Author's Response

Review: *Huszar et al., Regional climate model assessment of the urban land-surface forcing over central Europe*

Responses to K. Trusilova's Comments

- *Comment: for the analysis of the statistical significance of the effects from the urban parameterization to the temperature, precipitation and wind you use the significance test T-test. However, the applicability of this test poses one very important criterium to the analysed data: the data MUST BE NORMALLY distributed. For the temperature data this might be fulfilled in some cases, but not for the precipitation and not for the wind. The distribution of precipitation usually has a shape of a Gamma-distribution and the distribution of the wind has the shape of a Weibull-distribution. This means the t-test IS NOT APPLICABLE to the data. That is why, for example, we see such little signal of the precipitation changes in the Figure 8.*

  It is true that the assumptions of the t-test, in their strictest form, do require normality (or, more accurately, normality of sample means), and the statistical distributions of the variables investigated are not precisely Gaussian, certainly not for precipitation or wind speed. In our test setup, however, we did not work with these variables directly, but rather applied a paired setup, thus analyzing the differences of matching values in the signals compared. As a result, the signal subjected to the significance testing was substantially closer to being Gaussian than the original series themselves, even for a variable as asymmetrical in its distribution as precipitation. Admittedly, there were still assumption violations even in the paired test setup (especially in case of precipitation, mainly due to high number of zero-zero pairs), and significance tests for precipitation and wind speed were therefore also carried out using a sign test, the results of which are now used in the manuscript. Nevertheless, it can be mentioned that differences between the spatial patterns of statistical significance obtained by t-test and by sign test were generally quite small (likely due to asymptotic behavior of the t-test for large samples, such as ours). In the revised manuscript, the use of paired t-test setup is now explicitly mentioned in the methods description; sign test is now used for assessment of statistical significance in case of precipitation and wind speed.

- *Comment: please discuss how your findings differ to those of Trusilova et al (2008).*

  Thank You for pointing our attention to this study, which we missed during the revision of past studies dealing with the topic. This is a very relevant paper similar to ours, which is now cited in the revised manuscript. Furthermore, we compared our results for temperature and precipitation to their results. It is found that our model reduces the diurnal temperature range due to urban coverage less than in Trusilova et al. (2008) but the summer precipitation reduction is bit higher (20–30%) than their results suggest.

- *Question: how many wall/roof/road layers were used for the simulations? how thick were these layers? How the inner building temperature is treated in the simulations? This infomation may be useful for explaining the daytime urban cooling if, for example, the building walls are set too thik and the model does not "warm" it enough quickly during the day producing the phase-offset in the diurnal temperature curve.*

  In the revised manuscript, we included the number of roof/wall/road levels and their thickness: roof – each 5 cm thick, wall –each 5 cm thick, road – 5 cm, 25 cm, 50 cm and 75 cm thick. The inner building temperature was set to a constant value of 298 K. We think, that the modelled phase-offset of the diurnal temperature curve is caused by the reduced heating within urban canyons due to enhanced shadowing during morning hours, however when the sun is high enough (i.e. the solar angle is small) this shadowing becomes also small, the intensive warming of urban surfaces (road and wall) starts and the UHI begins to develop.

Responses to Referee #1's Comments

- *Comment: My major concern with this study is the physical explanation of the results. I do not understand why the results relating to temperature and turbulence (temperature and mixing-layer height) show a wide-spread increase all over Europe while the results related to the water budget (evaporation, absolute humidity and precipitation) only show a localized decrease over the cities. This needs a proper explanation in the manuscript.*

The wide-spread change of a given variable due to urbanization is, in general, related to the surface sub-grid treatment in our model. The surface model runs at a 2 km x 2 km resolution while the "dynamical" model resolution is 10 km x 10 km. The impact on 10 x 10 km resolution, which is shown throughout the manuscript, can be thus viewed as the superposition of the impacts over 2 km x 2 km sub-grid boxes. In nearly each grid box there is at least one urbanized 2 km x 2 km sub-grid box (except over the sea in the northwestern corner of the domain and in south next to Italy). The simulated temperature and turbulence related changes and those of humidity are directly connected to the surface characteristics and thus their perturbation due to urbanization is higher where the model grid is composed of more urban sub-grids. This is seen e.g. over large cities. So we cannot agree that in case of humidity, the decrease is localized. It is only magnified by denser urban sub-grid coverage.

