

Interactive comment on “Validation of the Martilli’s Urban Boundary Layer Scheme with measurements from two mid-latitude European cities” by R. Hamdi and G. Schayes

M. Rotach (Referee)

mathias.rotach@env.ethz.ch

Received and published: 5 July 2005

please note that the e-mail adress given above is outdated - mathias.rotach@meteoswiss.ch

General The paper presents a validation of the probably most advance urban turbulence transfer scheme, i.e. that of Martilli et al (2002). This validation is performed using a single-column model and is based on data from two recent urban boundary layer experiments in Basel (BUBBLE) and Marseilles. The authors have introduced a modification to the urban turbulence transfer scheme, namely they take into account

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

of moisture effects (which had been neglected in the original scheme). Unfortunately, the validation does not take this modification into account in any meaningful way (see major comments). Still, the paper offers a useful and important contribution to the advancement of our knowledge in urban turbulent exchange. Publication after major changes is therefore recommended.

Major comments 1) The authors claim that vegetation (and hence evapo-transpiration) is a major missing process in the surface exchange parameterization of Martilli et al. - and this is indeed true. In their validation using two urban datasets they fail to give the percentage vegetation cover at the two sites. Also, they compare 'urban' vs. 'classical' simulations, but do not assess the influence of their newly introduced vegetation contribution. Finally, in the case of the BUBBLE experiment there is one additional urban site with a much larger vegetation cover than at the chosen site U1 (approximately 31% as compared to the 16% at U1). It is therefore suggested to add another type of simulation ('noveg') in order to demonstrate the impact of including moisture processes. With this the question can be addressed, which of the processes (urban dynamical and thermo-dynamical processes vs. the additional evapo-transpiration) has the largest impact. This would especially make the transfer scheme more valuable to applications over suburban surfaces (e.g. US cities) where usually large fractions of vegetation can be found. 2) The authors describe their results but usually fail to make reference to other published work. In particular, an identical approach (one-column simulation of the U1 site in Basel) has been taken by Roulet et al. (2005) (and the authors cite the paper - but do not make any comparison to their results). Similarly, Lemonsu et al (2004) have simulated the site in Marseilles and again the paper is cited but not used for comparison. This has to be added.

Specific comments (technical) (counting each line including titles, equations etc) abstract, l. 1 The Martilli's s.e.: either use 'the parameterization of Martilli' or 'Martilli's parameterization'. Many occurrences throughout the text. Key words I don't think that Basel and Marseilles are really good keywords P3, l. 1 Field measurements (e.g. Ro-

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tachĚ.) P3, l. 2 Ěvertical structure of turbulence (not turbulent) fields P3, l. 6up three urban surfaces (or surface types) P4, l. 1up Ěin Rotach et al (no M.W.). Also, the paper is apparently not published in 2004 and therefore the citation should be 2005. P5, l. 10 ĚThe measurement set-up P6, l. 3 At both sites the highest instruments were mountedĚ. P6, l. 1up ..balance components for selected clear sky days. Also the following sentence (For U1Ě.) needs rephrasing P7, l. 2 The symbols (QH, Q* etc) need introduction. P7, l 12 Ěshortwave radiation (not radiations). P7, l. 7up Ěa different emissivity. It may be worthwhile to mention that Marseilles is considerably more to the south than Basel and has a much more Mediterranean climate. P8, l. 2 (end of section 2.3): The results should be put into perspective to the long-term climatology for urban surfaces. See for example the paper by Grimmond and Oke or, for site U1 in particular, the paper by Christen and Vogt (2004). P8, l. 11 force-restore, not force restore P8, l. 4up in proportion to, not: in proportionsĚ P9, l 3up at 30mĚ: In Table 1 it says 31.7m. P10, l. 11 For the 'classical' simulation, the authors should specify what changes specifically are made, i.e. for example what value for the roughness length is chosen (and why). P10, l. 3up Good correlation is foundĚ. P10, l, 10 This is the first evaluationĚ.: Roulet et al (2005) and Lemonsu et al (2004) have performed similar comparisons. This has to be acknowledged. P10, l. 13ff Ěand then averaged. In presenting the results, the authors should also find a way to show the run-to-run variability (e.g. in Fig. 5 and others to follow as Fig. 6, 7). P10, l. 3up Ěin good agreement with the measured 0.10Ě. P11, l.1up Ěin magnitude than the radiative loss. P14, l. 5 ..and Vogt (2004) estimate (not estimates). P15, l. 14 Ě..seems to underestimate the latent heat flux: The authors should comment on the fact, that also at U1 a similar underestimation is found. Fig. 6, caption It should be mentioned that storage is not observed (but residuum) - certainly at top of the tower. Similar in Fig. 10. Fig. 8 The vertical scale is quite excessive. For example a range 15 to 45 °C would be enough and would show the results much clearer.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4257, 2005.