Author's response on Interactive comment on “On the long term impact of emissions from central European cities on regional air-quality”
by P. Huszar et al.

The authors would like to thank for the detailed review of the manuscript including valuable comments and corrections. Our responses follow one-by-one:

Reviwer #1

Reviewer's comment:
My impression is that is too much work for one paper. I am convinced about findings concerning goal 2 (the sensitivities part), but several questions remain on goal 1 (the evaluation part, which I would primarily see as an evaluation of the modeling system per se, followed by the estimation of the contribution of the impact of emissions on air quality).
Ideally, I would first expect to see a pure evaluation paper, investigating thoroughly the basic weaknesses of the modelling system (e.g. poor temporal correlation and large biases) by looking into emissions and the way they are disaggregated, the impact of Boundary Conditions and the biases of key-meteorological variables on chemistry. If this work, which is forced by reanalysis meteorological fields produces average (occasionally below average) metrics, it cannot be expected to perform better, when coupled to a regional climate model to study for example the impact of future emissions/climate on air quality.

The importance of this works lies in using an on-line modeling system as a tool for the investigation of the impact of emissions controls on air quality. The online regional meteorology-chemistry models are an emerging community, which establishes fast in the field of air quality modeling studies. Before replacing the widely used offline meteorology-air quality models, it is important to evaluate the performance of the new-generation online models, and gain some valuable insight into the nature of thecomplex and not so well understood interactions of meteorology and chemistry.

This work, includes a considerable amount of work on the impact of emissions on central European air quality and the some impact metrics (e.g. AOT40), however I missed some information on the added value of this work, compared to similar previous literature using offline models. What is the incentive to use an online model in this specific study instead of an offline? What will be the added value for having a computational expensive system to investigate emission/air quality issues? And more importantly: Is this model capable to reproduce accurately the state of the atmosphere? Or we simply use a more sophisticated but less understood modeling system, undermining the quality of final results? Can you compare the evaluation performance of this modeling system with the offline system and support the use of the online?

Authors' response:
The reviewed manuscript presents results within a wider scientific scope: to quantify and qualify the present and future impact of urban emissions on both air-quality (tropospheric chemistry in general) and climate (see the acknowledgement for the related project). We chosen an integrated assessment developing and applying an online coupled modeling system. This is a step forward, as using an offline coupled system could produce valuable information on the impact of emissions on atmospheric chemistry, however, the climate feedbacks trough radiation (e.g. on temperature) would remain unknown. This work thus involved large number of online simulations were the climate and chemistry were calculated simultaneously.
Regarding the presentation of all the concerning work, we have chosen the following approach. 1) first

we intended to present a paper that evaluates the “chemical” part of the results, leading to this paper. 2) the results concerning the climate impact of the chemical perturbations due to city emissions (ozone and aerosols) are planned for a follow-up paper currently being in preparation.

In accordance with the above, we decided to include the description of the modelling system in the first paper (this one), and, as this manuscript focuses on 'chemical' results, we also included the validation of the air-quality related output with surface measurements; unlike the meteorological model outputs, which will undergo evaluation in the follow-up paper mentioned before. We, however, admit that a better concept would have been to organize this two papers in a companion paper (Part II) with this manuscript as Part I.

The description of the modeling system and the valiadation is done in light of a the performance of previous version of the coupled system RegCM3CAMx, which was detailed and thoroughly validated in our earlier study, Huszar et al. (2012). Many of the model discrepancies, especially regarding ozone, are found to be similar as in this earlier study, however, some improvements are also identified that can be attributed to better (1hour) time resolution of data exchange between the participating models (RegCM + CAMx). One major deficiency of this earlier work was the relatively large ozone bias which we tried to eliminate by using a larger domain for developing more realistic chemical boundary conditions for the inner (10 km x 10 km) domain. However, we admit that even choosing so, many of the ozone biases remained (although a bit smaller) and in future, time and space varying chemical boundary conditions have to be definitely used.

In Huszar et al. (2012), considering online coupled ozone and aerosols led to 1) a slight (but statistically significant) improvement in model performance regarding temperature 2) or to not significant improvement at all. We have earlier applied the model RegCM in many other studies at a same or similar set-up as here. Although these studies did not include online coupled chemistry and two-way interactions (as in this study), we can make an assumption that the climate impact of perturbed air-chemistry (mainly on radiation temperature) is small changing the RegCM's performace only a little (as seen in Huszar et al. 2012).
So, answering the reviewer's questions:

What is the incentive to use an online model in this specific study instead of an offline?
The goal which gave us an incentive to use an online coupled model was to quantify not only the chemical impact of urban emissions but also the simultaneous radiative feedbacks triggered by these chemical perturbations. This goal requires an integrated online coupled climate-chemistry model approach, chosen in this study.

What will be the added value for having a computational expensive system to investigate emission/air quality issues? And more importantly: Is this model capable to reproduce accurately the state of the atmosphere?

