

Interactive comment on “Large eddy simulation of urban features for Copenhagen metropolitan area” by A. Mahura et al.

Anonymous Referee #1

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General: The paper presents ‘LES-type’ simulations for an urban area (Copenhagen, Denmark) investigating the differences in certain near-surface parameters for different types of surfaces in the area. These surface types are different in their vegetation cover, type of building structure, etc. The results are indeed, as the authors state, useful for validation of, e.g. NWP models, albeit only in the area under investigation. It should be stressed in more clarity that shown mean daily cycles for e.g. net radiation, surface temperatures and turbulent heat fluxes etc., and in particular their differences among the various surface types are specific for the region under consideration. Building characteristics (material, spacing etc.), latitude, upwind conditions (prevailing vegetation, presence of sea surfaces etc.) are all not generally transferable to any other city

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in the world. A possible weakness of the paper is the fact that no verification/validation whatsoever of the modeling set-up is presented. The authors claim that there is no experimental data for the Copenhagen Metropolitan area available (p.11185, l. 19). However, during COST-715 some data for the Copenhagen area was employed - what should be known at least by some of the authors. If indeed the data were not accessible, the least that should be done is to compare the results to some characteristics as available from the open literature (see specific comments for details). The 'interpretation' of the results (section 3, all subsections) to some extent suffers from these weaknesses (no generality and no validation) as mentioned above. All the numbers and details given are not really of interest to anyone except one who would want to verify a NWP model in exactly this area. Rather than presenting all these numbers (at what time, over which surface, which mean value is simulated for a given variable - e.g., p. 11192, l. 7 and then countless times after that), it would be valuable to 1) discuss the differences between the surface types and 2) try to find support (or the opposite) in the literature for the discovered features. Finally, the language needs some revision and should be checked by a native speaker.

Specific Comments: p.11184, l. 17 LES is not a simulation type in which no turbulence model is employed for parameterization. As the authors themselves state three lines below the small-scale turbulence is indeed parameterized. This erroneous statement must be corrected. p. 11185, l. 6 'Moreover, the fluxes': it should be stated which fluxes. Most likely the authors refer to turbulent fluxes (which are indeed rarely measured in operational networks (radiation fluxes, in contrast, usually are). p. 11186, l. 16 A reference for the Mosaic approach would probably be in order. p. 11187, l. 8ff This sentence doesn't make sense to me. p. 11189, l. 8 DMI-HIRLAM model system: horizontal resolution should be provided. p. 11190, l. 2 'Typical vertical profiles': based on what? Statistics? Monin-Obukhov Similarity Theory (MOST)? Other scaling regimes? If MOST then, at least some comments on its applicability in this urban environment and height range would be necessary. p.11192, l. 12 'were observed': simulated is probably more appropriate. p. 11194, l. 23 'For the simulated air tem-

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perature, water vapor, $\ddot{E}$.': Where are these variables evaluated? 1 level (which?), one layer (how defined)? Inspecting Fig. 5 would suggest the latter from the very small numbers (differences) in both variables between the various surface types. If it should be results from one level (most likely the lowest model level, then) one would ask whether a difference (between different surfaces) of 0.01g/kg water vapor is of any practical significance, given the measurement and modeling uncertainties. p. 11195, l. 17 ' $\ddot{E}$ are negligible': it might be relevant here that the turbulence exchange model that the authors employ in this study does take into account thermodynamic (urban) features, but not dynamic ones (except some roughness length changes). So, it does not really come as a surprise that simulated differences in wind speed are minimal. p. 11196, l. 2 ' $\ddot{E}$ nigh hours are characterized by negative heat fluxes $\ddot{E}$ '. This is an example of a variable for which observations exist (albeit from other cities). The vast majority of observations indicate that in urban areas, near the surface, turbulent heat flux remains positive throughout the day and almost for the entire year (e.g. Christen and Vogt, 2004). This is one of the examples where at least a comparison with results from other studies would be in order. p. 11198 Conclusions: the 'conclusions section' is a mere summary, again elaborating on the numbers, as if these would be of any generality (what they are not). The authors should rather try to put their results in perspective of other studies and try to work out conclusions for further studies. p. 11200, l. 9 ' $\ddot{E}$ over urbanized areas': again, this is not true in this generality, but rather for the Copenhagen metropolitan area. What is possibly usable for other areas is the sensitivity and characteristic differences. Fig. 1 caption: b) is 'dominating' (not dominated) type of surface (again in Table 1). Table 1: What is 'Max'? Maximum (I suppose) of what?

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