameterizations. This material would add to the length, but some details (e.g. specifics on the measurement platforms) that are not needed could also be removed. It seems that one of the advantages of testing an urban canopy

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parameterization for a field campaign period is availability of a wide range of data that is normally not available. Yet, the abstract is worded to basically just list the instruments used even though the most important aspect is having a wide range of measurements aloft that characterize the boundary layer and not just the surface quantities.

2) Figure 3 shows a commercial/industrial region, but Figure 2 has no grid cells with this type of urban classification (denoted in black). Figure 3 shows that Houston is either low or high density residential. Yet subsequent figures show results for the commercial/industrial regions. Either the plot is mislabeled or I am missing something. The test starting on page 24052 then goes on to show results of the model for the commercial/industrial regions. Please clarify.

3) It would also seem that the performance of the model depends on both the urban canopy and boundary layer parameterizations since they are coupled. It would have been useful to see how UCM performs with another PBL parameterization. At least the authors should comment somewhere in the text regarding what they expect the performance would be had the urban canopy parameterization was used with a different boundary layer parameterization. Another area of discussion are the parameters used for both the LSM and UCM. It was shown that the LSM could be made somewhat better by adjusting parameters. Presumably the UCM could be similarly “tuned”; however, adjusting parameters needs to be based on some type of data rather than just trying to fit the prediction of atmospheric quantities.

4) Some additional discussion is needed on measurements for two key analyses made in this paper. First, surface flux measurements were not collected in Houston proper. I understand that modelers use what is available, but they need to discuss that data in Houston would have provided the most useful data to evaluate the parameterization. Second, discussion is needed regarding the lack of boundary layer depth and other boundary layer properties at night, which are critical to evaluate the performance of the model at this time. Or am I missing some information regarding the measurement strategy during the field campaign?

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5) The last paragraph in the summary section seems out of place. Since the paper is focused on urban canopy parameterizations, this material should be moved earlier, so that the paper ends on conclusions regarding the primary focus of research. Also, the implications of their findings on the urban canopy parameterization should be discussed. For example, how will these results affect air quality predictions? Or are the changes (that are modest for some parameters) going to be significant in terms of other applications?

Secondary Comments:

Title: "in the WRF model" could be removed from the title since it is merely the host model for the urban surface parameterizations that are the focus of this study.

Page 25034, line 24: Change "Model showed" to "The model showed".

Page 24036, line 25-20: This sentence is a bit misleading. Some of the previous studies were performed before more sophisticated urban parameterizations were available or fully tested in mesoscale models. The sentence makes it sound like a conscience decision not to use a more advanced urban canopy parameterization.

Section 2.1, first paragraph: Most of this text seems unnecessary. The authors should just include a sentence with a reference for the WRF model that contains these details and the version number, and combine that sentence with the next paragraph. Readers unfamiliar with WRF will not know what the "Advanced Research version" is compared to the other version that is available.

Page 25038, line 23: 35 levels is rather coarse. Please include some information on how many nodes are located within the lowest few kilometers of the atmosphere. Since strong vertical gradients in temperature, humidity and wind can occur this region, particularly associated with the sea/bay breeze, it is important to know whether the current model configuration is sufficient to resolve the phenomena the model is intended to simulate.

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Section 2.2: It would be useful to provide sub-section headings for the LSM and UCM descriptions to better differentiate the two.

Page 25040, line 23: Are there other studies that evaluate the UCM parameterization in WRF? It would be useful to cite those if possible. Also, has the UCM parameterization in WRF been applied to the Houston area?

Page 24053, lines 21-22: What were the vegetation fractions based on?

Section 2.3: Much of the discussion on the details of the YSU scheme seem unnecessary in this paper since the focus is on urban canopy parameterizations. There is only one other short section that discusses how this scheme affects the simulation results, in terms of the nocturnal boundary layer. It would seem that only the introductory material and the last sentence is important.

Page 25044, line 20: Why only use 10 stations and why these particular ones?

Page 25044, line 24: Change "in the northwestern area and northeastern area from Houston" to "northwest and northeast of Houston". Please comment that it would have been useful to have surface flux sites in Houston where the importance of the urban canopy parameterization is the greatest.

Page 24045, line 6: Sentence "Figure 2 shows ..." seems out of place. Figure 2 shows more than just the location of the surface site. Perhaps moving this phrase earlier in the paragraph.

Page 25045, second paragraph: Are there are any papers that can be cited regarding the aircraft flights and instrumentation?

Page 25046, line 4: As stated in my major comment, I don't see which model cells are classified as commercial/industrial region.

Page 25046, line 10: Change "formation" to "characteristics" or "structure".

Page 25046, lines 21-22: This sentence correctly shows that the LSM does better

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than the UCM for this classification, yet it seems to be downplayed in the manuscript. Overall UCM does better than LSM overall, but still requires some improvement.

Page 25048, line 7: I cannot really tell which stations in Figure 2 are the wind stations.

Page 25049, line 3: This sentence is awkward. Change “northeastern forest area from Houston” to “forest northeast of Houston”.

Page 25049, line 14: Why show these days as opposed to other days? Please clarify in the text.

Page 25050, line 1-3: What is described is difficult to see in the plot. Also, it would be useful to reiterate that the stronger land-sea thermal contrast is related to changes in temperature over Houston (Table 4, etc.).

Page 25050, line 14: The surface fluxes in Fig. 8 are not over the Houston metropolitan area as stated in text. Based on the figure I would say the site is in a small city outside of the metropolitan area.

Page 25050, line 17: Change “for lower” to “in the lower”.

Page 25050, line 24: The predicted boundary layer depths at night are lower than 300 m for some nights, so the explanation provided may not entirely explain the problem with the nocturnal low level jet. The relevance of the whole discussion regarding the wind evaluation is not clearly articulated. I would expect that the urban boundary layer would have a larger impact on surface temperatures and boundary layer temperature profiles (and consequently PBL depth), than on winds. The results seem to confirm this, and one would not expect the urban canopy parameterization to change the predictions of the low-level jet.

Page 25051, section 4.4: Please describe the uncertainties associated with the estimates of PBL depth derived from the aircraft. Has this technique been described in a paper that can be cited?

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Page 25053, line 4: Would be useful to add what the wind direction is (presumably westerly) for the warm advection. One has to piece this information together from multiple plots to visualize what is going on. Are the winds over-predicted for the warm advection?

Page 25053: I do not think how water temperatures were prescribed in the model. Databases may have good estimates for ocean temperatures, but the estimates over bay may be off. Some additional discussion on the bay temperatures is needed in the model configuration section.

Page 25055, line 14: Change “great potential to accurately” to “great potential to more accurately”. The application in this study certainly shows an improvement, but there is room for more improvement.

Figure 2: I do not see “dots” in this figure. There are 2 solid circles for the surface flux sites, but are they different than dots?

Figure 8: Explicitly state that the site is a low density residential area.

Figure 9: Explicitly state that the site is in a forest region.

Figure 10: It is hard to tell the differences in the plot. I suggest changing it to a color plot, with color denoting speed and using the same vector length for the wind direction.

Figure 15: Include specific time for (a) and (b).

Figure 16: Please label Houston Ship Channel and Barbours Cut. Most readers will not know where these locations are.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25033, 2010.

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