Dear,

We would to thank the editor, Dr. Michel Van Roozendael, for taking care for the review process. We also want to thank the two referees for their positive response and their time spent on reviewing the manuscript. We are convinced that our manuscript has substantially benefited from their interesting remarks and proposed changes. The answers to the reviewers' comments can be found below. The revised manuscript is attached alongside this document. We are looking forward to your response.

Kind regards,

On behalf of all (co-)authors
H. Wouters, K. De Ridder, M. Demuzere, D. Lauwaet, N.P.M. van Lipzig

Attached: revised manuscript (acpuhiparis_revised20130617.pdf)

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.

Author's response to review of Referee #3, posted on 13 March 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

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

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.

> **Comment on P25943, L2:** *"characteristics" (instead "characterstics").*

We have corrected this in the final manuscript, see r. 44

> **Comment on P25943, L4:** *"...favoured by high solar irradiation..." not clear (during the previous daytime period)*

We have made this sentence more clear in the final manuscript, see r. 48:
"It is especially favoured by high solar irradiation (clear-sky) during the **preceding** daytime period, no precipitation, low wind speeds and stable stratification."

> ***Comment on P25943, L9-L28:*** *This part of the introduction does not add much. Authors should instead focus on the state of the art on the understanding of the processes (which is the subject of this article).*

We have shortened this part of the introduction. We now keep focus on the relevance of understanding the UHI processes, see r. 54 – 62

> **Comment on P25944, L16:** *"known" instead "know"* → the corresponding sentence was reformulated (r. 85 – 89)
> **P25944, L18:** *"...mixed-layer height ~~during the day~~ is at least ..."* → corrected at r. 79 – 82
> **P25944, L19:** *...than ~~the~~ during the day and ..." → corrected at r. 80*
> **P25944, L16:** *"...Therefore, an ~~an~~ idealized advection model..."* → corrected at r. 193 – 195
> **P25945, L1:** *...little is known about orographic..."* → the corresponding sentence was reformulated (r. 85 – 89)
> **P25945, L10:** *"... for an in-depth analysis of the evolution..." → corrected at r. 121*

We have corrected these typing errors in the final manuscript.

Comment on P25945, L19: *References must be in alphabetical order or in the order of year of publication (check all)*

We have checked that the references are in alphabetical order

Comment on P25945, L23: *"...using Ridder's (1979)...": reference is missing*

This reference is now added to the final manuscript, see r. 963 – 965

Comment on P25946, L3: *"... Rodell et al. (see 2004) ..."*

This refence is corrected in the final manuscript, see r. 150

P25947, L1-L2: *"... with values reaching..." : this has been already specified earlier in the text*

We have left out this sentence from the final manuscript, see r. 185

Comment on P25947, L16: *"... at horizontal resolution of 16, 4, 1km."* → corrected at r. 204
P25948, L8: *"...(Jain, 1989) are applied subsequently..."* → corrected at r. 234
P25949, Eq(2): *Mistake in the sign of equation* → corrected at pp. 4
P25949, L18: *"...can be integrated from Eq. (2):"* → corrected at pp. 4
P25951, L25: *".. have a daily mean of ..."* → corrected at r. 322
P25951, L26 : *"... or 19% which starts to increase too early..."* → corrected at r. 324 – 325
We have corrected these typing errors in the final manuscript.

P25953, 1st / 2nd paragraphs: *These paragraphs are not clear. For instance, "The reduction in latent heat is the most important urban heating mechanism ..." is a bit confusing.*

We have carefully revised these 2 paragraphs. Hereby, we now clearly identify and compare the source terms for urban heating by looking at the differences in the surface energy balance between the two scenarios, see r. 353 – 383

P25956, L6-L9: *It would be more appropriate to remove this sentence. It is repeated in the conclusion of the section and in the conclusion of the paper.*

We have removed this sentence. As the introduction paragraph in section 3.4 is very short, it is now removed. Instead, we have clearly defined the goal of section 3.4 at the end of the introduction, see r. 119 - 124

P25956, L22: *How the fit has been done? (It does not seem to be the best for the run SCEN?)*

We have improved the fits by using linear regression on the lowest 150 metres in both scenarios.

Therefore, the stability of the linear profile in the scen case was increased from 5K/100m to 5.8 K/100m. The temperature at the inversion height was calculated from the averaged temperature between 150 and 400m above ground level. This improvement slightly affected the results for the idealized model for the adiabatic cooling. As a result, the decreased adiabatic cooling over the city compared to cropland is 1.5K (25%) instead of 1.3K (compare r. 581 in the new manuscript with r. 15 on page 25958 in the old manuscript.) We have now described the method of the fits at r. 488 – 496.

P25958, L12 *"decreases" instead "increases" ??*

We have replaced the sentence in the new manuscript (r. 573):

Original sentence: ***"****If only the surface sensible heat flux is considered (wh = 0), the relatively low difference between the base case and scenario case in surface heating between 22:00 UTC and 23:00 UTC increases the UHI to 4.8 K"*

New sentence: *"Excluding the upward motion, the differential surface heating between the base case and scen case between 22:00 UTC and 23:00 UTC results in an UHI buildup from 2.7K to 4.6K at the surface (compare 'base NLFT' with 'scen NLFT' in Fig. 12 (b)). This UHI buildup is smaller than for the simulations including the vertical motion (6.1K)."*

P25958, L17: *You could also mention that the vertical extent of the UHI is different.*

Yes, it is true that the vertical extension is affected. We have added this interesting finding to our results in Section 3.4.3 (see r. 587 – 588) and to the conclusions (see r. 795 - 796)

P25959, L9
"... is not sophisticated, the UHI..." → corrected at r. 735
P25966
Ref "Makar et al. 2006" is missing → added at r. 947 – 949

Figures General comment: *the captions are sometimes very long and should be simplified.*

We have revised the captions of Figs. 1, 2, 3, 6, 7, 9 and 10

Fig.2 and Fig.3: *These diagrams are not very clear. You should specify which curve is for θ(t) and which one is for θ(t+dt) and clarify the values on x-axis an y-axis. The dotted line for the θ(t) vertical profile is not clear and confusing with the dotted lines used to show dh.*

We have improved these two figures and captions according to your recommendations. We have indicated on the figures which lines correspond to θ(t) and θ(t+dt).

Comment on Fig.9: *You should add the star that indicates the Paris city centre (like for Fig.10).*

We have added the star at each panel of Fig. 9