Author's response to review of Referee #2, posted on 7 May 2013 on "The diurnal evolution of the urban heat island of Paris: a model-based case study during Summer 2006"

H. Wouters, K. De Ridder, M. Demuzere, D. Lauwaet, N.P.M. van Lipzig

The authors would like to thank the anonymous referee for the time devoted to review the manuscript and for his/her useful and constructive comments. All comments by the referee were carefully addressed and the manuscript has substantially benefited from the proposed changes. We would like to clarify our changes below.

Note: our changes in the manuscript are highlighted with the yellow marker.

> ***Comment a):*** *why the ground heat flux (G) is not even mentioned in the subsection of the surface energy budget. The authors should clarify why they do this. In the nighttime this flux can be important. The surface scheme that they describe computes G but here nothing is said. This should be put in perspective and justified. Can you neglect G in the city center or do you put it inside the anthropogenic flux? If so, why? C13955*

The storage heat flux 'DQs' represents the total heat transfer to/from the urban surface that includes buildings (roofs and walls) and roads. This is equivalent to the ground heat flux represented by 'G', so this is not neglected. To clarify this, we have added this information to the manuscript (r. 367-370).

> ***Comment b)*** *Comparison of computed and observed profiles at Trappes (rural?) seems to indicate that the model tends to produce too stable layers in some of the nocturnal profiles. A bias around 1 K is mentioned in the text for the urban areas and 1.5 for the rural ones. Default is attributed to deficiencies in the turbulence scheme but it could be also that other regional structures, such as low-level circulations induced by the topography or the urban heat island itself, generate mixing by shear and that these are not well captured by the model. Almost no vertical information on the wind is given, so nothing can be concluded on this issue: were there low-level jets observed? and modelled? if so, could they be relevant?*

It is true that the ARPS model indicate too stable layers in some of the nocturnal profiles. It is also found that the ARPS model shows an overall too large low-level jet which reach an overestimation of a factor two in some of the profiles. Indeed, this may indeed be the consequence of an underestimated vertical turbulent mixing generated by low-level circulation or the urban heat island. We have added these comments in the model evaluation, see r. 307 – 312. As this remark is extremely interesting, we have also devoted new seperate subsections 3.4.4 and 3.4.5, which now explain the impact of the nocturnal stability and the nocturnal low-level jet on the UHI build-up with the idealized advection model (r. 599 – 607). We have also summarized these findings in the last paragraph of the conclusions section (r. 798 – 800; 805 - 809), and the end of the abstract (r. 28 – 30).

> ***Comment c)*** *In section 3.4.1. the nocturnal radiation cooling is discarded readily just*

mentioning a reference. This important decision should be more sustained since, for very stable nights with weak turbulence, radiation may be even dominant. If you decide to neglect it, it must be because turbulence is large enough, logical over the city, but less clear for the rural scenario case. You take a wind of 3.5 m/s for the advected air, but this can easily lead to very low values of wind in the surface layer and decouple it from above, making radiation relevant there.

It is true that low 10m wind speeds down to 2 metres per second were found in the ARPS results and observations, so that radiative cooling can become important when weak turbulence is found. The effect of radiative cooling on the UHI build-up is now discussed extensively in a seperate Section 3.4.6 (see r. 668 – 725) . Hereby, we have calculated explicitely the radiative cooling to ground and space following Pielke (2002) Eq. 8.41. As a conclusion, the reduction of the radiative cooling to the surface due to the mixed-layer above the city compared to the NBL over cropland may enhance the UHI buildup as well, but only for a few metres above the surface. This finding is added to the conlusion section (r. 801 – 804).

As Section 3.4 has become quite large, the different boundary-layer processes and features are now seperated in subsections of section 3.4. We also have broadened the scope of the paper in which we are investigating the impact of boundary-layer features on the representation of UHI in atmospheric models:
- abstract: see r. 28 – 32
- introduction: see r. 85 - 94, 122 - 125
- conclusions: see r. 796 - 811

Comment d): *is the wind from the east a synoptic feature? One would say that, for weak synoptic pressure gradients, the wind would blow from the west, downslope. Then there would be no uplift due to topography. How would that change the picture?*

Yes, the wind direction is a synoptic feature. It is true that the uplift depends on the wind speed and direction and that this uplift depends on synoptic situations. Indeed, if the wind would blow downslope from west, it would go just parallel to the surface (so no relative upward motion). We have added these comments as an introduction (r. 538 – 544) for studying the effect of vertical motion on the UHI build-up which is now presented as a seperate subsection 3.4.3 (r. 535 - 598). Herein, it is found that, when discarding the uplift, the adiabatic cooling for both the base and scen case is eliminated. Because this adiabatic cooling was larger in the scen case, the overall UHI-buildup is reduced. The vertical extent of the UHI is affected as well.