**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

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

R. Hamdi and G. Schayes

Dear Mr. Rotach,

In the first place, we thank you for your careful and constructive review of our paper. And we apologize for the delay of the response (due to change in my professional position). We have tried to follow all of your requests. A point by point explanation follows below for your major and specific comments:

Major Comments:

* In their validation using two urban datasets they fail to give the percentage vegetation cover at the two sites.

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It is not true because the percentage vegetation cover is mentioned (16% for U1 and 14% for CAA) in Table 1 (line7).

* Also, they compare 'urban' vs. 'classical' simulations, but do not assess the influence of their newly introduced vegetation contribution.

The aim of this manuscript is to validate the urban module of Martilli against measurements of surface energy budget, surface temperature, and canyon air temperature from two urban sites with vegetation cover less than 20%. So, to assess the influence of both urban and the new vegetated part of the scheme, we must validate the module over a location with higher fraction of vegetation. In the appendix a new simulation is added where the urban scheme is validated, in the case of the BUBBLE experiment, for the urban site U2 with higher fraction of vegetation (31%). We add in the conclusion. "However, for the tow locations tested in this study the vegetation fraction in each grid cell is relatively low. Thus, what is really tested is the urban part of the scheme. In appendix A, ..."

* The authors describe their results but usually fail to make reference to other published work.

In the paper by Roulet et al. (2005), it is indeed true that the authors validated the urban module of Martilli in an identical approach over U1, but in that paper they do not analyze the surface radiation budget and the surface energy fluxes. However, we have added in the new version of the manuscript a comparison with the paper of Roulet for the validation of canyon air temperature.

In the paper by Lemonsu et al. (2004), the authors validate the Masson (2000) Town Energy Budget over the Marseilles city center from 18 June to 1 July 2001 and from 5 to 11 July 2001. A comparison between the two urban schemes is added:

1.In the end of section 5.2.2. "However, the comparison between the statistics performances of Martilli's and TEB schemes shows that the...". 2.In section 5.2.4. "The

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comparison with the statistics performances of TEB ..."

Specific comments:

* The Martilli's, either use 'the parameterization of Martilli' or 'Martilli's parameterization'. We use now Martilli's parameterization.

* Key words. I don't think that Basel and Marseilles are really good keywords. The key words are now: 1-D numerical simulation, street canyon meteorology, urban parameterization scheme of Martilli.

* Field measurements (e.g., Rotach) We agree. It is done in the new version.

* Vertical structure of turbulence (not turbulent) fields. We agree. It is done in the new version. * Three urban surfaces (or surface types). We write now 'three urban surfaces' in the new version.

* In Rotach et al. (no M.W.). Also the paper is apparently not published in 2004 and therefore the citation should be 2005. We agree. It is done in the new version.

* The measurement set-up. We agree. It is done in the new version.

* At both sites the highest instruments were mounted. We agree. It is done in the new version

* ... balance components for selected clear sky days. Also the following sentence (For U1) needs rephrasing. We write now, "...balance components for selected clear sky days: 17, 18, 23, 26, and 30 June 2002 for U1, and all days between 18 and 30 June 2001 for CAA."

* The symbols (Qh, Q* etc) need introduction. QH, QE, and QS are already introduced in section 2.2 (Instrumentation). And Q* is introduced in the sentence "... are useful parameters for the detection of diurnal trends in the partitioning of the net radiation Q* into QH, QS, and QE and..."

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* shortwave radiation (not radiations). We agree. It is done in the new version.

* a 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. We write now "We note that Marseilles is considerably more to the south than Basel, it has a much more Mediterranean climate, and it is characterized by the presence of strong winds."

* (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 bu Christen and Vogt (2004). We add in the end of section 2.3. "In fact, the Bowen ratio of urban surfaces may depend on various factors like climatic setting of the city, precipitation, phenology and the difference between rural and urban discharge coefficients. A long-term climatology for the urban site U1 can be found in Christen and Vogt (2004)."

* force-restore, not force restore. We agree. It is done in the new version.

* in proportion to, not in proportions to. We agree. It is done in the new version.

* up at 30 m: in Table 1 it says 31.7 m. We agree. It is done in the new version.

* 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). A new table 3 is added in which we summarize the input parameters used for the classical simulation and for the vegetated fraction of the urban grid cell.

* Good correlation is found. We agree. It is done in the new version.

* This is the first evaluation: Roulet et al. (2005) and Lemonsu et al (2004) have performed similar comparisons. This has to be acknowledged. Sentence 'This is the first evaluation of the urban module of Martilli where modeled surface fluxes can be compared with observations ' is removed from the text.

* are 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). We think that the run-to-run variability can be obtained from the statistics presented in Table 5, 6, 7.

* in good agreement with the measured 0.10. We agree. It is done in the new version.

* in magnitude than the radiative loss. We agree. It is done in the new version.

* Christen and Vogts estimate (not estimates) We agree. It is done in the new version.

* seems to underestimate the latent heat flux: The authors should comment on the fact, that also at U1 a similar underestimation is found. We write now: "Just as it is found in the urban site U1, the urban simulation seems to underestimate the latent heat flux..."

* Fig. 6 caption it should be mentioned that storage is not observed (but residuum)-certainly at top of the tower. Similar in Fig. 10. We add in the figure caption of Fig. 6, 10 the sentence "The black asterisks are the observed fluxes at the top of the tower (the heat storage flux QS was determined as the residual term)".

* 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. We agree. It is done in the new version.

Sincerely yours,

R. Hamdi and G. Schayes.

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

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