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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.

Anonymous Referee #3

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The submitted manuscript provides an assessment of regional impact of urban emissions on climate over central Europe for the present and future climate based on the regional climate model RegCM4.2 coupled to the chemistry transport model CAMx. I would suggest acceptance of the manuscript for publication after taking into a number of comments that follow.

Comments 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 simu-

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lation over the period 2046-2055. Maybe the authors could make this point even more clear within the manuscript. Page 6, Section 2.4: I think that the authors should provide more details for the individual ensemble members. 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). 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? 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

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and Vautard, ACP, 2010). 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). 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. 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?

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