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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of urban land-surface on extreme air pollution over central Europe" *by* Peter Huszar et al.

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Authors response on: "The impact of urban land-surface on extreme air pollution over central Europe" (acp-2020-399)

By Peter Huszar et al., 2020

Dear Referee #3,

Thank you for your detailed review and for sharing your comments with us! We will address all of them and our one-by-one responses follow below including the modifications made in the revised manuscript.

General comment

I feel a bit more discussion of the role of chemistry in controlling the NOx and O3 (and PM2.5) concentrations would be appropriate. Two different chemistry and aerosol schemes are used; a discussion of how they differ, and showing results that clearly illustrate if they give similar or different results would be interesting. Also, ozone formation is affected by temperature, solar radiation (cloud cover); how do the meteorology changes caused by the urban canopy affect the chemistry?

Authors response: Our paper is based on previous research made to investigate the role the urban canopy plays in controlling the city scale meteorological conditions and consequently the chemistry and transport of pollutants. The mutual links between meteorology and gas-phase chemistry (NOx-ozone) in urban areas is detailed in Huszar et al.(2018a), whereas in Huszar et al.(2018b) we extended this analysis to primary and secondary aerosols. Finally in Huszar et al.(2020), after identifying that vertical eddy transport plays the most important role, we focused on this aspect of the urban canopy meteorological forcing on chemistry. All three studies give robust results on which fractional processes play a role in the modelled differences in concentrations after introducing urban landsurfaces, e.g. in case of NOx, the most important effect is the eddy removal from lower model layer while for ozone, increased urban temperatures play role too by enhancing dry deposition and the NO+O3 reaction. Furthermore, these results are in line with previous similar studies for both European and other urban areas (these studies are referred to in the Introduction and also in the Discussion). Nevertheless, to make the paper more self-explanatory, we included in the Discussion section a few notes on which processes in urban areas control the NOx-O3 and aerosol chemistry and transport (based on the findings in our previous papers.) Regarding the inter-model differences in chemistry and aerosol modules, we admit that our discussion lacks to give more detail on this issue, however our study did not intend to present itself as a model comparison study and here more models are used to solely increase the robustness of the results. This is also true for showing results from different resolutions. Our results from individual models and grid resolutions are qualitatively same and very close to each other quantitatively proving the urban canopy meteorological

effects on chemistry manifest themselves in a similar way in different models and they are not an artificial feature of a selected model or resolution. We could certainly add many comments on the reasons for some of the modeled differences between models but this would drift the focus of the paper too much from what it intends to present.

A section on the observations used to evaluate the model is needed. What is the accuracy of the observations? Where are they located?

Authors' response: We provided a new section within the Model validation subsection which describes all the measured data in detail that are used in the model validation, including their resolution and the data source.

A description of how PM2.5 was determined from the model results is needed.

Authors' response: We included a sentence how PM2.5 is obtained from model output (in WRF-Chem these are directly available, whereas in CAMx they have to be calculated as a sum of all primary and secondary aerosol).

The line plots showing all the numerous model results are very difficult to read. It would be helpful to have separate plots to illustrate specific differences, such as 1 set of plots to show the difference in resolution for one model, and another set of plots to show multiple models at 1 resolution. Or find some other way to illustrate those model differences (e.g., biases, bar charts of mean bias, correlations, etc.).

Authors' response: As our study is primarily intended to show an intermodel comparison at different resolutions, we limited on Figure 2. (Fig. 3 in the revised manuscript) the presentation to the innermost 1 km domain only, except for the 2 yr WRF-Chem run made only at 9 km (blue line). Now the figure's message is more clear. Presenting results for the 1 km only is justified also by the fact that the differences between different resolutions with the same model are very small. We also increased the font in every figure to make them easier to read (a suggestion from the other reviewer).

I found the Discussion section a bit difficult to read. It would be helpful if the figures

more clearly illustrated the points discussed in this section and were referred to at appropriate points. It would also be helpful to have subsections in the Discussion, perhaps separating the findings related to the urban vs no-urban simulations, differences due to model resolution, differences due to chemistry, for example.

Authors' response: We divided the Discussion and conclusion section into three parts, the first one discussing the model validation results, the second and third then discussing the impact on meteorological conditions and average/extreme air pollution. We also added references to figures and tables, results from which are discussed.

Minor/Technical comments

l. 16 (and elsewhere): 5% percentiles is usually written 5th percentile.

Authors' response: corrected through the entire manuscript.

l. 124-127: define TUV and MEGAN acronyms

Authors' response: defined.

l. 187: "Chemical boundary conditions for the outer domains were taken from the CAM-chem data (Lamarque et al., 2012)." Be more specific about where the boundary conditions come from. I do not know of any archived results from the Lamarque et al. 2012 paper. If they are from the results provided by NCAR they should be referenced as described on: https://wiki.ucar.edu/display/camchem/CESM2.1%3ACAMchem+as+Boundary+Conditions If you ran your own simulations, the details of that should be given.

Authors' response: We used the results provided by NCAR, so we changed the references to those offered by the UCAR wiki page (Emmons et al.2020 and Buchholtz et al.2019).

l.204: perhaps more details of how MEGAN was run could be included - which vegetation map, which meteorology data, or is MEGAN online in the model?

Authors' response: We included a description of the megan input data (plant functional types, leaf area index data, emission factor maps). The meteorology used to run MEGAN is taken from the RegCM and WRF runs except the WRF-Chem experiments where biogenic emissions are computed online. This is now clarified in the text.

l. 487: "large taen" -> larger than? Authors' response: corrected.

l. 562-4: I don't follow this statement. Should "decreasing" be "increasing"?

Authors' response: Decrease is correct as we refer to the separate impact of urban landsurface only (as in the entire manuscript), Of course, taking emission into account would completely change the narrative due to the large positive impact of urban emissions. But this is not the focus of this paper and this is clearly stated in the Introduction.

Additional proof-reading is needed. There are a number of grammar errors and typos.

Authors' response: We corrected some typos found in the manuscript. Further proofreading will be conducted by the Copernicus publishing office if the paper is accepted for publication.

References:

Huszar, P., Karlicky, J., Belda, M., Halenka, T. and Pisoft, P.: The impact of urban canopy meteorological forcing on summer photochemistry, Atmos. Environ., 176, 209-228, https://doi.org/10.1016/j.atmosenv.2017.12.037, 2018a.

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