Where we found only localized effects is the precipitation. However, it is subject by high spatial and temporal variability, especially during summer when precipitation over central Europe is mainly of convective nature. Therefore the impact is seen and found significant only over the most affected areas, i.e. over gridboxes with high percentage of urban coverage corresponding to large cities. However, setting lower treshold for the significance (e.g. 90%), impact (mainly decrease) is detected over rural areas as well.

- *Comment: A related point is the urban cool island which is mentioned in the introduction. It would have been interesting to see whether the model is able to reproduce this feature or if this is lost in the overall warming produced by the model. The only hint is given in Fig. 12 which shows this effect in the measured data but it is not reproduced in the model. Fig. 12 has to be discussed in this respect.*

  Indeed, the Urban Cool Island effect is reproduced in our simulations but is rather weak. Although it is hard to recognize from Figure 11, during morning hours from 9 am to 12 am the curve representing the SLUCM simulation for the city center is slightly colder than the "vicinity" curve. Quantitatively, it is colder by -0.05 to -0.1 K. For Prague, the UCI is about -0.05 K on average. This is much less than the measured -0.2 to -0.3 K. The explanation can be in inappropriate urban parameters set for Prague. The domain wide parameters are representative for a typical central European city while the very center of Prague is characterized by more narrow streets than this domain average. In conclusion, having wider street canyons, the morning cooling caused by building shadowing is not strong, bringing less pronounced UCI.

  To gain spatial information about the UCI, we evaluated the urban impact for summer 8–11 am (see attached figure). This figure clearly reveals the UCI effect. The modelled urban impact is negative in many cases, or at least the overall warming is suppressed or is not statistically significant, which is the results of the competing effect of the UCI and the overall domain-wide warming.

  The UCI related results are now commented upon in more detail in the revised manuscript and the aforementioned figure is added in the manuscript as well (Fig. 14 in the revised manuscript).

- *Comment: The results for wind are mainly inconclusive. Most parts of the results did not pass the statistical significance test. What remains is an increase in summer nights and a decrease in winter nights over the cities. Once again, no convincing physical explanation is given. Local secondary flow circulations driven by urban heat islands (this is what the authors assume that it could be the reason) are usually small-scale and are most likely not resolved on a 10 by 10 km grid. The winter-time nocturnal decrease is not interpreted at all. A possible explanation could be quite different and is once again related to the simulated temperature response. A night-time cooling in winter leads to a stronger thermal stability of the PBL and thus to reduced 10 m winds. Likewise a night-time warming in summer leads to a less stable PBL and thus to an increase of 10 m winds.*

  By replacing the criticized t-test in case of wind speed and precipitation with the sing-test, the significance of the results for wind slightly increased, but the overall picture did not change. We extended the explanation of the modelled wind speed changes. In both winter and summer daytime a slight decrease over cities is modelled. Similar decreases were found by Klaić et al. (2002) or Hou et al. (2013) and we attributed it – in line with previous studies – to increased roughness of the surface. Such urbanization related wind stilling was documented even on global scale by Vautard et al. (2010) analyzing the Northern Hemispheric observed winds. Another factor influencing the urbanization impact on wind is the destabilization of urban boundary layer (UBL) due to higher urban temperatures (seen also in connection with the PBL increase) which on the other hand leads to enhanced winds. These competing effects may result in slight wind speed decrease over cities and a small wind speed increase around these areas during daytime conditions.

The increase in wind speed during night-time in summer can be related to decreased nocturnal stability due to the presence of UHI. Another contributing factor is related to the urban-breeze circulation, when, in the presence of thermally induced horizontal pressure gradient, convergent motions towards the city form (Hidalgo et al., 2010), but we agree, that this local scale circulation pattern probably cannot be resolved with our resolution of 10 km x 10 km; most of the modelling studies dealing with this phenomenon used much finer resolution. E.g. the mentioned study calculated on a 500 m x 500 m resolution grid.

Enhanced roughness in urban environment may play a governing factor in the winter nightime decrease of winds, resulting also in lower PBL height modelled. A different summer nocturnal behavior is simulated for northern Italy (urban areas within the Po valley). Here, even in summer night time, wind speed decrease is modelled, again, probably governed by increased surface drag. A similar result was found for another southern city, Lisbon (Portugal) recently by Lopes et al. (2012).

The above mentioned explanatory paragraphs were added to the discussion in the revised manuscript.