The added value is the possibility to calculate radiative feedbacks and impacts on temperature (and climate in general). We also assume, based on previous validation studies involving RegCM and CAMx that the capability of these models reproducing the state of the atmosphere (both meteorology and climate) is will not change significantly if coupling them online w.r.t case when they are coupled offline.

Or we simply use a more sophisticated but less understood modeling system, undermining the quality of final results?

Choosing an online coupled system was a requirement rather than an option, given what was mentioned above.

Can you compare the evaluation performance of this modeling system with the offline system and support the use of the online?

The performance of was compared to the previous version of the RegCM-CAMx coupled system published in Huszar et al. (2012) and a slight improvement is noted, which is attributed the improvements made in the upgraded version of RegCMCAMx couple used in this study. We can further assume, that the calculated species concentrations in an offline couple would be very similar (at least in a climate sense) as, between an online and offline realization of the same run, only the meteorological driving conditions are different, but these are considered small in a 10 year average.

Technical comments:
Reviewer's comment:Introduction is rather lengthy, could be shortened to reflect a literature overview on issues relevant to European emission/air quality.
Authors' response: We shortened the introduction where it was possible (e.g. in the listing of studies from different cities).

Reviewer's comment: Sections 4.1.1 to 4.1.4: These sections are very short and there is no attempt to explain the current findings. On the other hand, Section 5 "Discussion and conclusions" is a very lengthy one. My suggestion is to move parts of the S5 discussion into S4.1.1-S4.1.4 and try to provide some connections, between the findings that can be related (e.g. O3 biases with NOx biases).
Authors' response: The reason, sections 4.1.1 to 4.1.4. (the validation) does not contain any explanation of the current findings is, that these sections are only presenting the results and all the related discussion/explanation is included in the Discussion and Conclusion section, which also connect findings concerning NOx and ozone biases. To make a paper more readable, we split the last section into pure Discussion (that contains all the explanation of the results and discussion of the findings) and the main conclusions are included into the Conclusion section.

Reviewer's comment: My major concern is the poor O3 correlation. Eventually the invariant boundaries could be the problem? Authors can refer to literature and cite previous work reporting improvement of temporal correlations with the use of space/time variant BCs, and also explain, to which extent they believe that improving the BCs in their mother domain, could improve results in the nested central European domain. I think it should also be a priority for the modeling group to update the BCs in future versions.
Authors' response: Indeed, we believe the time/space invariant BC conditions for large mother domain are the major source for ozone biases and low correlation. Recently, Akritidis et al. (2013) were investigating the impact of BC on the simulated ozone concentrations using the same two models as in our study (an offline couple of models RegCM and CAMx). They found a clear improvement in the correlation coefficient when using global chemistry model (ECHAM5/MOZART) based BC (which are thus time and space variant). Their correlation of monthly ozone values using time/space invariant BCs is 0.74 that compares very well to our value (0.77). Introducing the MOZART BCs, the correlations increased by often more than 0.1.
As a conclusion, we will focus on improving the BCs (taken from global model) in future simulations with the presented couple. We also included the comparison with this study in the Discussion section to provide additional explanation for ozone biases.

Reviewer's comment: Also in the evaluation plan of the modeling system I think authors could include

some key meteorological parameters like temperature, radiation which strongly affect chemistry and emissions. May be it will be too much too add for the current manuscript that is already very lengthy, but I think is an important action, which need to be considered as a future action.
Authors' response: As mentioned above, we would like to separate the presentation of chemistry (air-quality) and meteorology related results of the conducted experiments. We rely on previous validation experiments done by RegCM which showed that this model is capable of reproducing the mean state and variability of the atmosphere. However, the follow-up manuscript being in preparation will definitely contain a validation of the meteorological parameters, especially those related to radiation (radiative fluxes, temperature etc.).

Reviewer's comment: Discussion and conclusions could be separated. Conclusions could be a short paragraph summarizing the most basic findings of this work and eventually future steps to improve the methodology followed.
Authors' response: We separated the last section into a Discussion section which now only discusses and explains the results (also in the light of previous studies). The Conclusion section then contains the main conclusions of the study with future steps to improve the methodology used.

Reviewer's corrections were included in the revised manuscript.

Changes to the original manuscript: apart from minor corrections made following the reviewers comments, the most important changes include 1) splitting the last section into two separated: Discussion, and, Conclusions. 2) additional explanation of the biases encountered in case of ozone, and its connection to NO2 biases, included in the Discussion part. 3) enlarging fonts in the figures were it was necessary.

References:
Akritidis D., P. Zanis, E. Katragkou, M. Schultz, I. Tegoulias, A. Poupkou, K. Markakis I. Pytharoulis, Th. Karacostas, Evaluating the impact of chemical boundary conditions on near surface ozone in regional climate–air quality simulations over Europe, Atmospheric Research, 134, 116–130, 2013.

Huszar, P., Miksovsky, J., Pisoft, P., Belda, M. and Halenka, T.: Interactive coupling of a regional climate model and a chemistry transport model: Evaluation and preliminary results on ozone and aerosol feedback, Clim. Res., 51:59-88, 2012. doi: 10.3354/cr01054