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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "The regional impact of urban emissions on climate over central Europe: present and future emission perspective" *by* Peter Huszár et al.

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Author's response on Anonymous Referee 3 comments:

We would like to thank to Anonymous Referee 3 all the comments, suggestions and corrections in his review of our manuscript. We addressed all and our point-by-point responses including the modifications in the manuscript follow:

*Referee's Comment: Page 5, Sections 2.2: As far as I understand when the authors refer to the experiments for the future period 2046-2055 practically they refer to experiments forced by the ERA-interim meteorology of the decade 2001-2010 with*



*chemical ICBC of the decade 2001-2010 but with anthropogenic emissions of 2050. I think although the authors mention this, it is still somehow misleading the notation for a future simulation over the period 2046-2055. Maybe the authors could make this point even more clear within the manuscript.*

Author's response: We agree that this point is not made enough clear in the manuscript and the reader can be easily mistaken to think that meteorological changes are considered in the future as well (apart from the emission changes of short lived gases/aerosols). So we stressed it in the abstract as well as in the part of the manuscript with the description of the experimental set-up

*Referee's Comment: Page 6, Section 2.4: I think that the authors should provide more details for the individual ensemble members.*

Author's response: We achieved the perturbed runs by changing the RegCM integration step. The base (default) ensemble member was integrated at dt = 30s timestep from the very beginning trough the whole simulation. The further two ensemble members were created by running the model with 1) dt = 15s and 2) dt = 10s timestep for the first few days, and than turning to default 30s timestep for the rest of the simulation. With this we achieved a physically consistent but numerically perturbed ensemble.

We made this clear in the revised manuscript.

*Referee's Comment: Page 7, Section 3.1: The authors show differences in Figures 3 and 4 between E-obs gridded data with a resolution of roughly 25 km and RegCM data with 10 km resolution. This can be done either with an upscale interpolation from RegCM towards E-OBS or with a downscale interpolation from E-OBS to RegCM. What was the interpolation procedure that the authors followed? Furthermore the authors should provide information for the used E-OBS data (e.g. version, resolution, reference).*

Author's response: The E-OBS (version 12.0) gridded observational data was used in the validation (described by Besselaar et al. 2011). Data is available on a 0.25 and 0.5 degree regular lat-lon grid (roughly 15 km x 20 km at the modeled latitudes), which were interpolated to the model grid (10 km x 10 km).

We included in the revised manuscript a detailed description of the observational dataset used and the interpolation process performed.

*Referee's Comment: Page 7, Section 3.2: Overall it seems from Figure 7 that the urban emissions lead to decrease of ozone over an extended area in Germany as well as at sub-urban and rural areas around the big cities due to NO titration even though it is the summer period. Taking into consideration that there is only slight ozone increase (of up to 0.5 ppbv) for the rest of the domain it could possibly postulated that there is an average ozone decrease for the whole domain due to urban emissions which is somehow not expected for summertime. Maybe it would be insightful if the authors try also to use only the daytime O3 data in order to reduce the effect of nighttime ozone removal (due to NO titration) and discuss this issue. Furthermore, is this slight ozone increase statistically significant?*

Author's response: We found that the main driver for ozone decrease is the first order removal by NO, i.e. NO-titration. In Huszar et al.(2016) we made a sensitivity simulation were NOx emissions were reduced by 20%. This resulted in a strong ozone increase in urban areas, were strong ozone titration occurred. As a domain average, ozone slightly decrease over the first two model layers (i.e. up to about 150 m), however, over higher levels, the domain averaged ozone change is positive. We checked the impact on day-time and night-time average ozone in JJA and found that the daytime changes are although similar in pattern to the "all-day" average, the ozone production far from urban centres is however somewhat stronger (up to 1 ppbv). Regarding the significance, performing t-test showed that the changes are basically significant everywhere (98% level of significance), except those areas where the ozone impact is nearly zero – i.e. the transitional areas between areas where ozone is removed and those with ozone production (see the attached Fig. 1 below).

In the revised manuscript, we added some notes considering this issue, however we do not want to add more figures as the chemical behavior due to urban emissions was already discussed in Huszar et al.(2016) and this paper is intended to focus on the



meteorological/climate effects.