- *Comment: The only vertical profiles from the simulation are shown in Fig. 9. They show the destabilization of the atmosphere during day and night in summer. This should lead to more thermal convection and more convective precipitation. It would be interesting to learn whether there is a shift in the model from large-scale precipitation to more convective precipitation in summer. In this context the abilities and effectiveness of the convection parameterization in the RegCM4.2 model have to be discussed.*

  Indeed, we found in our simulations a widespread warming in the PBL as well as a specific humidity decrease (which we do not present in the manuscript). This results in higher lapse rates, destabilization of the atmosphere leading to more thermal convection. By looking at the precipitation changes decomposed to large scale and convection portion, we see no significant change in the first one, i.e. the simulated total precipitation changes presented in the manuscript correspond mostly to convective precipitation decrease. We use the Grell convective scheme (Grell, 1993) with the Fritsch and Chappell closure (Fritsch and Chappell, 1980). In this approach, precipitation rate is proportional to the updraft mass flux which is further proportional to the buoyant energy available for convection (ABE). With higher lapse rate the ABE increases but lower humidity in the PBL is counteracting. In our simulations, this latter effect dominates. In summary, reduced evaporation and consequently reduced humidity in the PBL provides less available moisture to convection which in turn drives the simulated precipitation changes.

Responses to Referee #2's Comments

- *1. The major concern about the manuscript has been already raised in the interactive comment submitted by Dr. Trusilova. In line with Dr. Trusilova's comment, I would suggest that the authors re-conduct the statistical analysis of their results, since the majority of the examined variables cannot be assumed to follow a normal distribution, which should be the case when using the t-test. Non-parametric tests should be used instead.*

  (Answer also given to K.Trusilova's SC) It is true that the assumptions of the t-test, in their strictest form, do require normality (or, more accurately, normality of sample means), and the statistical distributions of the variables investigated are not precisely Gaussian, certainly not for precipitation or wind speed. In our test setup, however, we did not work with these variables directly, but rather applied a paired setup, thus analyzing the differences of matching values in the signals compared. As a result, the signal subjected to the significance testing was substantially closer to being Gaussian than the original series themselves, even for a variable as asymmetrical in its distribution as precipitation. Admittedly, there were still assumption violations even in the paired test setup (especially in case of precipitation, mainly due to high number of zero-zero pairs), and significance tests for precipitation and wind speed were therefore also carried out using a sign test, the results of which are now used in the manuscript. Nevertheless, it can be mentioned that differences between the spatial patterns of statistical significance obtained by t-test and by sign test were generally quite small (likely due to asymptotic behavior of the t-test for large samples, such as ours).

  In the revised manuscript, the use of paired t-test setup is now explicitly mentioned in the methods description; sign test is now used for assessment of statistical significance in case of precipitation and wind speed.

- *The number of figures in the manuscript should be significantly reduced. To my view, the current large number of figures does not help in highlighting the value of the study. Reducing the figures could certainly improve the quality of the manuscript. Reduction of figures could be achieved by means of removing certain variables from the analysis. For instance, the cross section diagrams could be removed, as well as one of evaporation/specific humidity. Selecting between monthly/hourly profiles for the examined variables could also help in reducing the figures and making the manuscript's message more clear to the interested reader. According to my opinion, hourly profiles are of more usefulness than monthly profiles, considering that the heat island effect is examined. In summary, the authors should re-visit their results and decide what is most important to show.*

  In line with the referee's proposal, we removed the evaporation figure (Fig.6 in the original manuscript). This variable is directly connected to the type of the landuse, so its decrease is a direct consequence of the urban treatment and its analysis brings relatively little new information. Furthermore, as significant impact on precipitation is modelled only for summer, we removed the winter precipitation change subfigures and did similarly for the specific humidity, and we merged these two fields into one figure.

  We decided to keep the vertical cross-section figure for temperature as it gives interesting information not only on the horizontal distribution of temperature response and helps interpreting the results.

  We removed the monthly plots and instead, we use a table where only the summer means our presented, when the UHI is important.

- *P18543, 2nd paragraph: I would suggest that the authors slightly extend the description of the heat island generation. Since their study is based upon this particular urban effect, a more clear and concise description is necessary. For instance, several aspects of heat island generation are not referenced, including evapotranspiration and anthropogenic heat.*

  The heat island generation description has been extended in the revised manuscript to focus on more aspects of UHI (like evaporation, anthropogenic heat etc.).

