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Atmospheric Chemistry and Physics Discussions

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 #3

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Evaluation of urban surface parameterizations in the WRF model using measurements during the Texas Air Quality Study 2006 field campaign S.-H. Lee, S.-W. Kim, W. M. Angevine, L. Bianco, S. A. McKeen, C. J. Senff, M. Trainer, S. C. Tucker, and R. J. Zamora Page(s) 25033-25080

The study presents a very extensive evaluation of two of the current urban parameterizations implemented in WRF, while introducing a third alternative based on several parameter changes. Model runs for a 6 day period corresponding to the TexAQS 2 measurement campaign are performed to enable comparison with observations of 2 m temperatures, 10 m wind speed and direction, surface energy balance fluxes, wind

profiles and atmospheric boundary layer height. Results underline some improvement when using the more sophisticated Urban Canopy Model (UCM) scheme. The major reason identified is the ability of the UCM to represent

(1) the impact of sub-grid scale vegetation on energy partitioning and (2) heterogeneities between urban grid cells. The study is very well presented and results are of great interest for the urban-WRF user community. Several clarifications are however needed to facilitate the understanding of the model settings (and their reproducibility). Some adjustments on the "surface energy balance fluxes" section (4.2) are also suggested. I would therefore suggest acceptance of the manuscript after some "minor revisions".

General comments:

* Given that the version of WRF used in the study is very recent (WRF 3.1, March 2009) the results presented here are of great interest for the WRF/urban user community. The urban modelling options were revisited in this 3.1 release (Chen et al., 2010) and this paper is probably one of the first evaluating the updated system with such an extensive set of observations. It is however not always very clear which schemes/settings are used for the simulations. I therefore suggest more extensive references to the paper by Chen et al. (2010) which provides an overview of WRF 3.1 and its urban modelling options.

1) Some references are needed with regards to the "original" urban LSM: is it the "bulk urban parameterization" as described in Chen et al. (2010)? In that case a reference to the work of Liu et al. (2006) is probably needed.

2) A similar comment holds for the UCM; the author only reference Kusaka et al. (2001) but the version of the UCM implemented in WRF 3.1 has been modified (see Loridan et al., 2010). The modifications mainly focussed on a simplification of input parameter list which led to the one presented in Table 3. Most of these modifications are actually listed at the end of section 2.2 (i.e. roughness length and displacement height

calculated via morphometric method, internal calculation of aspect ratios and sky view factors) - a reference to the Loridan et al. (2010) paper would be appropriate.

3) The UCM from WRF 3.1 has the ability to include an anthropogenic heat contribution to the sensible heat flux. Has this option been activated in the runs presented? In any case can the author comment on their choice (i.e. if included: what profile/magnitude? and if not: is it neglected? or left out for better comparison with the LSM?).

4) In several places the authors state that "the urban parameterizations implemented in WRF" are evaluated. I believe it is important to specify that only two of the three options implemented in WRF are evaluated: the more sophisticated Multi –layer urban canopy model from Martilli et al. (2002) is also available as an option in WRF v3.1 (see Chen et al., 2010) but not used in the study.

* The comparison of modelled and observed surface energy balance fluxes is very interesting and useful to understand the differences in performance from the 3 schemes. However, more details are needed with regard to the measurement system (unless a reference to another study where it is fully described is provided): for instance information on the height at which the fluxes are measured would be of interest to compare to the height of the lowest WRF level (20 m) at which they are simulated. Some comments on the measurement footprint, and comparison of its scale to the 4 km x 4 km horizontal resolution would also improve the evaluation. More generally, is there a reference for the TexAQS 2 campaign?

* The authors compare modelled and observed incoming radiative fluxes but I believe the outgoing components and/or the net all wave radiation would teach more about possible model deficiencies. Were these fluxes observed? Could they be included in the analysis? In particular the ability of the UCM to represent radiation trapping within the canyon should be better reflected in the outgoing components.

* An interesting feature from Fig. 8 and 9 is that the UCM is the only scheme catching the dip in incoming shortwave radiation on the 12th of August when more cloud cover

was present. Could the author comment briefly on that aspect? I suppose it is directly linked to the fact that the UCM generates higher turbulent heat fluxes but any further comment would be of interest.

* The English in a number of places needs to be improved

* There are a lot of places where references are needed

* Acronyms should be given in full when they first appear.

Specific comments:

p25036, l11: "the urban parameterizations implemented in WRF" → only 2 out of 3, see comment above.

P25038, l7: "The LSM" → "Noah LSM" is the complete name of the scheme.

P25038, l23: The authors refer to the "vegetation fraction" but I think what is meant here is "green vegetation fraction" (or shading factor as it is called in WRF: parameter SHDFAC). This distinction is important in order to avoid confusion with the "vegetation fraction" mentioned in p P25042, l21 which refers to (1-fU) in eq. 4. One is a plan area of vegetation cover (i.e. 1-fU) and the other is defined as the part of the vegetation that is photosynthetically active (green vegetation fraction). Same comment applies to Table 2. I think the distinction is important because as it stands it appears that the LSM is actually ran using a tile approach (i.e. as for eq. 4).

P25038, l23: the original value for this green vegetation fraction is said to be 10% in the text but is 5% in Table 2. From WRF tables it seems to be 10%

P25039: reference to Liu et al. (2006) needed somewhere.

P25040, l11: → "green" vegetation fraction

P25042, l11-l18 → this section is a description of the Loridan et al. (2010) work; a reference to that study is needed.

P25043, l17: “determined a method” → “determined from a method”

P25044: Observations section. Is this the first time the TexAQS 2 campaign is presented? If not I think a reference to where more detailed can be found is needed.

P25049, l4,5: this is an interesting point, could the authors provide more details? Is warmer air being advected from the urban cells? Is there a dependency on wind direction?

P25048: as mentioned in the general comments, analysis of upward components of the radiative fluxes and/or net all-wave radiation would help to evaluate the ability of the schemes to simulate the radiation balance (see for instance Grimmond et al. 2010).

P25052 and Fig 15: simulated → simulated from UCM only should be clarified

p25054, l3: “this may be due that ...” → “due to the fact that”

References

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