*Referee's Comment: Page 7, Section 3.2: The authors state that the saturated NOx conditions cause ozone titration for the lower model levels. Do the authors mean "with NOx saturated conditions" that ozone production is in the VOC-limited regime or simply refer to the first order ozone removal by NO titration? This point needs clarification. Normally with NOx titration we mean the process of O3 removal through direction reaction with NO which takes place during nighttime and in the vicinity of large NO emission sources. The saturated NOX conditions (or VOC sensitive conditions) is a different issue. The split between NOx-saturated or Nox-sensitive regimes is driven by the chemistry of odd hydrogen radicals with HNO3 being the dominant sink in the first case and peroxides the dominant sink in the second case. Maybe the authors could also refer to the photochemical regimes in their simulations for winter and summer using VOC/NOx or H2O2/NOy ratios (see also the study of Beekmann and Vautard, ACP, 2010).*

Author's response: We agree that the term saturated NOx conditions along with titration is not correct, as here we really meant first order ozone removal by NO. Indeed, in Huszar et al. (2015) we showed that simultaneous reduction of NO+NO2 emissions leads to ozone increase over urban areas due to less available NO entering the O3+NO reaction. Only further from cities was found the NOx emission perturbation without any effect, which indicates NOx-saturated conditions. But again, in the manuscript, we meant NO titration as the main cause for ozone decrease.

We also agree that it would be interesting to see the split between NOx-saturated or NOx-sensitive regimes by evaluating the suggested ratios, however this would go beyond the scope of the paper so we decided to only make a clarification of the ozone changes from the point of view of interactions with NOx emissions.

*Referee's Comment: Page 12, Section 4: It is stated that the model results encounter large wet biases over mountainous areas. This is connected to the convective scheme as high-resolution simulations with the default Grell-FC scheme tend to significantly over estimate precipitation for the mountainous areas as pointed in previous studies*

*with RegCM (Torma et al. 2011; Zanis et al., 2015).*
Author's response: We agree that the use of Grell scheme with the Kain-Fritch closure could be another source of precipitation overestimation over mountainous areas.
We included this into the revised manuscript and the suggested references as well.
*Referee's Comment: Page 12, Section 4: The authors mention that in winter weather is characterized with more stable conditions and reduced variability. In what sense? Do they mean in terms of static stability? Otherwise this statement is wrong as in mid-latitudes, winter is characterized with higher synoptic variability and stronger baroclinic instability.*
Author's response: Here, we meant reduced instability or in other words, increased stability in winter. this greatly influences the vertical mixing and thus the vertical extent of the urban impact.
We made this point clear in the revised manuscript.
*Referee's Comment: Page 12, Section 4: The authors claim that the maximum cooling is shifted toward later hours due to delayed propagation of aerosol signal through the boundary layer. Is this a speculation or it can be justified from the results of this work or from results of previously published work?*
Author's response: This was based on a rather speculation, however we reconsidered the causes for this shift and found that the main reason for this is probably the shifted maximum aerosols concentration towards later hours in the afternoon in DJF compared to JJA.
To support this, we added a figure (Fig. 17) (and an accompanying discussion) to the manuscript's Discussion section showing the diurnal cycle of the vertical distribution of the urban impact on aerosols.

References

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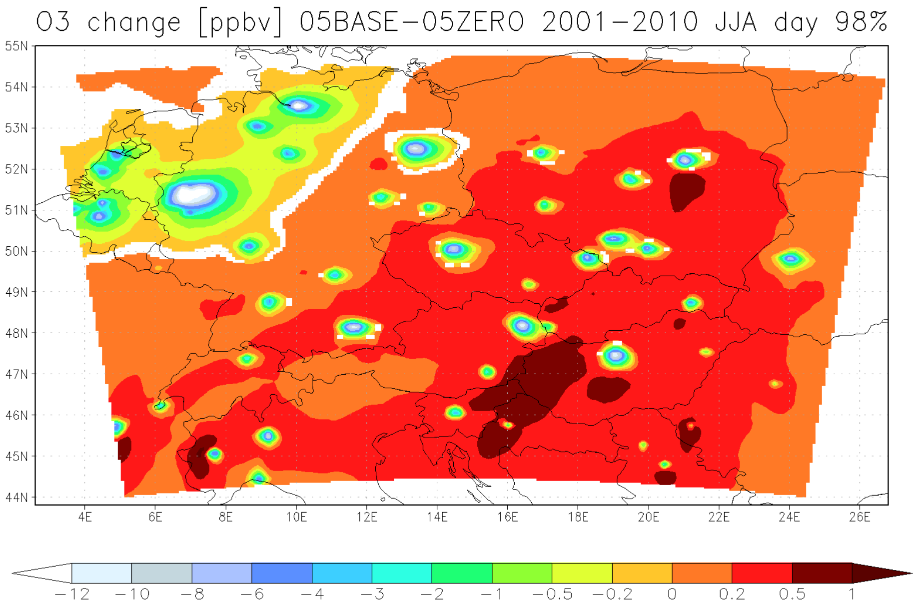


**Fig. 1.** The urban emission induced day-time ozone changes. Shaded areas mean singificant changes on the 98\% level using t-test.