- *Section 2.1: I would suggest that this particular section of the manuscript be revised. The presentation of the implemented model is somehow hard to follow, since several aspects of the adopted configuration are mixed up with information about changes in the model characteristics between different versions. I would propose to the authors to re-write this section, allowing for a more concise presentation of the implemented model and the adopted configuration (e.g. short description of the model, adopted configuration for the various physics processes, focus on the parameterization of landsurface processes).*

  In the revised manuscript, the description of the model without experiment specific information is now included in Section 2.1, while Section 2.2 gives – again, without experiment related configuration – a description of the urban canopy model used. Section 2.3 then contains all the configurations for the performed experiments.

- *P18548, L20-23: The authors state that the CORINE land cover database was used for deriving land use information, summarized in the definition of two urban land use categories (i.e. urban, sub-urban). However, the procedure for deriving the land use information from CORINE is not described. For instance, the CORINE database includes several urban land use categories, such as continuous/discontinuous urban fabric, port areas, airports, and green urban areas, just to mention a few. What was the procedure followed for grouping and remapping the different CORINE urban land use categories into the two (urban, sub-urban) categories used by RegCM?*

  As mentioned in the manuscript on P18548, line 20-23, two databases were used to create the land use map of the model domain – Corine2006 and Global Land Cover 2000 (http:// bioval.jrc.ec.europa.eu/products/glc2000/glc2000.php) for areas where Corine2006 was not available. From the Corine2006 dataset we used only the Urban fabric category (Level 2 cat. 1.1, included in Artificial surfaces group). The land use category "urban" in the manuscript is a compilation of Corine2006 category 1.1.1 – Continuous urban fabric and category 22 – Artificial surface and associated areas from the GLC2000. The "sub-urban" category is identical to Corine2006 category 1.1.2 – Discontinuous urban fabric. The other Corine2006 categories from the group of Artificial surfaces (i.e. Level 2 categories - Industrial, commercial and transport units; Mine, dump and construction sites; Artificial non-agricultural vegetated areas) were not taken into account.

The following two sentences were added to the manuscript: “Urban category is a compilation of ’Continuous urban fabric’ land use type (cat. 1.1.1 from Corine2006) and ’Artificial surface and associated areas’ (cat. 22 from GLC2000). Sub- urban category is identical to ’Discontinuous urban fabric’ land use type (cat. 1.1.2 from Corine2006).”

- *Specific minor comments*

- *The authors should try to reduce the use of the first plural ("we used", "we changed", etc) within the entire manuscript, since, to my view, it lowers the presentation quality of their manuscript.*

  The first plural sentence has been – where possible – removed in the revision.

- *P18543, L6-8: ".., which is a well-known phenomenon". Which is the "well-known phenomenon" referenced herein? This part of the sentence needs to be rephrased to be more clear and concise.*

  We meant UHI here. The sentence was rephrased to be more specific.

- *P18546, L6-11: The part of the paragraph describing the changes from verions 3 to version 4 of RegCM does not really add much to the manuscript and could be removed.*

  This part was removed.

- *P18546, L20: “An improvement of the BATS scheme: : :”. Improvement relevant to what? Needs rephrasing.*

  We meant improvement in describing the surface fluxes against the case, when the sub-grid division of the grid box is not applied. The sentence was reformulated and extended accordingly.

- *P18548, last paragraph: The comparison between the 10km and the 2km grid does not really add much to the manuscript. Instead, I would suggest using Fig.2 for presenting the overview of the used modeling domain, along with the urban coverage (Fig. 2, left) as resolved in the model simulations.*

  We removed both figures and replaced them by one which shows the model orography at the “dynamical” 10 km x 10 km resolution and the urban coverage at the subgrid scale 2 km x 2 km resolution.

- *Figures 10-11-12: Adding error bars for the computed profiles would be helpful, allowing for a better interpretation of the data.*

  We added error-bars for the diurnal figures (the monthly figures where replaced by a table).

- *P18558, L20-24: It is stated that in winter, the impact of AHR is greater during the day. I would expect that AHR have a greater impact during the night, when temperatures are lower and central heating is extensively used.*

  The hourly profiles of AHR in our simulations assume that significantly larger heat is released from vehicular traffic during the day than during night and that AHR from indoor heating peaks during morning hours as the heat from nocturnal heating is released with a lag due to thermal conduction trough the roof and wall. As a result, daytime AHR is slightly larger than during the night.

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