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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 1 comments:

We would like to thank to Anonymous Referee 1 for his comments, suggestions and corrections. We addressed all and our point-by-point responses including the modifications in the manuscript follow:

Referee's Comment: 1. P.5, For the future emission used in the study, it is said "A moderate climate policy is assumed on p.5 (line27), how would the findings change if mitigation strategies in moderate climate policy fails, or not achieving the emission *target? Could the authors provide some context about this? Especially, authors remarked in the conclusion (p.13 beginning from line 10), that the radiative impacts of urban emission presented here are at lower limit. It would be essential and interesting to compare the radiative impacts of chemical perturbation under a less-successful mitigation climate policy and to the one presented here for moderate climate policy.*

Author's response: The intention of the study was to 1) evaluate the urban emission's impact for present day emissions and 2) the same impact in future assuming a future scenario of emissions. As the future emissions are lower (except certain activity sectors) in general in this scenario, the impact itself was (as expected) quantified to be also lower a bit. One can conclude that without achieving the emission reduction goals for the middle of the century (for which we had the emission scenario), which would mean that emissions would stay at the present level, the radiative impact of urban emission would not change.

We added a small paragraph to the description of the emissions/scenarios to clarify this point.

Referee's Comment: P.7, session 3.1 model validation, suggest to write a brief summary about the validation of chemical species here to facilitate the readers' understanding.

Author's response: We agree that a brief summary of the most important points regarding the chemical validation (presented in Huszar et al. 2016) is necessary to include in order to facilitate the reader's understanding of the model's overall performance.

We therefor added a paragraph to address the most important findings during the chemical validation.

Referee's Comment: P.7 and P.10, model biases in surface temperature and precipitation would greatly affect the aerosol loadings within the boundary layer through, for example, wet scavenging and turbulent eddy transport of aerosols. Moreover, the vertical distribution of the aerosol species and ozone would also be affected by surface temperature and precipitation. Although the authors give reasons for the model biases

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(P.10-P.11 session 4), the author should further elaborate and provide support about the findings in this study is robust or at least not significantly affected by such model biases. Noting that authors already partly addressed (P.13 line 11-12) that the radiative impacts will be under-predicted, it would be good to give some quantitative measure of the underprediction or some sensitivity test results to strengthen the conclusion.

Author's response: We completely agree that the manuscript misses a discussion of how the model biases affect the results. In this context the precipitation bias is the most striking and therefore we made two additional sensitivity runs for a shorter, 2001-2005 period: one experiment for the base (05BASE) case and one for the zero (05ZERO) case. In these runs the precipitation fields from RegCM were reduced by 30

We added a paragraph in the Conclusion section to address this issue. We also included a figure (Fig.18) as an example for the increase of aerosol concentration (for nitrates – for other aerosols the response is similar, and for ozone it is almost negligible).

Referee's Comment: P.11 line 30-31, it is about the long-wave heating rate and temperature of air and the surface. It would be more convincing to provide numerical results and/or figures about the vertical heating rate profiles obtained from different simulations. Especially because the model results are known to have negative surface temperature bias.

Author's response: The corresponding paragraph tries to explain the reader that the main driver for the simulated radiative/temperature changes is the aerosol enhancement, i.e. less shortwave radiation reaching the surface. However, one must consider also the impact of the ozone changes which encompass both reduction near the surface and large sources, and production further from them and above higher elevations. Here, we refer to previous studies that concluded that ozone LW cooling rates in the lower troposphere (where ozone changes are significant in our study) are very small due to similar temperatures with the surface (Petty, 2006; Liou, 2002). This means that any perturbation of ozone concentrations near the surface have a small

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longwave radiative impact - unlike for aerosols, that in our study interact only with short wave radiation (in the used version of RegCM4 only large dust particles interact with LW radiation).

We agree that it would be conclusive to evaluate the short-wave heating and long-wave cooling rates for aerosols and ozone separately, but this is impossible within the modeling framework at this stage (i.e. one cannot output individual rates for each species), so we would like to rely purely on the cited literature about ozone heating/cooling rates and that of aerosols.

Referee's Comment: P.12 line 31, suggest to provide results of atmospheric stability obtained from different simulations to further support the argument "delayed propagation of aerosol signal..."

Author's response: We re-evaluated the causes of the delayed surface response of the winter urban aerosol enhancement by analyzing the diurnal cycle of the aerosol vertical distribution. We found that there is a shift of maximum aerosol load towards later hours (due to the emission distribution during the day). Because of the shift of the maximum values there is a slight shift also in the radiative impacts.

This has been clarified in the manuscript and a figure has been added for DJF and JJA showing the diurnal cycle of the domain averaged vertical profile of urban induced aerosol load (those aerosol types that we considered in the radiative calculation)

Referee's Comment: 6. P.13 line 3, please clarify how to reach the number "10% of the total cooling" or relate to other session in the paper. It is not clear to readers how author reach this "10%".

Author's response: In the revised manuscript, we made this clear: the cooling by urban aerosol (up to about 0.05K) represents approximately 10% of the cooling caused by all (i.e. not only urban induced) aerosol (up to about 0.5 K).

Referee's Comment: P.13 line 22, "...climate impact of urban emissions is very small..." but P.13 line 10,"...lower limit", then it is not legitimate to say "climate impact of urban emission is very small". Please rephrase appropriately.

Author's response: We agree that such a formulation is not legitimate, as in theory, if it is only a lower limit, the actual effect could be higher, becoming no longer "small". We reformulated the whole paragraph to stress that even without these shortcomings, the urban impact on radiation/climate would not change (increase) significantly as 1)the most important urban aerosols were considered, showing that the disregarded SOA comprises about 10% of the aerosols considered in radiation calculation 2) the effect of fixed boundary conditions is significant in the outer part of the domain only and becomes minimal in the inner part 3) the sensitivity runs showed that without the modeled precipitation bias there would be even larger temperature decrease, but this change is of one order less than the overall cooling. We made these points clear in the Discussion part of the revised manuscript.

Technical corrections

Author's response: We implemented in the revised manuscript all the technical corrections suggested by the referee.

References:

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